# PLATE TECTONICS AT THE THRESHOLD OF MIDDLE AGE[1]

TJEERD H. VAN ANDEL[2]

## ABSTRACT

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Plate tectonics has been in the foreground of geological thinking for almost twenty years. It has proved to be an immensely useful concept and its consequences, especially for the history of the earth and life, have by no means been fully explored. As is inevitable with any comprehensive theory, however, flaws are also beginning to show, mainly in the form of a growing of *ad hoc* modifications which cast doubt on the claim that this is the ultimate unifying new global tectonics. What the future shall bring is a matter of conjecture, but any new dynamic theory of the earch is certain to include continental drift.

It is now almost a quarter century ago that the march towards a new theory of the earth – begun some fifty years earlier by Alfred Wegener – rather suddenly accelerated, to end just a few years later in wide acceptance of the currently dominant model of plate tectonics.

The protagonists, then largely quite young, have now reached middle age, and so has, in terms of the usual lifespan of scientific theories, plate tectonics itself. In the years that have passed since then our science has changed enormously, more than through the introduction of radioactive dating, as much, probably, as through the combined impact of Lyell's disposing of catastrophism and Darwin's introduction of the theory of evolution. It seems proper to take stock as this point and see whether the theory remains youthful and vigorous or is beginning to show some early ravages of age, the need, perhaps, for a face-lift or two.

A geological revolution it has been called, and a revolution it certainly was. At the 1928 meeting of the American Association of Petroleum Geologists in Atlantic City, where Alfred Wegener was read out of the community of respectable geologists, one participant declared in evident despair that, if one were to believe Wegener's hypothesis, one should have to forget everything learned over the past 70 years and start all over again (CHAMBERLAIN, 1928). All I can say is that he was not far wrong.

My memory of the rapid flow of events in the late 1950s and early 1960s is vivid, as I watched the mass of marine geological and geophysical data grow, skeptic at first and amazed at the evident willingness of fellow scientists to indulge in soaring flights of fancy. It was a time when enthusiasm not uncommonly left evidence far behind, and when those who, like myself, thought that the world just could not have been put together quite so tidily, were met with sometimes undisguised contempt.

Now, a mere two decades later, what was then a courageous and farsighted set of hypotheses has graduated to the position of ruling theory, but the contempt for skeptics still emerges at times. The reviewer of a recent book on Precambian plate tectonics stated without attempt at tact that those who question the literal application of 'classic' plate tectonics to even the remotest past thereby simply demonstrate their inability to understand the concept, thus carrying Hutton's famous adage 'no vestige of a beginning, no prospect of an end' to a frank denial of the second law of thermodynamics. It is not just possible that, on an earth driven by a heat engine, Precambian plate tectonics was different from the Cenozoic mode, it is a certainty.

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[2] Geology Department, Stanford University, Stanford CA 94305, U.S.A.

This sort of thing, of course, happens to all theories once they become the conventional wisdom and, imperceptibly at first, begin to retard rather than to advance the pursuit of knowledge. I shall return to this later but let us not overlook here the elegance of the theory itself and of the manner of its birth, an elegance which, even if it does not guarantee truth, always inspires the loyalty as well as the admiration of the scholarly beholder.

The revolution proceeded in three steps, each with its own confirmatory stage. The facts, in the beginning, were all derived from the ocean floor, the grand morphology with mid-ocean ridges and trenches, the distribution of heat flow, seismicity, and volcanicity, the curious increase in age of many volcanic islands towards the ocean margins, and the striking patterns of marine magnetic anomalies. The list could be longer; decades of study of the ocean basins finally bore fruit in showing a remarkable regularity, here and there even symmetry (somewhat overstated in the term 'mid'-ocean ridge), rich in intrinsic though initially dark and difficult meaning.

The first attempt at explanation was simple, borrowing (unknowingly) from Arthur Holmes' already old proposal that mantle convection drives the continents. Dealing successfully with such matters as mid-ocean ridges, trenches, heat flow patterns and various other symmetries, the model signally failed to account for either magnetic anomalies or changes in crustal age, nor did it have any need for the deep seismicity of the Benioff zone.

Amended around 1960 by H. H. HESS (and widely circulated, although published only in 1962) to become what was later called the 'seafloor spreading' model, these details were mostly taken care of. The model received impressive support from F. J. VINE & D. H. MATTHEWS's explanation of the magnetic anomaly pattern (1963), and from J. TUZO WILSON's transform fault hypothesis (1965) and its seismological verification by LYNN R. SYKES in 1967. It was no wonder that, in a flush of triumph, many for a moment lost sight of reality.

