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Tectonic Consequences of the Earth's Rotation by Robert C. Bostrom. Oxford, UK: Oxford University Press, 2000. 266 pp. \$95.00 (hardcover). ISBN 0-19-509028-4.

In the past, analyses have made it seem unlikely that the Earth's rotation has affected global tectonics. Recent data suggest otherwise.

With this statement, albeit, only partly true, Robert Bostrom opens his chapter on Historical Perception, seeking the roots for thoughts about a connection between the Earth's rotation and global tectonics. Under the reign of plate tectonics, the potential link between the long-term inertial slowing of the Earth and its geological evolution has not been a matter of serious debate. Regardless of this ignorance, the strongly latitude-dependent lunar tidal friction is being assumed currently to be the dominant cause of both secular deceleration of the planet's spin rate and acceleration of the Moon's rate of recession. At present, the gravitational influence of the Sun-Moon system on the Earth, having its maximum effects at low latitudes, is relatively significant. However, there is hardly any geological evidence that deep ocean basins existed prior to the middle Mesozoic, some 150 million years ago, probably implying that oceanic tidal slowing of the Earth was not significant in earlier geological times.

The moment of inertia of a rotating body is a way of expressing the concentration of its mass about its centre of gravity. The greater the concentration of mass, the greater its moment of inertia, and the faster the body will spin. Thus, any net inward motion of mass increases the Earth's moment and its rate of rotation. Similarly, any net outward (equator-ward) mass transport decreases the moment of inertia as well as the planet's spin rate. An observation supporting the hypothesis of a relatively faster spin velocity in the geological past (compared to now) is that the present Earth has a certain excess flattening, making its equatorial bulge some 200 m larger than that expected. Munk and MacDonald (1960) suggested that this non-hydrostatic bulge is a relic of a faster rate of rotation in the past. Also, lengths of day estimates based on growth rings in fossil shells demonstrate the overall reduction in spin rate for the last 600 million years or so (as per data compiled by Creer [1975]). If the Earth has been undergoing long-term internal degassing, giving rise to continual upward transfer of mass since Precambrian times (see Storetvedt, 2003), probably even reducing the size of the core, it may have been in a state of decelerating rotation during most of its history. In his book, Robert Bostrom hardly touches upon these important aspects of Earth's rotation.

The gravitational tug from the Moon and Sun are predominantly recognized by the rise and fall of the sea, but the solid Earth too undergoes an up-and-down motion caused by external gravitational pull. The latter (bodily) tidal response,

producing much less surface distortion than the oceanic part, constitutes one-way body waves progressing unimpeded around the Earth—in a direction opposite planetary rotation. However, since the solid Earth is not perfectly elastic the bulge does not relax immediately, causing a certain torsional stress on the outer brittle layer. Hence, any long-term structural effect on the Earth's crust (tectonics) would not seem an impossible proposition. So, at the outset, Robert Bostrom has a viable project.

The tidal dissipation mechanisms have remained speculative to this day, not least regarding those that possibly operate on the Earth's 'rocky' part. Despite the fact that the bodily and oceanic dissipations seem impossible to separate, the essence of Bostrom's book is to attempt to evaluate their possible tectonic consequences. Regrettably, the author leans entirely on purely speculative plate tectonic presumptions, including the highly problematic issue of solid-state mantle convection. Thus, by building on models and concepts that have continuously been strained by ad hoc repairs for more than 40 years, the author's endeavour has minimal chances of success. In fact, the book's opening citation, "There is no end, but addition" (T. S. Eliot), neatly summarizes the author's impossible task of finding his way through the plate tectonics deadlock.

