

*Fifty years of Plate Tectonics
Afterthoughts of a witness*

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based on paper accepted for Tectonics

Tout est dit et l'on vient trop tard.

Everything has been said and we arrive too late.

La Bruyère (1696)

- I argue that in the mid-fifties, Earth Sciences entered a state of supercooling where the smallest input could lead to the immediate crystallization of new ideas.
- I mention the ignored contribution of a genial precursor: Boris Choubert. When the idea comes too early, it is ignored.
- I discuss another strange case. The discarded theory of the good model. In 1966, I proposed the first quantitative plate model and discarded it for the wrong reason.

The horses of Lake Ladoga of Curzio Malaparte



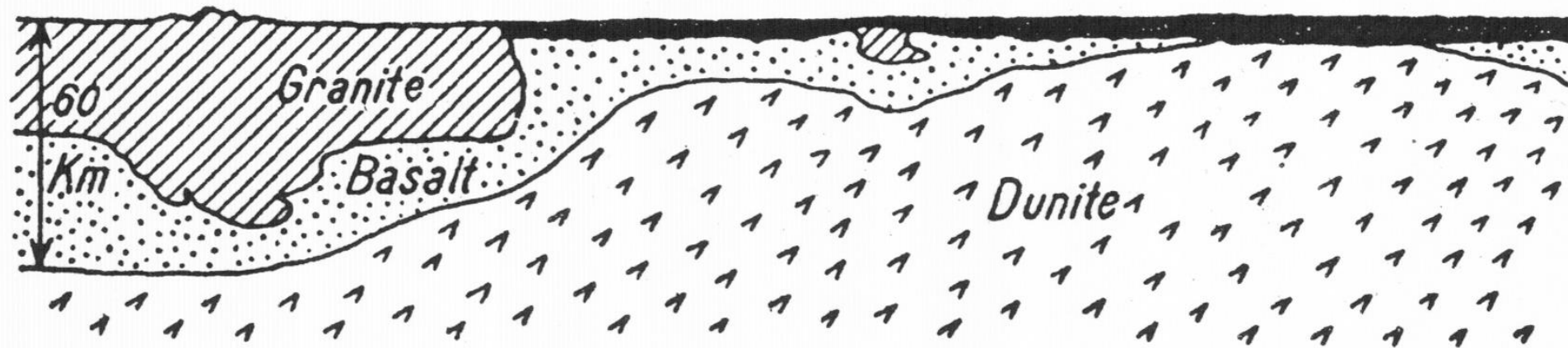
- **In the fifties and sixties, Earth Sciences were in a super cooled state.** Anything could lead to the solidification of the new paradigm any time, anywhere.
- Striking proofs of the existence of this supercooled state were the independent proposals of the corollary *Sea Floor Spreading (SFS) = Magnetic Anomalies* by Fred Vine and Lawrence Morley in 1963 and of the *Earth spherical plate kinematics* in 1967 by Jason Morgan and Dan Mc Kenzie.

Continental Drift was a major conceptual advance but did not lead to a Revolution. Why?

Continental drift was a paleogeographic model which was presented as a theory of the Earth.

A fatal ambiguity.