The reality was, however, that thin-skin seafloor spreading raised many structural problems, that the model did not fit that most symmetric ocean, the Atlantic where trenches are virtually absent, and that the proliferation of transform faults began to demand long, impossibly narrow convection cells.

The final hypothesis, confidently announced as the 'new global tectonics', emerged in 1967-68 in a rapid-fire series of publications. It was able to deal systematically and on a global scale with the horizontal motions of the earth's crust, it offered hope that it would unify the tectonic histories of distant lands and, above all, it appeared capable of rigorous testing. Its only flaw, not regarded as particularly serious in the countries of its birth, was its inability to account for, or rather its indifference to, large vertical movements in plate interiors.

Looking back one now realizes that the step from subsurface convection to seafloor spreading, though enormously valuable, was not so large. Much greater was the one from seafloor spreading to plate tectonics, demanding that immensely difficult though largely subliminal feat, the removal of a conceptual block. The act, in this case, consisted of ignoring the Mohorovicic discontinuity and the almost universally held view that it and the difference between ocean basins and continents were the two essential features of the crust of the earth[3]. Once several, understandably young geophysicists had decided to do just that, however, and instead had put on maps where the seismic and volcanic action at the earth surface was, the image of large plates suddenly emerged in startling clarity, geometric treatment followed suit, and plate tectonics was born.

I should like to emphasize here that throughout this whole daisy chain of hypotheses both the data set and the models derived entirely from the oceans, and that up to the very last stage no need was felt for drifting continents. Even though paleomagnetic studies beginning in the 1950s had revived Wegener's idea, those of us engaged in explaining the ocean basins were not especially intent upon testing that concept, while many continental geologists viewed the goingson with indifference, regarding them as irrelevant to their concerns. It was a remarkable reversal of Wegener's course, and all the more intriguing because plate tectonics succeeded, no more than the continental drift hypothesis, in furnishing a satisfactory driving force, yet did not find its acceptance impeded by this failure as it had been Wegener's misfortune. Evidently, a ruling theory is more likely to collapse under wide recognition of its own accumulated cracks and fissures, than as a result of the emergence of a new, more complete and coherent model.

Since then the exploitation of the new theory has mainly followed three intertwined pathways. First, and long dominant, came much activity designed to establish the present and past configurations of plates in the oceans, eventually yielding various paleogeographic reconstructions and an enhanced understanding of plate kinematics. A second pursuit, that of plate dynamics, enjoyed a brief period of vigor but then sank away into obscurity, from which only recently new tools to map the nature of the mantle have begun to rescue it. Thirdly, there has been a persistent and still growing interest in processes at plate boundaries. Of these, the divergent boundary always seemed the simplest, and application of an impressive array of technology has indeed proved that to be to some extent true. A reasonable level of understanding of the volcanic and structural processes that create new lithosphere is thus beginning to emerge.

On the other hand, due to the deep-seated nature of much of the evidence, converging plate boundaries have proved less hospitable to investigators, forcing them to turn to ancient orogens in the hope of applying the key of the past to the present.

[3])This was not made easier by the fact that in those very years many were still dedicated to the concept of drilling the Moho, and were vastly perturbed by the unwillingness of the U.S. Congress to invest substantial tax monies in such a 'fundamental' enterprise. We may be grateful, that it never happened.

These, however, are not my principal concerns; rather it is my fancy to test the smoothness of the road behind us as well as the one ahead.

In 1973, the United States Geodynamic Commission brought forth a plan for the exploitation of the new theory which, in addition to some understandable jubilation and many a proposed logical followup, also raised two major questions which remain unanswered to this day, and have gradually moved more into the foreground of our consciousness. A third emerged slightly later from accumulated experience. The questions are these: how successful have we been in the reconstruction of past plate configurations, how well does the theory deal with regional, perhaps even global uplift and subsidence in plate interiors, and how well does plate tectonics serve to explain events and processes on plate boundaries, especially converging ones?

Reconstructions of Cenozoic and Cretaceous plate configurations are now common. Some, such as ERIC BARRON and co-workers (*e.g.*, 1981), have found the global paleogeography of the last 100 Ma without mystery, others have tried just the Pacific Ocean and found it hard. In fact, it increasingly seems that, going back past 50 or 80 Ma, one is forced either to create *ad hoc* platelets, or to violate the basic rule that says plates must not be deformed internally. This rule, of course, is necessary if reconstructions are to be based on the precepts of spherical geometry rather than on personal preference, and we always realized that its application to nature was a matter of scale. It is beginning to appear, however, that internal deformation may be a matter more momentous than we once thought (or hoped). For the early Mesozoic and Paleozoic, with no ocean floor left, paleomagnetic data provide only latitudes, while longitudes must be guessed at or derived from circumstantial evidence. Even so, however, sensible reconstructions now exist which, whether entirely correct or not, have greatly enhanced our apprecation of just how different geographically the past was from the present. They, and especially their Cretaceous and Cenozoic equivalents, have produced a cascade of secondary revolutions in stratigraphy, paleobiology, and paleoclimatology, and given birth to a new discipline of paleoceanography, thereby enabling us to say things about environments that a few years ago were not even thought possible, and to do so at least partly independent of the paleontological evidence. This, in turn, has enriched the potential of paleobiology and given new dimensions to the study of evolution.

