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# Science in Isolation: American Marine Geophysics Research, 1950–1968

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The emergence of plate tectonics theory during the 1960s stemmed from large-scale efforts to investigate the sea floor and to interpret results in terms of a horizontally mobile crust. Many scientists, particularly those within the Soviet bloc, refused to accept the new ideas about the earth. Most authors fault the conservative Soviet scientific leadership for halting the progress of geophysics in the East. By contrast, this article examines facets of the Western scientific community that begin to explain both the exclusion of Soviet scientists from participating in the "plate tectonics revolution" and also the meteoric success of the theory in the West.

*Key words:* Earth science; plate tectonics; marine geophysics; oceanography; Cold War science.

## Introduction

Science during the Cold War never truly acquired an all-inclusive, international flavor. Just as perceptions of German, Jewish, and Marxist science fragmented the scientific community in the 1920s and 1930s, the era after the Second World War found the content of science itself dividing peoples into distinct international communities along lines traditionally defined by politics and ideology.[1] One divisive forum was the new theory of plate tectonics, introduced in 1968 as the crowning synthesis of decades of research in the marine sciences.[2] Plate tectonics reined in the disparate concepts of fracture zones, oceanic trenches, sea-floor spreading, mid-ocean ridges and rifts, transform faults, and magnetic anomalies, and it placed them into a global theory at variance with the most common perceptions among geologists twenty years earlier. The crust of the old earth was solid, and its only motions were the settling of continents perpetually struggling for gravitational equilibrium. The new theory painted a picture of a dynamic earth whose crust consisted of vast blocks constantly moving against each other, driven by convection currents in the mantle and governed by the geometric laws of spherical rotation. Scientists, particularly marine geophysicists, in several countries shared responsibility for the successful maturation of plate tectonics theory. Investigations funded by

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countries on both sides of the Iron Curtain, including some cooperative efforts such as the International Geophysical Year and the Upper Mantle Project, allowed literally dozens of scientists in the 1950s and 1960s to publish work that was fresh and unexpected about the sea floor. Americans in particular appreciated the quantitative character of plate tectonics and its power to resolve so many arguments about the dynamics of the earth, which no previous theory of horizontal crust mobility had done.[3] Yet enthusiasm for the theory was not universal, especially among scientists in the Eastern bloc.

Led by Vladimir Beloussov, an editor of the journal *Tectonophysics*, Soviet geophysicists rejected the idea that the crust of the earth could move horizontally over large distances. Instead, they preferred to believe that all motion was vertical, as huge deeply-penetrating blocks of crust struggled with each other to find gravitational equilibrium. These Soviet scientists strongly opposed plate tectonics. Western authors, seeking reasons for this intransigence, looked for intellectual roadblocks inherent in being Soviet. Some claimed that because the Soviets lived on a stable land mass, it was only natural for them to disbelieve the crust could move horizontally.[4] Others put the onus of the "Russian rift" on a conservative scientific community in which radical ideas were unwelcome.[5] One American marine geologist who corresponded with Soviet colleagues throughout the 1950s and 1960s, Henry William Menard, blamed Beloussov for imposing his brand of vertical tectonics on the entire Soviet scientific community, retarding tectonics in Russia by a decade or more and making virtually all work there simply "unfortunate."[6] Others interpreted the controversy as a counterpart of the old philosophical debate about what kinds of criteria are required to dissolve an old theory and embrace a new one.[7] In that case, the Soviets simply must have had a more intensive—or simply different—set of criteria.[8] One sociologist, after weighing the social and intellectual resources statistically in scientific publications, asserted that because scientific knowledge is a cultural product, it was perhaps safe only to say that many of those most opposed to plate tectonics were scientists who had the largest publication "investments" in other theories, making them less inclined to support something radical.[9] Soviet scholar Nikolai Krementsov has argued that Soviet science relied upon authoritative spokesmen in whom whole disciplines invested great power to direct scientific development.[10] Perhaps Beloussov was such a spokesman for geophysics. All of these authors imply that in some form or another institutional conservatism was at work in the Soviet scientific community, with only a few renegades supporting the idea of mobile continents.[11]

Though these explanations do begin to account for the "Russian rift" in marine geophysics, the actions of Westerners (particularly Americans) also contributed to the exclusion of Soviet scientists from the development of plate tectonics. Soviet scientists, faced with an Anglo-American scientific community that was neither open to all nor free from military control, were never fully integrated into the international scientific community. Security measures among American oceanographers, meant to keep Soviet scientists from research results, confined the intellectual discussion of important research results to an exclusively Western international scientific community. Even when more inclusive international collaboration was in vogue in the 1950s, logistical and technical constraints prevented Soviets from full cooperation. Meanwhile scientists in the West increasingly published results that interpreted research in terms of a horizontally mobile crust. In the late 1960s, several scientists began to call it a "revolution" in earth science. They called plate tectonics the new paradigm and demanded a concomitant research program worthy of it. When the Soviets resisted this new paradigm, after never participating in the development of the new ideas, many Westerners decided that Soviet knowledge of the earth, since it was formulated outside the framework of plate tectonics—and was outside the exclusive Western scientific community—was peripheral to science.

