

## Perspectives and Retrospectives

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# Introduction to the publication of the extended outline of Jason Morgan's April 17, 1967 American Geophysical Union Paper on "Rises, Trenches, Great Faults and Crustal Blocks"

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(Received by publisher July 31, 1990)

It is often stated that the spherical plate tectonic model was independently and simultaneously developed by Mc Kenzie and Parker in their paper published in *Nature* in November 1967 and Morgan in his paper published in *Journal of Geophysical Research* in March 1968 (e.g. Cox, 1973). Mc Kenzie and Parker thus have publication anteriority, although the first submission of Morgan's paper occurred on August 30, 1967 whereas the paper of Mc Kenzie and Parker was submitted about two weeks later. Yet, Jason Morgan had presented a first version of his paper entitled "Rises, trenches, great faults and crustal blocks" at the American Geophysical Union (AGU) meeting in Washington D.C. on April 17, 1967 to the packed audience of a "Sea Floor Spreading" special symposium presided by Fred Vine and H.W. Menard. This occurred two months before Dan Mc Kenzie, then at Scripps Institution of Oceanography, began working on the same model (D.P. Mc Kenzie, pers. commun., 1983). The reason why the presentation by Jason Morgan at the AGU meeting has not established his priority is that the talk he made did not correspond to the abstract he had sent. Consequently, there is no written record of this communication there.

Actually, Jason Morgan had written an eleven page extended outline of his presentation, including the nine figures illustrating his talk. This short paper was sent to about ten people immediately after the meeting. I was among these ten people.

In addition to myself, Morgan (pers. commun., July 1987) does not remember all the addresses of his paper. He writes "I am quite sure I gave copies to Bill Menard (at Scripps) and Tuzo Wilson (at Toronto) and, I am fairly sure, to Lynn Sykes (at Lamont), Carl Bowin (and/or Joe Philipps, at Woods Hole) and Fred Vine (at Princeton). I might have sent one to Jerry Van Andel as I used magnetic profiles from the Circe cruise". Unfortunately, Morgan lost his own copy and none of those who received it have yet made it available to the scientific community. I thought I had also lost my own copy. Yet, during a recent move of my office, I found it. I publish it here with the agreement of J. Morgan, thus revealing the exact substance of his presentation at the AGU. I also found a preprint of the paper he submitted to *Journal of Geophysical Research* later on August 30, 1967, which was later revised and accepted on November 30, 1967.

According to J. Morgan (pers. commun., July 1987), "this short description of the main ideas in plate tectonics was written the week before the AGU. The last two pages were written and reproduced the night before the meeting" (as indicated by the different quality of reproduction of these two pages). Morgan writes "I was in Guyot Hall (in Princeton University) until 2 or 3 A.M. and we were to leave for Washington at dawn."

Note that the extended outline has the same title as the paper later published in the *Journal of*

*Geophysical Research (JGR)*. Eight, out of the nine figures, will later be included in his JGR paper as well as most of the text. In particular, the first two paragraphs make most of the substance of the abstract of the March 1968 paper.

A comparison of this April 1967 version to the published March 1968 paper shows that the 1967 version forms the nucleus of the 1968 one. This nucleus consists of the definition of the model and a demonstration of its validity over the Mid-Atlantic Ridge between Africa and America. The Pacific was only briefly alluded to in figs. 3 and 8 and was not discussed in any quantitative way. On the other hand, the 1968 version includes a brief discussion of the twenty blocks model (shown in fig. 1), a definition of the Pacific/America motion based on the strike-slip faults and fault-plane mechanisms along the North American continent western border and finally a determination of the Pacific/Antarctica motion which is used to estimate the motion of the Antarctica block relative to Africa by assuming closure of the Africa–America–Pacific–Antarctica–Africa circuit. This published version was accepted in revised form on November 30, 1967, seven months after the extended outline had been circulated. Although it is more elaborate, it adds nothing to the spherical plate tectonic model, as defined in this early April 1967 version.

On the basis of this document, it seems extraordinary that, in this hall packed with the best geophysicists and geologists in the United States, nobody got excited or even interested by the implications of Morgan's ideas. They were too new, too different from anything which had been done. Even among those who received the extended outline and had time to digest these new concepts, I apparently was the only one to have considered it sufficiently important to drop everything else and start working along these new lines. Thus, as I have written elsewhere (Le Pichon, 1984, 1986), the source of my June 1968 paper was the 1967 extended outline of Morgan. I had attended the meeting in Washington and had listened to Morgan's paper but had not been impressed by it. However, when reading the extended outline, I got very excited and started working along these lines. Part of my work got incorporated in the Heirtzler

et al. (1968) paper. I first extended Morgan's kinematic analysis of the Africa/America accreting boundary to the Antarctica/Pacific, the Eurasia/America and the Africa/India (actually the Africa/Arabia) accreting boundaries to test his concept. Based on a suggestion of J. Heirtzler, I used an oblique Mercator projection to test the geometry of opening of these accreting plate boundaries. I also devised numerical search methods to define the Eulerian vectors of rotation. By the end of August 1967, this first part of my work was completed. At this time, neither Morgan nor myself had any knowledge that D. Mc Kenzie was working at Scripps on his "paving stone theory" and J. Morgan had no knowledge either that I was exploiting his model.