In his review of past attempts at linking Earth rotation and tectonics, Robert Bostrom seems to be unfamiliar with important work by German and Austrian geoscientists around the turn of the 19th Century. Thus, by integrating palaeoclimatology and geophysics, the old notion of True Polar Wander had already been substantiated at that time. Without having actually read the book, Bostrom, in passing, refers to the 2nd edition of A. Damian Kreichgauer's *Die Äquatorfrage in der Geologie* (1926), cited by Reginald Daly. However, already in the 1st edition of this book (Kreichgauer, 1902), a dynamic link between tectonics and the Earth's rotation was outlined in surprising detail. Kreichgauer (an Austrian geologist) also discovered the Pole-fleeing Force, i.e., the combined effect of the dynamics of Earth rotation and the principle of isostasy (Archimedes' principle of floatation). Kreichgauer found that the equator-ward force of crustal motion, directed away from the poles, would have produced fold belts aligned along time-equivalent equators, while a second set of tectonomagmatic belts would have evolved in meridional settings. In other words, owing to the westward-directed inertia forces caused by planetary rotation, the second set of tectonic zones was, according to Kreichgauer, oriented at steep angles to the corresponding palaeoequator. By the way, speaking of Earth rotation and inertia-triggered tectonic effects, how can Robert Bostrom write a whole book about the subject without including the latitude-dependent Coriolis Effect? This is a serious omission. At the end of his book he states: "Lacking the Moon, Earth would lack also a mobile lithosphere". This is going a bit far, because the Coriolis Effect would have provided the observed pattern of crustal mobility, albeit with possible reduced tectonic vigour.

For Kreichgauer (and some of his contemporaries, including Alfred Wegener), the geological evidence for ancient climates (from rocks and fossils) seemed to favour the existence of True Polar Wander. For dynamical reasons, the rotational

axis has to be aligned along, or remain in the vicinity of, the maximum moment axis. Hence, the inevitable conclusion was therefore that internal axial shifts had intermittently taken place during the Earth's history. In other words, the apparent displacement of the poles over the surface was the result of the globe intermittently turning over relative to the ecliptic. In the Kreichgauer dynamic system, the required changes in the Earth's moment, causing polar wander, were brought about by the equator-ward and westward movements of the entire crust, without relative continental motion. The possibility that internal changes of mass could alter the maximum axis of moment, and thereby instigate True Polar Wander, was not a discussion topic at that time. Nevertheless, the associated changes in the relative position of the ancient palaeoequators, whatever their cause, would have given rise to tectonic belts in variable orientations across the globe. This was really a breakthrough in tectonics, but it was basically disregarded by Kreichgauer's contemporaries. However, Kreichgauer's understanding of the phenomenon of True Polar Wander was, a decade later, picked up by meteorologist Alfred Wegener, the father of continental drift—and the 'forefather' of plate tectonics.

Regarding possible driving mechanisms for tectonic processes, Wegener followed Kreichgauer and other German workers by adhering to the combined tidal torques from the Sun and Moon, the Pole-fleeing Force and the 'Coriolis Effect. Such forces are indeed directed westward and towards the equator, but the tectonic consequences of their latitude-dependent variation have not been given proper consideration in the past. Robert Bostrom, referring to the famous Stanley Keith Runcorn and his early work on continental palaeomagnetism, mentions the tendency of Southern Hemisphere continental blocks to have rotated counter-clockwise, as against corresponding clockwise rotations in the Northern Hemisphere. So what if the by now accepted continental mobility consists basically of relatively modest inertia-driven rotations *in situ*?—**not** of relative lateral translations of crustal segments, as invoked by traditional continental drift and plate tectonics? In fact, it can be easily demonstrated that, for the larger land masses, only a few tens of degrees of relative 'in situ' rotations are required to explain the palaeomagnetically-based longitudinal discrepancies of the classical polar wander paths. Such a change in basic presumptions leads to a fundamentally different global tectonic system, in addition to avoiding the multitude of grave conceptual problems that flourished in the wake of plate tectonics. And equally important, mantle convection is no longer needed as a driving force in tectonics (Storetvedt, 1990, 1992, 1997, 2003, 2005).

By combining the changing palaeoclimate pattern with the observed global tectonic system, for different geological epochs Kreichgauer was able to draw polar wander curves that show remarkable similarities to modern polar paths based on palaeomagnetic data. In terms of geodynamics, Kreichgauer was indeed far ahead of his time. But his books were not translated into English, so the scientific message did not reach out beyond German-speaking Europe. The simple pattern of global polar wander for the past 400 million years or so, based on palaeoclimatic data, later described by Wegener (1929), is depicted in Figure 1a.