**The Anglo-American Invisible College**

Success in oceanography in the late 1940s and early 1950s was about access, and the controlling factor was security clearance. Americans did not share their research results with the Soviets, preferring to develop an international coalition of science that included the British but excluded scientists inside of the Soviet bloc. Some argued that access to military data or funding was not necessary to do good science, which could be accomplished even by those operating on tight budgets. Ernest Rutherford, the legendary director of Cambridge's Cavendish Laboratory, supposedly "did more and better nuclear physics with string and wax than most American laboratories did with their cyclotrons and electrostatic machines."[12] Although this may be true, oceanography, like geology but *unlike* experimental physics, was primarily a descriptive activity in which it was literally impossible to do good science without significant funding to make major expeditions. The U.S. Navy, the single greatest contributor to basic research in the years following World War II, jealously guarded its science.

In the late 1940s and early 1950s, the U.S. Navy dominated scientific research. During the war, the U.S. Army's management of the Manhattan Project gave it a virtual scientific monopoly from which the Navy benefited little. After the war, Admiral Harold Bowen siphoned off scientific manpower, mostly nuclear physicists, from the Army by convincing Congress to establish and fund the Office of Naval Research (ONR).[13] When Bowen retired, the scientists who remained at ONR began to pour funds into any research that might be related in some way to potential military applications, including earth science. Even after the creation of the civilian-controlled National Science Foundation in 1950, the scientific mobilization brought on by the Korean war solidified scientists' dependence on government funding, which often entailed classified military research.[14] ONR's criteria for usefulness were not strict, and it supported a wide range of research.[15] The first ONR contract went to Maurice Ewing, the director of the Lamont Geological Observatory at Columbia University, for seismic research on SOFAR, a method of transmitting sound waves thousands of miles through a layer of water insulated by temperature changes above and pressure changes below.[16] Additional funds flowed through the Navy's Hydrographic Office. When Navy and civilian scientists surveyed the mid-Pacific ocean floor with echo sounding equipment in 1950, they discovered that the sea floor was riddled with seamounts that could disrupt long-range sonar effectiveness. One of the priorities of several early expeditions in the 1950s, sponsored by both the Navy and the Scripps Institution of Oceanography in La Jolla, California, was to provide maps of the ocean floor for military use.

Scientists (figure 1) could only publish research in a major international journal, such as *Nature*, *Science*, or the *Bulletin of the Geological Society of America*, after approval by their military superiors. If it came at all, approval came slowly. H. W. Menard, a marine geologist at the Naval Electronics Laboratory (NEL) in San Diego, guessed that a few insiders had approximately five years to digest and interpret classified research results before it became public knowledge.[17] Menard made the best bathymetric maps in the world, using data gathered on expeditions in the Pacific. Yet the audience for his maps was limited; he shared them only with those scientists with classified clearance.[18] When he left NEL to join Scripps in 1956, he engaged in non-military research with the benefit of continued classified clearance. Lamont scientists such as Maurice Ewing and Bruce Heezen held the same *de facto* proprietorship over maps made of the Atlantic sea floor, in addition to the seismic data they held. In the absence of swift publication, small scientific

**Fig. 1.** Roger Revelle and Robert Dietz discussing dredged materials from the Pacific Ocean. Credit: Scripps Institution of Oceanography, courtesy American Institute of Physics Emilio Segrè Visual Archives.

communities materialized around institutions, providing informal peer review, often in hallways or even at cocktail parties.[19] Under the direction of the bullish Texan Ewing, Lamont churned out scientific papers *as* an institution, emphasizing the internal review process instead of publishers' referee processes. Seismologist Frank Press recalled that their "papers were written sentence by sentence together."[20] Ewing was known to prevent the publication of Lamont scientists' papers if he did not personally approve them; he simply would call up and pressure the editor of *Science* to stall or reject a publication.[21]

These small communities, bound by the need to keep Soviet scientists away from research results, melded into a single community of Western marine geophysicists. Robert Sinclair Dietz, Menard's supervisor at NEL, made it his business to keep tabs on outside scientific communities. After a year as a Fulbright scholar in Japan, he took a post as ONR's Science Liaison Officer in London. There he watched over the activities of European scientists, notably Soviets, by attending conferences and trying to assess their degree of sophistication in oceanographic science and technology. He sent his reports to officials in the military, government, and scholars in American universities.[22] By doing so, he helped to draw the boundaries of a research community that excluded scientists from the Eastern bloc. Menard likened it to an "invisible college," defined by British and American research interests.[23] It bridged not only American institutions such as Scripps and Lamont, but also the Department of Geodesy and Geophysics at England's Cambridge University. Its director, Edward Bullard, had established intimate contacts with Americans in the late 1940s. He resided at Scripps for a short time and helped to develop a heat-flow gauge for use on the 1950 Mid-Pacific expedition.[24] When he took the directorship he encouraged prominent scientists from Scripps and Lamont to come to Cambridge and help direct relevant research. The Canadian J. Tuzo Wilson, who later conceived the germ of his idea of transform faults while at Cambridge, called it a "splendid place to work." Bullard successfully created a magnet for researchers: an isolated working atmosphere in which all were aware of each others' work, providing an ideal climate for testing ideas.[25] Cambridge became one of several centers of gravity for the British-American "invisible college," attracting researchers from a variety of institutions (figure 2).