J. Morgan had spent the months of July and August in Woods Hole where he finished the version of his paper which was submitted to JGR on August 30. This version is close to the revised published version although its discussion of the Africa–America–Pacific–Antarctica–Africa circuit was not correct because of an error in the determination of the Pacific–Antarctica rotation vector which I later pointed out to him. It does not contain figs. 15 and 16 of the final version which were added after the Pacific–Antarctica determination was reworked. Morgan presented his paper in a seminar in Woods Hole during the month of August. He then came back there during the first week of September to attend a two-day conference. Also attending the conference were K. Deffeyes from Princeton, W. Pitman, J. Mudie from Scripps, myself and possibly J. Heirtzler from Lamont and quite a few others. Morgan presented his paper. I also presented my kinematic analysis, including the oblique Mercator plots. It was the first time that Morgan heard about my work. From then on, we freely exchanged data and documents. This helped Morgan to rework his Pacific–Antarctica rotation vector and the corresponding Africa–America–Pacific–Antarctica–Africa circuit. But this was also a great help for me because at that time I was attempting to obtain the first world kinematic model. The America/Pacific Eulerian vector obtained by Morgan gave me the possibility to close my six-plate model which, as pointed out to me at that

time by J. Morgan, accounted for most of the world seismicity. Later, Isacks et al. (1968) would use my six plates model to show that it is indeed broadly compatible with seismological data. Even now, it is difficult for me to forget the extraordinary excitement which was mine the day I realized that my six-plate model worked and that it could indeed account in a first approximation for the broad geodynamic pattern. This success encouraged me to move to the reconstructions of past configurations of the oceans, which I did in October for both the Atlantic and Indian Oceans. I felt I had plenty of time to complete my paper as I had decided to leave priority to the paper of Jason Morgan and later asked that my paper not be published in the same issue of JGR.

It was sometime in late September that both J. Morgan and myself received preprints of the Mc Kenzie and Parker paper just submitted to *Nature*. This is how we discovered that Mc Kenzie had been working on the same subject. Thus, the relationship between my paper and the paper of J. Morgan is quite obvious but both papers were written completely independently of the work of Mc Kenzie.

Mc Kenzie had arrived in Scripps Institution of Oceanography in June 1967 (Menard, 1986). Cox (1973) wrote that "in June, 1967" he got the idea of using rigid-body rotations to describe plate motions while re-reading the paper by Bullard, Everett and Smith (1965) on fitting the continents together... Robert Parker had just completed a general computer program called SUPERMAP for plotting worldwide geophysical data using any conceivable projection. Parker introduced the idea of using a Mercator projection in plate tectonics...". As noted above, I independently started using oblique Mercator projection in late May-early June 1967 and presented the first oblique Mercator maps with the Eulerian pole of rotation as pole of projection at the early September Woods Hole meeting.

In a letter written to me on October 11, 1983, D. Mc Kenzie explained in the following way the relationship between his paper and the paper of Morgan. "I was at the 1967 AGU meeting and attended the session in which Morgan spoke, up until the time he did so. But I had read the

abstract... and thought I would gain nothing from sitting through the talk and arguments and left to go elsewhere. The paper generated little general interest, and I did not hear about it until after Bob (Parker) and I had sent off our paper to *Nature*. When I did, I tried to delay publication, but the editor refused, saying that the issue had been made up." "I did not know until I read your paper (a preprint of Le Pichon, 1984) that Jason had sent you a preprint so early. The first I knew of what he had done was a brief account from John Mudie when he returned from Woods Hole. By this time, Bob and I had already produced the Mercator maps of the slip vectors, and John's report acted as an incentive to get something written. I had talked a great deal to Bill Menard about plate tectonics and had convinced him that it worked for the Pacific. JGR sent him Jason (Morgan)'s paper to referee and, I suspect because of our conversations, he was very critical of it when he showed it to me. I asked him what I should do and he said to go ahead and publish, which we did as everyone knows. When I came to Lamont and Princeton in the autumn of 1967 and discovered what had happened I felt very embarrassed and it was then that I tried to hold the *Nature* paper."