The model of Wegener consisted of a continent gliding on top of a ductile mantle

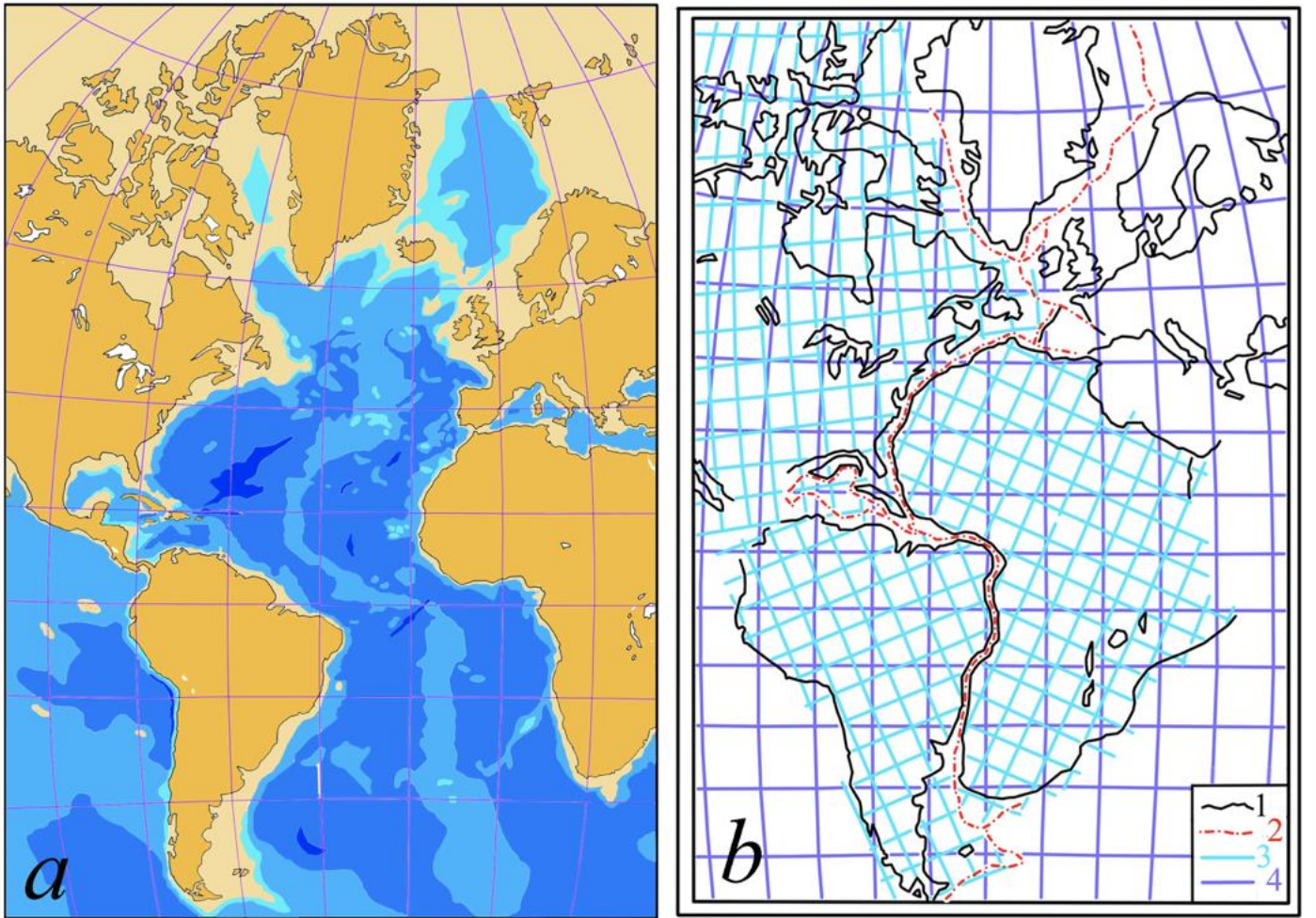


After Wegener (1929)

From Alfred Wegener to Tuzo Wilson, Boris Choubert, a precursor (1935!)