While the international community awaited publication, major discoveries shaped the outlook of Western scientists. On expeditions in the early 1950s, Menard discovered massive changes in elevation, or escarpments, extending hundreds of miles along the ocean floor. These fracture zones, as he called them, appeared at several points off the coast of California and ran westward along the Pacific ocean basin. This discovery posed startling questions: was the sea floor divided into provinces? If so, did the composition of the crust vary according to the province? The same expeditions brought back information on heat flow and magnetism. Scientists could not account for the great amount of heat released by the ocean floor, if they explained it by conduction from the earth's extremely hot core. The new data suggested that convection currents, proposed as the mechanism for continental drift by Arthur Holmes in 1932, might actually exist in the earth's mantle. Towing a magnetometer on these expeditions revealed that the magnetic anomalies in the rocks on the ocean floor were arranged by similarly-magnetized

**Fig. 2.** Maurice Ewing and Frank Press walking out of the mansion that served as the Lamont Geological Observatory. Credit: Photo by Warman, Columbia University, courtesy American Institute of Physics Emilio Segrè Visual Archives.

stripes many miles long. Since such stripes did not exist on the continents, this challenged the belief that the ocean floor and the continents were inherently similar. All of these findings were new and unexpected, challenging many basic assumptions about the earth.[26]

Security restrictions prevented most of these results from being published immediately, but not all of them. The Navy did not fund the magnetic research on these early expeditions, so the scientists could publish these results if they wished. Yet they did not bother to do so for many years, since within the closed community the results became common knowledge.[27] When they were published in a journal in complete form in 1961, they appeared in the same issue as an astounding interpretation of them by Victor Vacquier of Scripps, who had enjoyed access to them for years.[28] He indicated that the stripes were offset, often by many miles, by Menard's fracture zones. Vacquier showed that since a stripe pattern might break up and then resume hundreds of miles away, the earth's crust must somehow be mobile.[29] Only an insider such as Vacquier could have hoped to publish his groundbreaking article on the magnetic evidence for the crust's mobility before the magnetic survey itself was published. Scientists outside this community read such results in international journals without having been part of the scientific process of interpretation and debate.

Despite their status as insiders in the "invisible college," Western scientists who pursued non-military research using classified data also suffered from the Navy's security measures. Though scientists with security clearance could access classified research from other institutions, they typically had to arrive in person to access it. Menard's American colleagues knew that he had access to the most authoritative data, and they acknowledged that the success of their ideas were subject to his blessing. In early 1957 Bruce Heezen at Lamont began to receive press coverage for his discovery of a world-girdling rift he and Marie Tharp found along the mid-ocean ridge. This discovery indicated that the floor of the ocean was in tension, and that the crust might be expanding. For Heezen, this was the germ of proof for a theory that the earth's crust was mobile. After reading about the "fabulous rift" in the *New York Times*, Menard fired off a letter to Heezen indicating his distress over the theory, since there was no evidence of such a rift in the Pacific region. He considered issuing a contrary statement, especially since he understood the mid-ocean ridge to be a compression, not tensional, feature.[30] Menard's opinions on such matters were authoritative, and it must have irked Heezen to realize that the relevance of his research was contingent on it harmonizing with unpublished classified data. He replied immediately that all of the published profiles could be interpreted to support his view, though he recognized that Menard was sitting on a vast store of topographical profiles, all classified by the Navy, that might disprove the existence of a rift. Acknowledging the problem, he wrote: "If you could establish to our mutual satisfaction that your are right in this regard it might save me later embarrassment and the scientific literature further confusion."[31] The problem was mutual; Heezen of course had access to his own store of classified data, for Lamont had maps as well, all of which remained classified throughout the 1950s.[32]

Security concerns also constrained the British. Despite that the heat-flow device used on research vessels in the early 1950s was developed from an initial collaboration with an English geophysicist, British institutions were not always privy to the results of the expeditions. In 1953, the British Museum asked NEL to provide up-to-date figures for greatest depths in the Pacific basins and trenches. NEL responded that it was impossible to do so, given the classification status of the charts. Menard, who answered the query, added (helpfully?) that a number of the British figures were wrong, but he did not specify which ones were erroneous or make any corrections.[33] President Eisenhower, alerted to the damage this kind of security could do to Western collaboration, terminated the security classification "Restricted" in that same year. The Navy then simply upgraded their materials to a higher classification.[34] Yet ONR still showed its faith in the "invisible college" by providing funds to British scientists for their own research. A perusal of some of the more influential articles, explaining the puzzles of the sea floor in terms of the earth's mobility, from the late 1940s through the late 1960s reveals that a great deal of research was made possible by grants or contracts from the military, usually ONR. This included Englishmen Fred Vine and Drummond Matthews's famous article predicting that the stripes on the sea floor were actually a historical record of the earth's magnetic field. This work was based on the work of Dietz and Princeton's Harry Hess, who developed theories of sea-floor spreading based on mantle convection and the creation of crust at the mid-ocean ridge.[35] The Navy's funding practices solidified the already close relationship between English and American institutions.