Thus, Mc Kenzie heard about Morgan's work from a brief report of the early September Woods Hole meeting and then, presumably immediately after, from W. Menard who received Morgan's paper to review also in early September. It is then that Mc Kenzie decided to immediately write his short *Nature* paper, probably feeling that his approach (using the horizontal projections of the slip lines of earthquake fault plane solutions to determine graphically the position of the pole of rotation with oblique Mercator plots) was sufficiently different to justify doing so.

In his recent book "*The Ocean of Truth*", Menard (1986) confirmed that he received the early extended outline. He wrote "Jason Morgan sent me a preprint of his manuscript in its early draft, probably in the late Spring of 1967". Menard must have had this extended outline available to him when he wrote his 1966 book as he did quote in the book the first sentences of this early preprint. Menard added: "The manuscript certainly

circulated among my students, and we discussed it. The original draft, however, was difficult to fathom and it did not have the impact of the final publication". Yet, as discussed earlier, the plate tectonic concepts were clearly presented in this early draft, here published, which was not significantly different from the later 1968 version, in spite of what Menard wrote.

Actually, it is clear that the concepts were too new and appeared irrelevant both to Menard and his students. Menard, who had co-chaired the AGU session in which Jason Morgan presented his paper, wrote in his 1986 book: "I not only did not remember hearing Jason's famous talk, I didn't remember presiding over the session".

Finally, Menard (1986) stated: "I believe I also reviewed the paper for an editor". This can only refer to the August 30 version submitted to JGR which he presumably found at his return from the Nova expedition sometime after September 12, according to the information he gave in his book. At this time, as mentioned by Mc Kenzie in his letter to me, "he was very critical of it".

It is astonishing that Mc Kenzie twice so nearly missed the opportunity to learn about Morgan's model. The first occasion was when he left the room just before Morgan's talk on April 17. The second occasion was when Bill Menard, who had received the extended outline of the April 17 communication in late April, failed to mention it to Mc Kenzie although they "talked a great deal" together "about plate tectonics" (quote from the letter of Mc Kenzie) and although Morgan's "manuscript had circulated among Menard's "students" and had been "discussed" by them (quote from the book of Menard). But the approach followed by Mc Kenzie is sufficiently different from the one followed by Morgan to lend credibility to his story.

To me, the most surprising part of it is that Mc Kenzie confined himself to discussing the plate kinematics of the Pacific–America plate boundary based on earthquake fault plane solutions and did not consider the kinematics of the Atlantic ridge. In the equatorial Atlantic, good data on transform faults (Heezen and Tharp, 1965) and on earthquake fault plane solutions (Sykes, 1967) were available and the opening of the Atlantic Ocean is

the subject of the fit of Bullard et al. (1965) which gave the initial intuition to Mc Kenzie.

Morgan's paper was delayed three months by the review of W. Menard and could have been published in December 1967 instead of March 1968 if Menard had immediately accepted it as T. Wilson and J. Oliver later did for my own paper. It could also have been published in abbreviated form in June or July, had Morgan then decided to publish a cleaned-up version of his extended outline. But I do not believe that much would have changed in the later evolution of science. The major impact on the geologic community followed the publication of Isacks et al. (1968) "Seismology and the new global tectonics" who presented, on the basis of my kinematic model, a set of geophysical data compatible with a global model. What was needed to advance further was a comprehensive global model and the Lamont data had first to be published and released, which was done by Heirtzler et al., who incorporated part of my plate kinematic analysis, Pitman et al., Dickson et al. and Le Pichon and Heirtzler in the March 1968 issue of JGR, accepted on October 30, 1967.

In conclusion, I believe that the document published here establishes that Jason Morgan had clearly defined the principles of the spherical plate model and had given the first demonstration of its validity over the Atlantic ridge at the American Geophysical Union meeting, in Washington, on April 17, 1967. It also helps to better define the filiation between Morgan's paper and my paper. My main contribution was to dare computing a simplified global kinematic model (six plates instead of twenty of Morgan) and to show that it could still be considered realistic. In other words, I demonstrated the validity of the plate tectonic concept for a global quantitative description of the tectonics of the Earth. I also did use the oblique Mercator test simultaneously and independently of Mc Kenzie and Parker (1967) and made the first plate kinematic reconstructions of past oceans, pointing out the geodynamic implications of finite rotations around triple junctions.

On the other hand, the elegant discussion made by Mc Kenzie and Parker of the kinematics of the Pacific plate was original and first outlined in a correct way the significance of fault plane solu-

tions for plate kinematics. Their paper shows no evidence of any direct filiation from J. Morgan's paper, substantiating Mc Kenzie's claim that it was developed independently.

### Acknowledgements

I guess it is a little bit late to thank J. Morgan for sending me his extended outline in late April 1967! I wish to thank him and D. Mc Kenzie for helping me reconstruct what happened during these six exciting months.

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