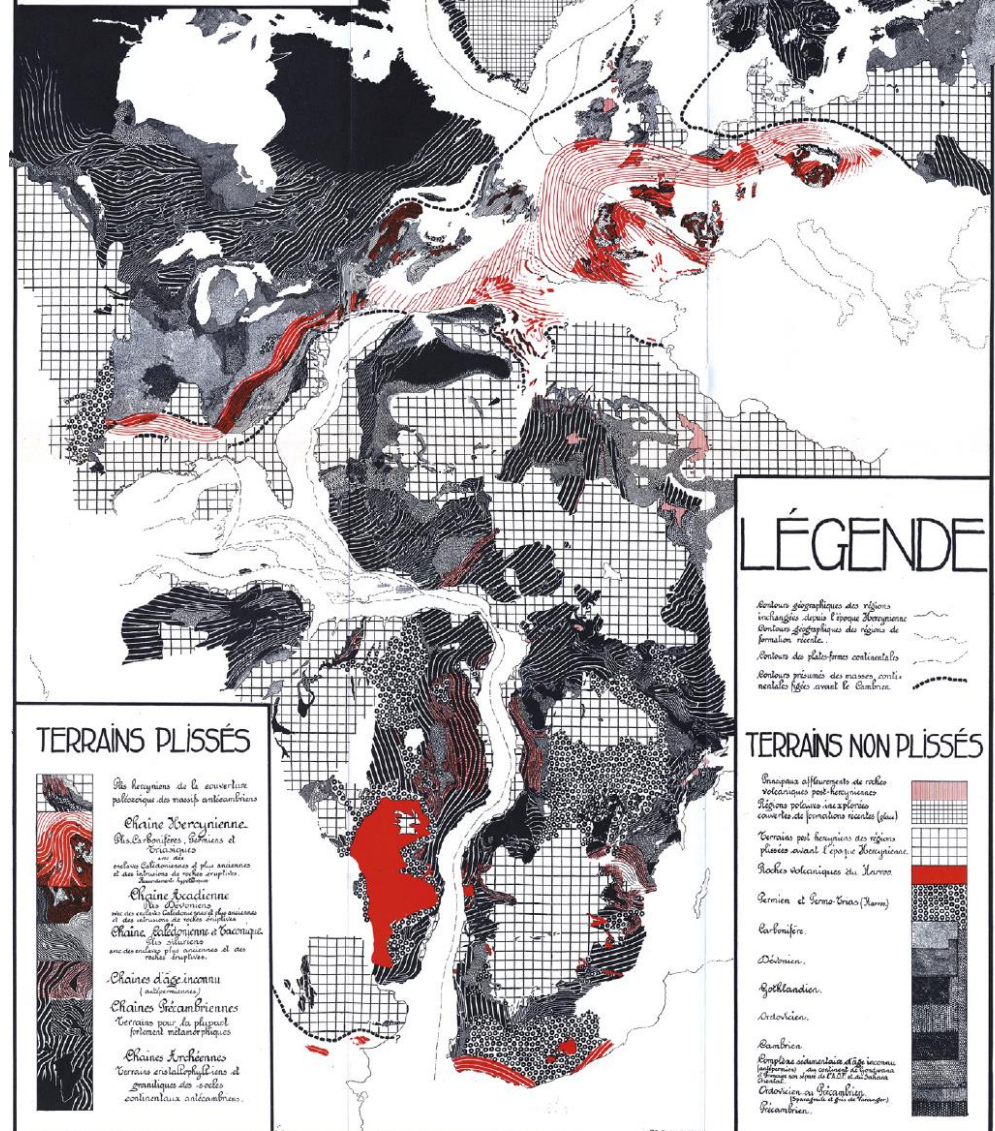
“The thesis of continental drift of A. Wegener, once received with significant favor, appears to be accepted by less and less scientists. I suggest that, instead of rejecting the whole theory as a block, one can save the essential principle of continental drift that is quite attractive because it enlightens problems that could not be solved by previous hypotheses. This is why I will attempt a thorough geologic verification that could either enable us to reject the theory or to show its explanatory value. The test I propose is whether continental drift gives a satisfactory explanation for the genesis of the Paleozoic and Precambrian mountain belts.”

The fit of Boris Choubert (1935)



Credit: Jacques Kornprobst

Relations
des
CONTINENTS ATLANTIQUES
À LA FIN DE
L'ÉPOQUE HERCYNIENNE
PAR
B. CHOUBERT.
ÉCHELLE 1/3000000



LÉGENDE

- Arrière géographiques des régions
inclinées dans l'époque Hercynienne
- Arrière géographiques des régions de
formation récente.
- Arrière des plate-formes continentales
- Arrière primaires des masses, conti-
nentales (type avant le Cambrien).

TERRAINS PLISSÉS

- Des terrains de la couverture
paléozoïque des massifs anticlinaliens
- Chaîne Hercynienne.**
Des chaînes, plissées et
cratériques
ou des chaînes de plus ancienne
et des chaînes de plus récente
formation.
- Chaîne Académique**
ou des chaînes de plus ancienne
et des chaînes de plus récente
formation.
- Chaîne Baltique et Scandinave.**
ou des chaînes de plus ancienne
et des chaînes de plus récente
formation.
- Chaînes d'âge inconnu**
(anté-hercyniennes)
- Chaînes ScandinaVIennes**
terrains pour la plupart
formés mésozoïques
- Chaînes Archéennes**
terrains cratéroplutoniques et
granitiques des orogènes
continentaux anté-hercyniens.

TERRAINS NON PLISSÉS

- Basaltes effluents de roches
volcaniques post-hercyniennes
- Séquences volcaniques des régions
couvertes de formations récentes (glac)
- Terrains post-hercyniens des régions
plissées avant l'époque Hercynienne.
- Basaltes volcaniques du Juras
- Permien et Trias (Russe)
- Carbonifère.
- Dévonien.
- Gotlandien.
- Artésien.
- Permien.
- Épaves sédimentaires d'âge inconnu
supérieurs, des continents et terranes
formés au type de l'États-Unis
Océaniques ou Cratériques
(type de l'États-Unis)
- Permien.