### Soviet Science Contained

If classification placed constraints on the British-American "invisible college," the constraints were doubly severe for the Soviets, since the security measures existed specifically to prevent relevant research in marine geophysics from reaching them. In the mid-1950s and 1960s, however, the United States and the Soviet Union engaged in new efforts of cooperation in science, participating in joint projects such as the International Geophysical Year (IGY) and the Upper Mantle Project. The research results from all American IGY expeditions were immune to military classification and were freely reported to data centers throughout the world. Yet even the rhetoric of openness, and what one historian called "orgies of international research and cooperation," could not forge an inclusive international community of earth scientists.[36] The Cold War's technical and logistical restraints still made full cooperation very difficult.

Soviet oceanographers desperately wished to seize opportunities for cooperative ventures with the West. Stalin's death in 1953 marked an end to the Soviet scientific provincialism imposed under his rule. Projects such as the IGY promised to broaden Soviet scientists' own limited international scientific community to include countries outside of the Eastern bloc. ONR's Robert Dietz, while attending an IGY planning meeting in Brussels in 1955, made a note of "the Soviet 'thaw' of the 'deep freeze' policy in science." He was surprised to see so many friendly Soviets there, since only one Soviet oceanographer had shown his face at an international meeting outside the Iron Curtain since before the war. The Soviets all had been briefed on a new policy of cooperation, and they were eager to participate in the IGY. At the meeting, Dietz met Vladimir Beloussov, who led the Soviet delegation, and Vladimir Kort, the head of the Oceanological Institute of the U.S.S.R. Academy of Sciences. The dividing line between Eastern and Western scientific communities was so strong that no Western oceanographers knew Kort, even by reputation.[37] From his conversations with the Soviets there, and also at a Limnological Conference in Helsinki the following year, Dietz recognized that oceanographers in Russia knew they could not work independently of American efforts. The Oceanological Institute lacked a close liaison with the Soviet Navy, the existence of which in America was a crucial reason for the flourishing of deep-sea research. Unlike Americans, Soviets differentiated between oceanography and oceanology, the former being the geographic description of the sea, the latter being the science of the sea. While in the U.S. the lines between these were fuzzy, in Russia they were distinct disciplines. The Soviet Navy's Hydrographic Office associated itself more closely with the Institute of Oceanography, both of which were located in Leningrad.[38] The Institute of Oceanology sat in Moscow, far from Leningrad and 33 hours away by plane from its main research vessel, the *Vityaz*, at Vladivostok.[39] Marine geophysics in Russia lacked a consistent base of support for expeditions, therefore they needed to cooperate with the better-funded and better-organized Americans.

Americans did not always respond favorably to overtures of cooperation. Soviet expeditions in the Atlantic, for instance, could not sail east to west because U.S. ports were closed to them, so instead they sailed up and down the map between Europe and more friendly South American ports. This limited their research flexibility, though ironically it allowed them to discover east-west fracture zones in the Atlantic similar to those already found in the Pacific by Menard.[40] On their vessels they also used old, imprecise equipment manufactured either in the Soviet Union proper or in its satellite states. Dietz noted that a Soviet handbook on oceanographical instruments, published in 1954, appeared equivalent to what an American text might have looked like in 1940. The Soviets were still manufacturing echo sounders that only measured the depth of the ocean basin to 3000 meters, though their most cutting-edge research vessels were using British-made echo sounders (accurate to 6000 meters) they had obtained during World War II.[41] In 1960, a Soviet marine geologist, Gleb Udintsev, wrote to Menard asking him to recommend a more sensitive precision depth recorder that worked well for Americans. Like Menard, Udintsev believed that the sea floor was divided into provinces separated by fracture zones.[42] Menard, eager to correspond with and aid one of very few scientists concerned with this aspect of marine geology, responded emphatically that Udintsev should definitely upgrade his equipment. Without better instruments, it was impossible for Soviet scientists to gain reliable data on the slopes and structures on the ocean floor or to see the complexities of the ocean's bottom relief, such as craters on small seamounts or perhaps the more subtle rift valleys in the mid-ocean ridge.[43] Unfortunately, as Udintsev soon discovered, there was no way for Soviets to buy the instruments from the American firms who manufactured them, nor was it possible to purchase the titles to them from American scientists.[44] Such hurdles, inherent in the political tensions between the U.S. and U.S.S.R., placed Soviet scientists at a distinct disadvantage.

Because they shared results, scientists during the IGY circumvented many roadblocks set against full cooperation. Menard organized the 1957 Downwind expedition explicitly to investigate Bruce Heezen's claim that the mid-ocean ridge contained a rift that girdled the entire earth. The results of the expedition indicated that no such rift existed in the Pacific, and that Heezen's claim that the crust moved horizontally away from the ridge, on a global scale, was false. These results, unlike all other expeditions, were made available for analysis and interpretation by all scientists participating in the IGY, including Soviets.[45] Western scientists went out of their way to include the Soviets when they could. When Nikita Zenkevitch, a Soviet marine scientist, wrote to Menard that the U.S. government was not honoring a request for deep-sea photographs taken on the Downwind expedition, Menard sent the photos personally.[46] But the IGY was an exception to the rule. Zenkevitch, hoping he had an insider friend in Menard, wrote again to ask him for a complete bathymetric chart of the north-eastern Pacific region. Menard reminded him that the IGY only applied to their expeditions in the south and central Pacific, and that the U.S. Navy still strictly forbade the release of all other unpublished soundings.[47] Ironically, the results from IGY expeditions tended to disprove horizontal motion of the crust. Western scientists discovered only later that the rift was indeed there, hidden beneath the American continents.[48]