Not by any means so joyous a scene greets us when we return to the matter of vertical epeirogenic movements. Geologists in the U.S.S.R., such as V. Belousov, recognized early that plate tectonics would not provide the explanation for regional continental rise and fall or eustatic sea level changes, the main themes of Euro-Siberian geology. Americans and northwestern Europeans, on the other hand, living near plate boundaries, found the new theory useful and, with a few exceptions such as L. L. SLOSS (1979), thought the vertical movement problem one that would soon take care of itself. It has not all done so, however. The current and still rising interest in past sea levels, fueled by the work of Exxon seismic stratigraphers and culminating in what is known as the Vail curve, has brought an interest in the causes of eustatic sea level change, but attempts to relate them to plate tectonics (*e.g.* PITMAN, 1978) have been less than successful (HALLAM, 1984). SLOSS, on the other hand, has devoted years to convincing his colleagues that the continents themselves, not just the sea alone, rise and fall, perhaps even in unison, and has suggested some mechanisms, although not very specific ones. He has not, however, so far convinced many that the problem, an old one as H. Stille's and later J. H. F. Umbgrove's search for the pulse of the earth shows, is a serious one. Yet the quest for rhythms of the earth remains a spellbinder, to which SCHWAN (1980) has recently fallen victim. The matter, however, is so slippery and the chronology so rubbery that he who seeks correlations between sea level changes, intensifications of orogeny, and plate motions, will generally be able to persuade himself but not usually his colleagues.

Perhaps even more crucial in our assessment of how well plate tectonics has served us is its application to the history of plate boundaries. It is there, after all, that most of the action is, and if the model should be capable of unifying the histories of rifts and orogens world-wide, it would have kept its promise.

Simplest is the divergent plate edge where ocean crust forms and, sometimes, continents split. Here, I think, we may pause with some satisfaction, and note that we understand far more about oceanic rifts than we did twenty years ago. It would not be surprising, in fact (a statement I may live to regret), if another twenty would achieve a quite satisfactory grasp of how new lithosphere is created.

Even rifting in continents is better understood now than it was, but as soon as a plate boundary touches a continent matters commonly become very complex. One can cite the Gulf of California, classic example of an incipient ocean. Originally seen as a simple shear, it became a set of short rift sections separated by long transforms. Now, new data require an otherwise not obvious plate boundary across central Mexico, an undocumented subduction zone at the tip of the peninsula, and a few microplates to make things fit (G. NESS, personal communication, 1983). Even so one has to contend with oddly distributed zones of seismicity and some incompatible spreading rates.

Let us allow this to rest, noting only that microplates have been found very helpful indeed, and turn to another subject. The pursuit of plate kinematics has not been limited to past plate configurations. Very early, almost immediately, there was a turn towards application of the theory to ancient orogens, the Appalachians being the first (DEWEY, 1970) and, initially, seen as a classic example of how plate tectonics would simplify our view of the earth. It was not to be so.

Subduction in the Appalachian geosyncline is first seen around 500 Ma ago, and is followed by the Taconic orogeny, perhaps because of that mind-boggling process, a reversal of the direction of subduction. Though not attributable to continental collision, the result was nevertheless the appearance of a sediment source to the east too copious to be easily identified with an island arc, and leading to a reversal of the direction of sediment supply from eastward to westward. Next followed the Acadian-Caledonian orogeny, a collision between North America and Baltica in the north, where it raised enough land to spread the Old Red Sandstone eastward and the Catskill delta to the west. Fair enough, but the central Appalachians farther south experienced a similar intensive deformation, though without benefit of a collision with an eastern continent. What, one reasonably asks, caused these twice repeated collisions and associated phenomena in the absence of any suitable continent pairs? It might be convenient to assume one or more microcontinents, slivers such as California may be some day, drifting in the Gulf of Alaska 50 Ma hence, but such *ad hoc* solutions, saving the day as they do, do not put the mind at ease.

The Appalachians are not alone. Wherever we look at a major orogen, be it the Andes, the western North American Cordillera, or the Alpine system, we find that, though the oceanic record indicates steady rates of subduction, orogeny and associated magmatism are distinctly episodic.