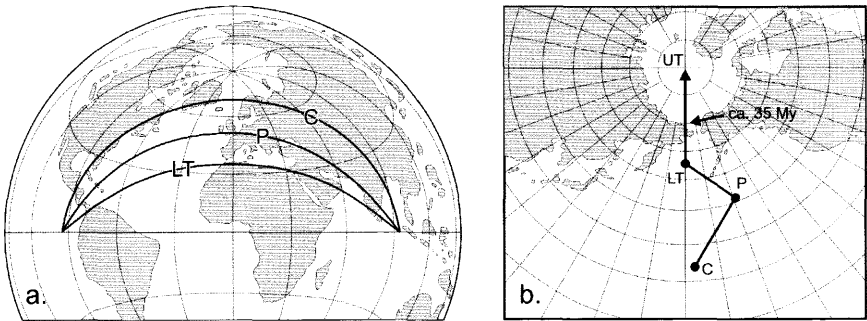


Fig. 1. Presentations of True Polar Wander (global) over the past few hundred million years, based on palaeoclimate observations—in terms of palaeoequators (a, Wegener [1929]), and palaeomagnetic evidence—in terms of the corresponding path of geographic poles (b, Storetvedt [1997, 2003]). Abbreviations: C, Carboniferous, ca. 350 million years; P, Permian, ca. 270 million years; LT, Lower Tertiary, 65–35 million years ago.

For comparison, the corresponding master curve for Global Polar Wander (Storetvedt, 1997), based on palaeomagnetic data, is shown in Figure 1b. For Robert Bostrom, and indeed for the majority of contemporary geophysicists, this simple picture of polar wandering seems to have gone unnoticed.

Considering the Northern Hemisphere palaeoclimate system, Wegener argued confidently that for the last few hundred million years the climate of the Arctic had gradually changed from tropical to polar, the most marked cooling having taken place only some 35 million years ago. A reverse trend, from polar to tropical, had taken place in southern Africa. Wegener ascribed this observational fact to the phenomenon of True Polar Wander—a spatial turning-over of the globe, in this case adding up to 70° of latitude. Indeed, if Wegener had avoided the distorted scientific vision that his preconceived idea of lateral continental drift undoubtedly inflicted upon him, he would have noticed that both the Arctic and Antarctic regions had experienced closely similar climatic trends over at least the last 400 million years. Unfortunately, Wegener ignored the growing evidence for ancient tropical to sub-tropical conditions in Antarctica that became available at the beginning of the 20th Century—which would have falsified his Pangaea concept, and indeed the very foundation of the later plate tectonics. Furthermore, with plate tectonics-constrained assumptions out of the way, many arguments in Bostrom's book would automatically become irrelevant.

In his discussion of True Polar Wander and its bodily tidal actions, the author seems to regard mantle convection as a naked truth. However, despite the fact that both the nature and scale of the postulated cells of slow mantle flow have become increasingly obscure, the idea of thermally-driven 'solid-state' flows in the mantle has had many lives. With the twin-concepts of plate tectonics and mantle convection taken for granted, the challenge has been to understand, evaluate and harmonize the whole range of observations with the constraints necessitated by different types of evidence and the different proposed models. In fact, conflicting

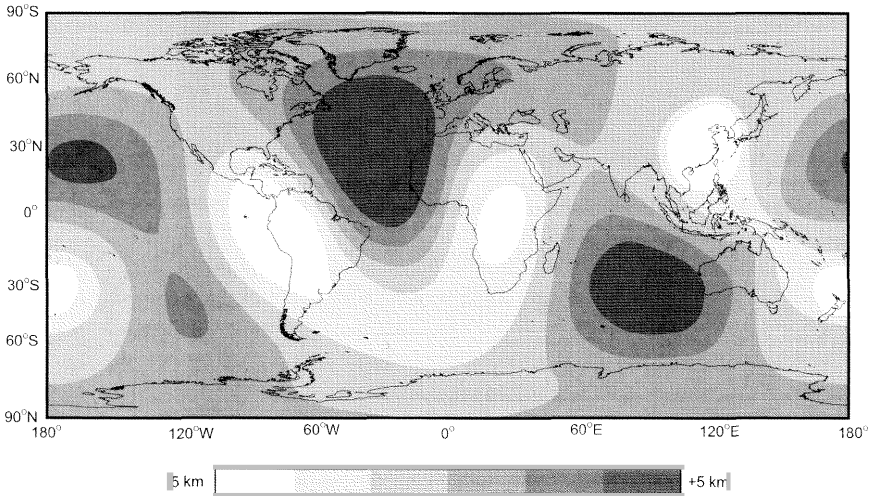


Fig 2 Diagram depicting inferred topography of the core-mantle boundary (CMB) zone, based on combined seismological cote phases (simplified after Morelli and Dziewonski [1987]) Note that upstanding regions of the CBM, when projected to the Earth's surface, correspond to the location of the world's oceans.

arguments have stimulated the formulation of a number of mutually incompatible 'systems' of thermal convection.