The Soviets understood the need for classification, but they were impatient with the sluggish publication practices of Western scientists. Both Zenkevitch and Udintsev urged Menard simply to publish soundings in a form acceptable to the Navy. Udintsev, wanting to participate in the interpretation of the sea-floor provinces Menard had found in the Pacific, lamented, ''I regret very much that at this time it is not possible for me to avail myself of the material of some of your expeditions.'' The bathymetric charts of Downwind helped him and his colleagues a great deal, but they still awaited the publication of the results of the Trans-Pacific and Northern Holiday expeditions, which were completed nearly a decade earlier.[49] The situation struck at the heart of the problem faced by an international research community connected only by formal publications and international conference presentations. In this case Menard was hampered because on these expeditions he did not take data that was of acceptable quality for publication; he had technical difficulties with his equipment, and he felt that any publication would lack scientific integrity.[50] Yet it was on Northern Holiday that Menard surveyed the most prominent fault scarp on earth, the Mendocino escarpment, the greatest of his ''fracture zones.'' His data, even if not publishable, was certainly both available and extremely valuable to scientists who visited Scripps.[51]

New techniques of analysis removed the Soviets even further from raw research results. Forbidden by the Navy to publish contoured bathymetric charts, Bruce Heezen at Lamont was hard pressed to illustrate his evidence for the world-girdling rift. So he and his colleague Marie Tharp resorted to track charts, which depicted the depth of the ocean floor along a given track. From such a chart they constructed physiographic diagrams, which simply were profiles of the ocean floor, showing a central rift along different parts of the mid-ocean ridge.[52] Such charts were wonderfully illustrative of the rift, thus supporting Heezen's own arguments. But their value for independent interpretations was very low. Readers of any publication no longer had a bathymetric chart of a region of the sea floor. Instead, they had Heezen's profiles, which he had chosen to fit his own interpretations. Soviet scientists wrote to both Heezen and Menard, suggesting a cooperative effort like the IGY that would establish a universally accepted tectonic chart of the world's ocean floors. The Soviets had begun such a chart on their own, but they were keen to incorporate the latest, most relevant soundings of the ocean floor.[53] A collaboration would compel American scientists to sort through and publish their stores of classified soundings. Americans, themselves annoyed by the constraints of classification, agreed to take part. Largely from their complaints, many restrictions on the soundings were lifted in the early 1960s, and American scientists for the first time were able to discuss with Soviets the echo soundings made since the war.[54] Despite this success, in the mid-1960s the Navy ironically achieved a *de facto* technological containment of data by infusing Lamont and Scripps with money to process data by computer. When Lamont and Scripps began to exchange data by computer tape, no other institution in the world yet had the technology to make sense of it.[55] Both of these techniques—track charts and computer data processing—exposed the readers of any scientific publication only to research interpretation, while alienating them even further from the original raw research results.

Even when scientists began to sympathize with the plight of their Soviet colleagues, bureaucratic resistance hampered their efforts to improve international cooperation. Recognizing the importance of having scientists physically present in their institutions and on expeditions, Americans began to invite Soviets to visit. On the International Indian Ocean Expedition (IIOE), one of the many international efforts spawned by the success of the IGY, Menard invited Udintsev to come along. Udintsev was to join the 1961 Monsoon expedition, a part of IIOE, in order to observe "all of our geological and geophysical techniques" on a survey of the Pacific sea floor.[56] Since the invitation for the expedition included only a leg between New Zealand and Tahiti, it did not entail entering the United States. Menard did not bother to obtain or even to ask permission to include the Soviet scientist. All went as planned until Udintsev wrote apologetically that he was unable to join Menard "owing to circumstances beyond my control."[57] The problem was not Udintsev's; Menard simply had not anticipated the resistance he would encounter: "[T]he whole thing blew up, because of objections from the Navy, who own the ship. I also heard that this procedure was frowned upon by the [National] Academy."[58] The National Academy of Sciences felt that the Americans should not agree to any visit by a Soviet unless there was an explicit agreement that an American would go to Russia for a visit as well. After bowing to the limitations of political even-handedness, Udintsev was able to visit Scripps in 1963, only after Menard received an invitation to travel to Russia.[59] The attempt to create a truly international scientific community during the IGY failed. As it was dubbed when the National Science Foundation sold it to Congress, it was indeed only a "one shot deal."[60] Only the military had a sustained flow of resources to conduct ongoing oceanographic research, and it naturally had a vested interest in defining the boundaries of scientific communities along Cold War lines.

## The New Paradigm Triumphant

Western scientists remained conscious that their Soviet counterparts were outsiders. To them, this was no surprise. They explained it away with claims that, for various reasons, Soviets could not be counted on to do good science. J. Tuzo Wilson made the logical assumption that their research, primarily conducted on or near the very stable Asian land mass, gave them no reason to buy into the concept of a horizontally mobile crust.[61] Many also still believed that the Soviets' strict adherence to anti-idealism meant they did bad science. Further, many believed that the Soviets were simply too backward to be taken seriously. To Western scientists, the Soviets operated outside the new and now dominant paradigm of earth science—plate tectonics—making real contributions by them virtually impossible (figure 3).