Neither do continent-to-continent collisions fit smoothly into the plate tectonics framework. The India-Asia collision has produced a splendid suture, but the monumental Himalayan range is younger, its rise involving considerable magmatism and crustal shortening. PETER BIRD's (1978) ingenious process of lithosphere delamination may well be the answer, but once again we are asked to put our trust in major, and rather special modifications of the model.

To return to the episodicity of mountain building, the currently fashionable concept is that of exotic terraines, fragments of oceanic or, less well argued, continental crust. When, after long voyages through ocean basins, they are finally caught up in a subduction zone, they raise havoc because their thickness and relative low density prevent them from going down. Accreting to the continental margin they force the subduction zone to find the easiest way out and to jump seaward. Numerous such exotic terraines have recently been proposed, and some solidly documented, first in northwestern North America, and now all around the Pacific 'ring of fire'. They are popular these days, multiplying at least as rapidly as hotspots did in the first few years after JASON MORGAN drew attention to their major role (1972). Their plausible, and sometimes not so plausible origins can be half a world away, although some may be exotic merely because their contacts with the surrounding formations were not so clear.

Apart from the question of how exotic many of these terraines really are, there is that of the magnitude and, if I may put it so, the usefulness of their role in global tectonic history. Small scale components of any model, such as exotic terraines are, satisfy their discoverers because they can call a piece of earth history truly their own, but they complicate the pursuit of orderly regional or, if they exist, global trends. If the earth should possess something resembling a unified history, the discovery of exotic terraines, no matter how real they may be, will inevitably play a retarding role. I therefore hope with Gary Ernst (KERR, 1983) that exotic terraines, although they complicate the picture, will turn out to have been a minor shuffle, but there is always reason to fear an increase in the degrees of freedom in science, because they allow many to avoid facing the big picture.

Finally, in this long list of concerns, let us note the insistence in plate tectonics that the lithosphere is unaware of the difference between ocean basins and continents, notwithstanding the fact that the theory works far better in the first setting than in the second. This precept has been nibbled at by a variety of proposals, such as subcrustal erosion, delamination, and the gradually emerging evidence that, as TOM JORDAN (1978) has insisted for some time, the differences between continents and oceans extend far deeper than the 100 km usually assumed for the lithosphere. It is not surprising that problems of both a scientific and a psychological nature should begin at the continental margins. After all, the ocean crust is simple in composition and has had a brief history, uneventful compared to that of the continents, and it is in the oceanic crust that the concept of an unlimited isotropic half space, so useful in geophysics, find its best approximation. The continental crust, on the other hand, as a rapidly growing data body from deep seismic sounding demonstrates, is far from isotropic and retains the memory of many billions of years of a complex deformation history.

One hesitates here, seeing the path lead inexorably to three branches of earth dynamics, one for the deep mantle, one for ocean basins, and another for continents. It seems best to desist at this point and proceed to a conclusion of sorts.

It does not seem extravagant to anticipate another revolution during which a new unifying theory will be shaped, this time probably starting either with empirical data from the continents or with a theory of mantle dynamics, rather than with the oceans. It should incorporate, among many things, the properties of the deeper mantle, the divergent memories of oceanic and continental crust, the episodicities of orogenesis, and the vertical movements of continents. In all probability, the new theory will be less simple and elegant than the one we cherish at the moment. It is also bound to be long in coming, because the defects of the current version do not yet appear to most geologists to be burdensome. Meanwhile we might spend our time learning to understand mantle dynamics, and becoming resigned to the (inevitable I believe) near future when each part of the world will have its own tectonic history, only loosely related to others by theory, like it used to be in the good old days.

Plate tectonics, proudly announced as the new global tectonics, never truly was that, being above all a model of the behaviour of the oceanic crust, with limited implications for events at plate boundaries adjacent to continents. Paradoxical as it may seem because plate tectonics and continental drift are not synonymous and not inherently coupled, its major gift to us has been the removal of a nearly universal mental block against continental drift. It is thus that we shall benefit most from the geological revolution of the 1960s; the shift from a fixed to a mobile earth has by no means been discounted yet in our grasp of the history of our planet.

I expect that this statement may be construed as evidence that my initial reluctance to embrace seafloor spreading continues to affect my judgement and prevents me from fully understanding plate tectonics. Be that as it may, I should like to close by noting that the recent geological revolution, so idiosyncratically laid out here, vindicates the man whose name graces today's award, and who was among the very few at Atlantic City in 1928 to warn his colleagues that they would eventually seem foolish in their resistance to new ideas no matter how intimidating they might be.

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