Compositional models of the mantle generally rest on assumptions of either one-layer (whole-mantle) or two-layer convection (upper and lower mantle separately), but ready solutions seem more remote than ever. In fact, if convection with plate tectonic time constants had been in operation, compositional heterogeneities could be expected to have been erased long ago. The pronounced inhomogeneity of the mantle, including the deep mantle roots of the continents, actually speaks against convection. It is indeed essential to ask whether thermal convection is at all feasible within a high-viscosity material like that comprising the mantle. What is more, the current choice of geotherm and other physical parameters for the deep Earth continues to be conditioned by the relevance of the convection hypothesis—for the sole reason that without it, plate tectonics would be without a driving mechanism. In other words, there is now every reason to look at the Earth with fresh eyes.

A range of studies suggest that the core is a good deal lighter than it traditionally has been considered to be and that it is not in equilibrium with the mantle. The presence of irregular core-mantle boundary topography (e.g., Morelli & Dziewonski, 1987) provides further evidence that this layer is a thermochemically active and heterogeneous zone. What if the core is in a state of degassing (of lighter material) and the core-mantle boundary represents the fundamental trigger of energy, eventually leading to the observed range of geodynamic and surface geological phenomena!

Figure 2 shows that, when projected onto the Earth's surface, upstanding

topographic regions of the core-mantle boundary zone correspond to deep oceanic depressions. This may indicate that processes at the outer core and/or the core-mantle boundary layer release energy as well as buoyant masses that on the surface of the Earth lead to formation of oceanic depressions. This is additional information arguing against classical continental drift and seafloor spreading. The mobile crust, as demonstrated by palaeomagnetism and space geodesy, then reduces to a kind of 'cogwheel' arrangement—a system of continental blocks undergoing variable rotations 'in situ'.

The relationship between the 'morphology' of the core-mantle boundary layer and the gross surface topography indeed argues against classical continental drift (Wegener's mobile crust)—the theoretical forerunner of plate tectonics. Instead, the early Earth may have had a pan-global continental crust, having gradually—through vertical mass transfer from the deep, in association with mineralogical changes en route (see, for example, Storetvedt, 2003)—turned the cover layer into its present mosaic of continental and oceanic regions. The mass reorganization associated with such a degassing Earth inevitably would have altered the internal distribution of matter, causing changes in both spin velocity and intermittent events of True Polar Wander. These changes in planetary rotation are most likely the triggers of geodynamic phenomena—the motor behind the range of surface geological processes. And not least, the mass of plate tectonics—inflicted artificiality, in present-day interpretations, will disappear.

Robert Bostrom acknowledges the moral support of the legendary Keith Runcorn, who, "more than any of his contemporaries (...) perceived the central importance of establishing the existence or otherwise of convection in the Earth's mantle; he identified, furthermore, means of observing the resulting surface-displacement: palomagnetism".

For decades Keith Runcorn was indeed very influential, and during the late 50s and 60s he turned his Newcastle Physics Department into a leading geosciences centre—the uncontested Mecca in palaeomagnetism and global geophysics at the time. Runcorn had an enquiring mind that never rested with 'finished' solutions. Thus, despite his crucial role in establishing widespread interest in the twin-concepts of continental drift and mantle convection, Keith became increasingly worried about the growing bewildered state in global geology and geophysics. Actually, at the end of his life he seems to have opted out of plate tectonics altogether. I would like to take this opportunity to give some of my personal recollections of Keith Runcorn's shift of attitude in global geophysics.