Westerners gained every indication that the Soviets could offer little new information to their existing body of scientific knowledge. There was only a paltry number of Soviet works published on marine geophysics in English, the language that the vast majority of scientists in the field spoke and read. More importantly, interactions with Soviets at international conferences inspired little confidence among Westerners. It was clear to any observer that science was a "sacred cow" in Russia, but Soviets complained of too many difficulties in their research for them to appear anything more than second-rate. Americans believed, from conversations with their Soviet and Eastern European colleagues, that in Russia: (a) it was often impossible for scientists to obtain books in their field; (b) scientists were customarily paid according to their scientific output; and (c) ships used outdated equipment and employed defective or insufficiently accurate analytical techniques. An overall impression emerged that marine geophysics in Russia was uninformed, insubstantial, and unreliable. Observers noted that Soviets often failed to appear at conferences, even when they were scheduled to deliver papers. At one meeting in Helsinki in 1960, Soviet papers were cancelled by the dozens owing to no show, a disappointment that Bruce Heezen called "an utterly typical Russian performance."[62] Ironically, the Soviets showed contempt for the research done in their European satellite states, "brushing it off," wrote Dietz in an ONR technical report, "as being of no importance."[63] The Soviet arrogance toward scientists in Eastern Europe was the same as that of Western scientists toward the Soviets.

Another reason to remain disinterested in Soviet views was the argument that philosophical biases skewed Soviet science. One of the fundamental principles of Stalinist dialectical materialism was that the accumulation of experimental and observational material should help to obliterate the idealist essences in many scientific theories. Ardent anti-idealism and an aversion to any science aimed at

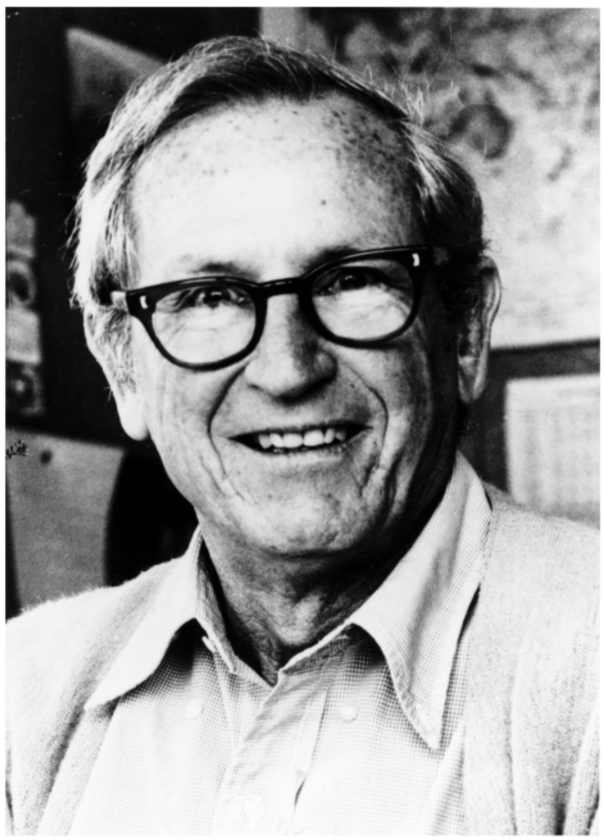

**Fig. 3.** H. W. Menard, who in 1986 wrote that he was conducting normal science under a new paradigm by the 1960s. Credit: U.S. Geological Survey, courtesy American Geophysical Union and American Institute of Physics Emilio Segrè Visual Archives.

increasing profits earned Soviet science a poor reputation in the 1930s.[64] Later, when Vladimir Beloussov published his plea to avoid jumping on the bandwagon of plate tectonics, he argued that this emerging model of the earth was too formalistic, too schematized.[65] Such reasoning appeared to fit the anxieties stereotypical of the proletarian scientist. The stereotype was no mere caricature of Soviet science; dialectical materialism did continue to inform scientific practice in the Soviet Union even into the 1980s.[66] Yet, a modern reader can sympathize with Beloussov's complaints. For one, the new tectonics *was* rather schematized, and its early proponents admitted as much. When Princeton's Harry Hess first proposed the theory that would be called sea-floor spreading, he did not call it his theory but rather preferred to "consider this paper an essay in geopoetry" in which he did not wish to "travel any further into the realm of fantasy than is absolutely necessary."[67] He claimed that new crust was created at distinct spreading centers, likely at the mid-ocean ridge, and that the crust traveled outward away from the ridge before being destroyed in the deep trenches found in the ocean floor. Indeed, all of plate tectonics was built upon uncomplicated model-building, which showed the fit of the continents and the geometric rotation of plates. But since the complaints came from Beloussov, they appeared to be more ideological than scientific. Old criticisms of Soviet science, inherited from the pre-war period, provided Westerners with a precedent to ignore him.

By the end of the decade, Hess's caveat about his theory being overly simplistic mattered little in the West, for the criteria required to legitimate his theory, despite its schematic simplicity, had been met. In 1960, when Hess first revealed his idea, the more stubborn minds would require not merely a plausible idea but also independent tests to corroborate the theory before converting to it.[68] In 1963, Vine and Matthews published an idea that was equally neat and plain. They stated that the magnetic stripes on the ocean floor were simply bands of mantle material magnetized according to the earth's magnetic field and then pushed away from their source at the mid-ocean ridge. If this were true, the stripes on the sea floor provided a historical record of changes in the earth's magnetic field.[69] In 1966, a ship operated by Lamont scientists, the *Eltanin*, recorded these magnetic reversals as it passed over the mid-ocean ridge. The results for one of the legs showed that the pattern of stripes (recording magnetic reversals) flanking one side of the ridge mirrored the stripes on the other. This discovery shocked scientists at Lamont; it indicated that Hess's "geopoetry" of sea-floor spreading was correct. The clincher for most scientists in the West came when the epochs recorded by the magnetic stripes were correlated with independent methods of dating the earth's magnetic reversals through paleontological and radioactive evidence.[70]

Scientists all over the world tried to put the evidence for sea-floor spreading into a global theory. Vladimir Beloussov insisted that vertical motion, not horizontal motion, accounted even for the new evidence. He explained the magnetic stripes by a process he called "basaltification." When vertical blocks shifted, basalt magma occasionally poured out between them, as in the case of the mid-ocean ridge. The magma then poured over the crust, covering the ocean floor. The flow decreased over time, creating a shingle effect. Beloussov claimed that the magnetic lines were simply layers of magma that solidified at different times.[71] Meanwhile scientists in the West were pursuing research based on the horizontal motion originally conceived in Hess's "geopoetry." They turned their attention back to Menard's fracture zones, which offset not only the magnetic stripes but also the mid-ocean ridge itself. In 1965 Lamont seismologist Lynn Sykes realized that seismic activity along the great fracture zones occurred only in the zone between the ridge crests.[72] The Canadian J. Tuzo Wilson proposed that these regions were in fact special kinds of fracture zone: transform faults, where different blocks of crust slid past each other, creating enormous seismic activity. The rest of the fracture zone was simply a "dead" transform fault, meaning that its two sides were traveling in the same direction. This concept bridged the concept of sea-floor spreading with a new tectonic theory of massive blocks moving relative to each other over the surface of the earth.

Wilson did not merely publish his idea and then hope for the best. His 1968 article in *Geotimes* declaring the "revolution in earth science" was polemical, intending to persuade, not merely to report. He wrote, "What an exciting challenge this is! What a chance for great discoveries! What an appeal to young men!"[73] Beloussov criticized his enthusiasm, calling it an appeal to emotion, not to rational science.[74] This was true enough, but Beloussov likely also was miffed by Wilson's exhortation to scientists, universities, and industry alike to abandon old concepts and to support by all means a new research program assuming a mobile sea floor. Yet to make any comment about bourgeois science would have made Beloussov vulnerable to further criticism. Wilson could be quite zealous with his propaganda, behaving more like an advocate than an indifferent and measured scientist. Others shared this role. Edward Bullard's diagrams of continental fits were produced on a large scale with expensive colored paper, a rather unusual occurrence, which he later admitted was probably responsible for their great impact.[75] Many advocates, including Dietz, then back at NEL, and several scientists at Lamont, went on lecture tours that some referred to as "roadshows." Few complained; Menard wrote, "A scientific revolution requires more than a letter to *Nature*. Even Darwin had his Huxley. A new idea must be advertised and sold."[76]

This term—"scientific revolution"—quickly invaded the vocabulary of nearly all earth scientists describing their field by the late 1960s. The new findings coincided with the publication of Thomas Kuhn's seminal philosophical work, *The Structure of Scientific Revolutions*. Few things justified the lonely success of plate tectonics as much as the West's fascination with Kuhn's book and the philosophy he espoused in it. Western earth scientists were utterly convinced that they were experiencing a Kuhnian paradigm shift, making all other hypotheses irrelevant. The time-honored tradition of the nineteenth-century geologist T.C. Chamberlin—the method of multiple working hypotheses—was discarded.[77]

Kuhn's book created a framework for understanding the shift in outlook between the worlds of the ancients and moderns, and earth scientists adopted his model. For Kuhn, the continuous cycle of scientific activity moved from normal science within an existing paradigm, to a collection of anomalies that did not seem to fit the existing paradigm, to a crisis in which the existing paradigm simply could not account for the number or magnitude of anomalies, to a paradigm shift in which a new outlook replaced the old, and finally to a new period of normal science pursued within the new paradigm.[78]


**Fig. 4.** J. Tuzo Wilson in 1982. Credit: American Institute of Physics Gallery of Member Society Presidents.

Kuhn found an enthusiastic audience among earth scientists. By 1966, Menard believed that he was already doing "normal science" within the framework of the new theory of a mobile sea floor.[79] Wilson (figure 4) could not have agreed more when he thanked industry and military for helping amass data in the 1950s and 1960s. "Was not Tycho Brahe," he asked, recalling Tycho's massive accumulation of astronomical data in the late 16th century, "followed by Kepler and Newton?"[80] Beloussov asked only that they all should wait for new data, perhaps to be provided by the international Upper Mantle Project, the committee for which he chaired.[81] Wilson demurred, claiming that if two groups of scientists studied whirlpools and one group refused to acknowledge that the water moved, no amount of data would help resolve the problem. "It's not new data, but a change in outlook that marks a scientific revolution," Wilson remarked, "as T.S. Kuhn ... has so elegantly pointed out."[82]