The close contact between the Physics Department in Newcastle and the Geophysical Institute in Bergen lasted for more than 30 years. But despite his many visits to Bergen, and our frequent encounters elsewhere, it took many years before I really had deeper scientific communication with Keith. He always wanted my opinion on matters that preoccupied his own mind, but when it came to my own scientific problems, and not least my doubts about the validity of some fundamental assumptions in global geophysics, he was for many years surprisingly silent. When I pressed him on these matters he mostly looked thoughtfully into the air, often smiling disarmingly and saying nothing, but

occasionally he would comment: "Yes, you may be right"! It was through a growing interest in history and philosophy of science on the part of both of us that I eventually developed a closer personal contact with Keith. This new situation came very suddenly, in early February 1980, following a colloquium on the evolution of the Central Atlantic that I gave in his department. At that time I had become very critical of many aspects of conventional continental drift and plate tectonics, so it was not surprising that my conclusions were at variance with the ruling dogma of the day. Something in my talk must have triggered Keith's curiosity, because from that time on I never had problems getting his attention. The following day he invited me to go with him to Durham, where he was going to give an evening lecture in the Department of Earth Sciences. In his car we discussed some of the grave problems facing plate tectonics, and I found him much more open about these matters than he had been before. Then he switched to the more philosophical aspects, asking me what kind of response I got from colleagues when I confronted them with my doubts about the established views. When I told him about all the non-scientific high-blood pressure/high-temperature reactions I had experienced by then, he turned to me smiling and said that that kind of behaviour was unfortunately very common but extremely unscientific. He then shared with me a number of amusing stories from his own repertoire, notably from the late 50s and early 60s, when he travelled widely in Europe and North America trying to convince the geosciences community about the reality of continental drift. Keith was extremely open-minded that evening, and we had a most enjoyable time together.

During the 1988–89 academic year I had a sabbatical leave at Keith's Newcastle Physics Department. During the daily coffee breaks the discussion topic was frequently the many intriguing aspects of global geophysics, not least the shortcomings of plate tectonics. One day, during a morning coffee in the Physics Penthouse, Keith said to members of the geophysics group present: "Well, we must take Karsten's criticisms of plate tectonics very seriously. Just remember all the observations we could never satisfactorily explain"! And he added, "I, for one, have often wondered, when I see the rotating globe just before the BBC News, why the Pacific Ocean is of roughly circular shape and covers about fifty per cent of the Earth's surface"! I was very impressed by such an honest and open-minded attitude of a man who for many years had been the *primus motor* in the process leading to the final acceptance of continental drift/plate tectonics, a theory which I now argued had to be denounced.

During the 1990 Copenhagen Assembly of the European Geophysical Society (EGS), Keith and I had longer discussions on the possible close link between Earth rotation and crustal deformation (tectonics). He was particularly interested in hearing my views about the link between the Coriolis Effect and the continental motion system—how relatively modest inertia-driven individual rotations of the larger continental blocks, around their 'centroids', could account for the dispersal of polar wander paths. The consequences of my new geophysical thinking would be devastating for plate tectonics, Keith admitted. During his subsequent visit to Bergen, in February 1992, he strongly encouraged me to continue my battle against

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the dogmatic attitude of the geoscientific establishment. He maintained that the geologists had made a complete mess of global tectonics and that the Earth sciences were in need of a brand new physical framework.

After his tragic death in December 1995, the Executive Director of EGS, Arne Richter, called me to discuss the memorial program for Keith at the forthcoming EGS Assembly at The Hague in April 1996. Arne Richter held that Runcom's growing disregard for plate tectonics was no longer a secret, not least in EGS circles. Keith's crucial role in breaking the ground for the modern drift-inspired global tectonics and his later lukewarm enthusiasm for these seemingly popular ideas were important facts for the history of science and therefore had to become publicly known. Consequently, in a couple of talks at The Hague EGS Assembly I unveiled some of Keith's criticism of plate tectonics—an exposition that created some stir in certain circles.

Bostrom's book does not pretend to give final answers. The author touches on many important and interesting aspects in global geology and geophysics, but with plate tectonics as the rigid frame for the mass of attempted interpretations and discussions, it is impossible to see a connecting phenomenological link in his analysis. I think the author is fully aware of the non-conclusive nature of his book, a situation for which the ingrained plate tectonics thinking is largely responsible. Nevertheless, despite my overall critical remarks, the author makes a number of interesting geophysical, historical and philosophical points. I therefore regard my copy a useful reference text.

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