Wilson's 1968 conversation with Beloussov in the pages of *Geotimes* proved to be the last word on the issue of the mobile sea floor. It established a philosophical justification for the barriers, provided by the Cold War, already keeping the Soviets away from Western earth science. The exchange followed on the heels of a failed international conference of geologists and oceanographers in Czechoslovakia the same year. The conference took place amidst a cooperative effort, the International Upper Mantle Project, that was headed by Beloussov and included many American scientists. Dozens of Soviets arrived at the conference, only to be encouraged to "melt away" not long after it began. Scientists awoke to the sound of gunfire as rebels fought Soviet tanks on the streets of Prague. Western scientists wore on their nametags black strips of mourning and protest, while the Soviet scientists simply disappeared from the conference. Beloussov's Upper Mantle Committee met without him.[83]

The absence of the Soviets mattered little. Lamont seismologists such as Lynn Sykes shaped the agenda of the Upper Mantle committee, and they decided to concentrate on seismic studies that would show what happened to the crust as it dipped into the trenches, as sea-floor spreading would have it. He and his colleagues soon published papers revealing that different velocities of seismic waves indicated that a cold, solid slab of crust was plunging deep into the molten mantle.[84] The same year, papers by scientists at Lamont and Scripps synthesized disparate concepts such as fracture zones, mid-ocean ridges and rifts, magnetic anomalies, and sea-floor spreading. These were the first papers published in a mature new science: plate tectonics. The authors envisioned huge spherical caps, or blocks, moving horizontally over the earth's surface, created at spreading centers and destroyed at trenches.[85] Menard and Wilson were right; it was a new paradigm for earth science.

Philosophy gave earth scientists a justification for pursuing their paradigm regardless of objections Soviet scientists might have. The trouble with Kuhn, for the Soviets, was that he did not allow for alternative paradigms. Another philosopher, Imre Lakatos, pointed out that even if a new paradigm was imperfect, it still retained its value as a guide for research.[86] That was precisely what Wilson had in mind when he called on industry and universities to throw their financial resources into earth science that embraces the concept of a mobile sea floor. He claimed that it was the most exciting event in geology for a century and that "every effort in research should be bent toward it."[87] When he began to publish in *Scientific American*, a semi-popular magazine with a conservative tradition of allowing only reasonably well-accepted concepts onto its pages, Wilson gained further legitimacy for his research agenda.[88] The Soviets, widely perceived as champions of a defective brand of earth science, could not possibly have a more competitive paradigm in mind. The Soviet scientific community, already physically, politically, and technically isolated, could be left behind without much concern.

## Conclusions

The Cold War made a single international community of earth scientists impossible. Classification of militarily sensitive research results created small, self-contained communities centered around institutions. In the West, these institutions merged into a somewhat coherent larger whole. Scientists with classified clearance at American institutions, such as Scripps and Lamont, spoke to each other, creating a multi-institutional network to discuss research interests and goals. American scientists reached across national borders to cooperate with scientists of the United States' closest political ally, Great Britain. Bound by security constraints, this community excluded Soviet scientists. Even when governments consciously attempted to bridge the gap between West and East, as in the case of the IGY, only limited and ephemeral cooperation reigned.

The communities fostered by the Cold War were reinforced by the development of two fundamental yet incommensurable scientific ideas about the earth's crust: horizontal mobility versus relatively minor vertical motion. Western scientists increasingly pushed their research agendas toward projects that accepted the concept of a mobile sea floor. Their perceptions of the practice of science in the East allowed them to believe that the Soviets' contrary interpretations were faulty and unreliable. As new evidence poured in from expeditions during the mid and late-1960s, even international efforts at cooperation were burdened by the different scientific aims of the participants. Soviets resisted Western interpretations of the new evidence, but projects were organized according to Western aims anyway. As the network of scientists in the West increasingly pursued horizontal mobility of the crust, scientists in the East pursued vertical tectonics. Two distinct international communities, fragmented already by the Cold War, crystallized for the long term over the issue of plate tectonics. Championing it as the new paradigm, Western scientists made acceptance of plate tectonics the criterion for legitimate knowledge in earth science. Englishman Edward Bullard admitted that the sudden acquisition of a grand theory had transformed earth science from backwater to bandwagon.[89] This particular scientific bandwagon was formed, cultivated, and advertised by an exclusive Western scientific community that was designed to keep the Soviets out.

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**A Child Needs Support**

J. D. Cockcroft wrote about Ernest Rutherford that:

> The Prof is a great economist and he's continually having tiffs with [Peter] Kapitza on this score. He told Kapitza the other day that he was an "expensive child." To this Kapitza retorted that "if you bring a child into the world you must educate it". And so he got his telephone.

Quoted in Guy Hartcup and T. E. Llibone, *Cockroft and the Atom* (Bristol Adam Hilger, 1984), pp. 30–31.