- The sea-floor spreading model came out of the simultaneous proposals by Bruce Heezen and Samuel Carey in 1955-1956 of symmetric formation of new ocean floor at mid-ocean ridge crests integrated in 1960 by Harry Hess in the Sea-Floor Spreading model.
- The global mid-ocean ridge system was the critical discovery by Maurice Ewing and Bruce Heezen in 1956 that directly led to the Sea Floor Spreading model of Harry Hess first presented in 1960. In 1959, the R.V.Vema was sent by Ewing to test the continuity of the ridge system using as a guide the earthquakes map of J.P. Rothé published in 1954. I was part of this discovery as a physical oceanography technician.

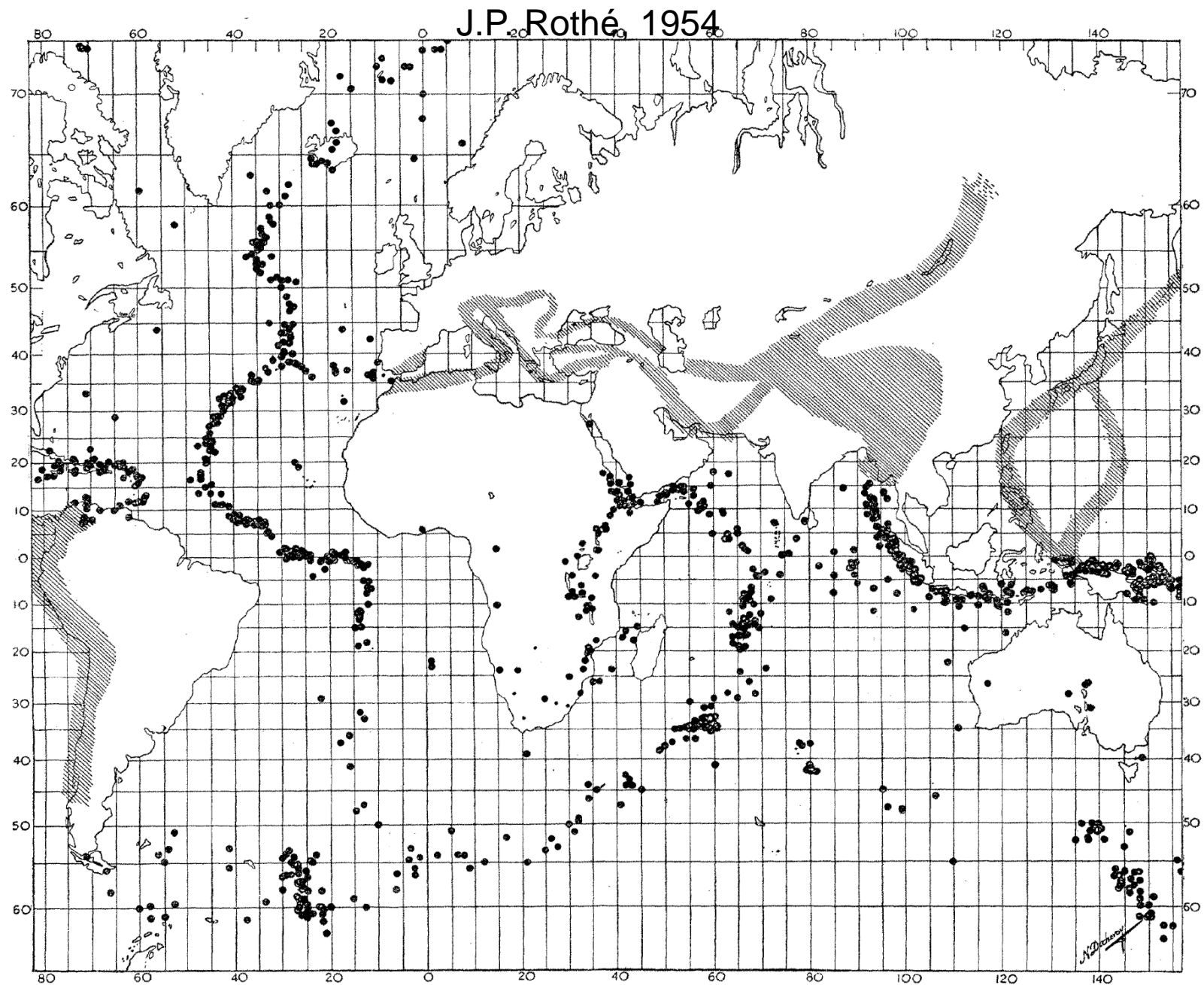


FIGURE 27. Epicentres des séismes de la zone médiane indo-atlantique et des régions voisines; les zones hachurées marquent l'emplacement des autres grandes zones séismiques.



On board Vema, 1963



Data: SIO, NOAA, NGA

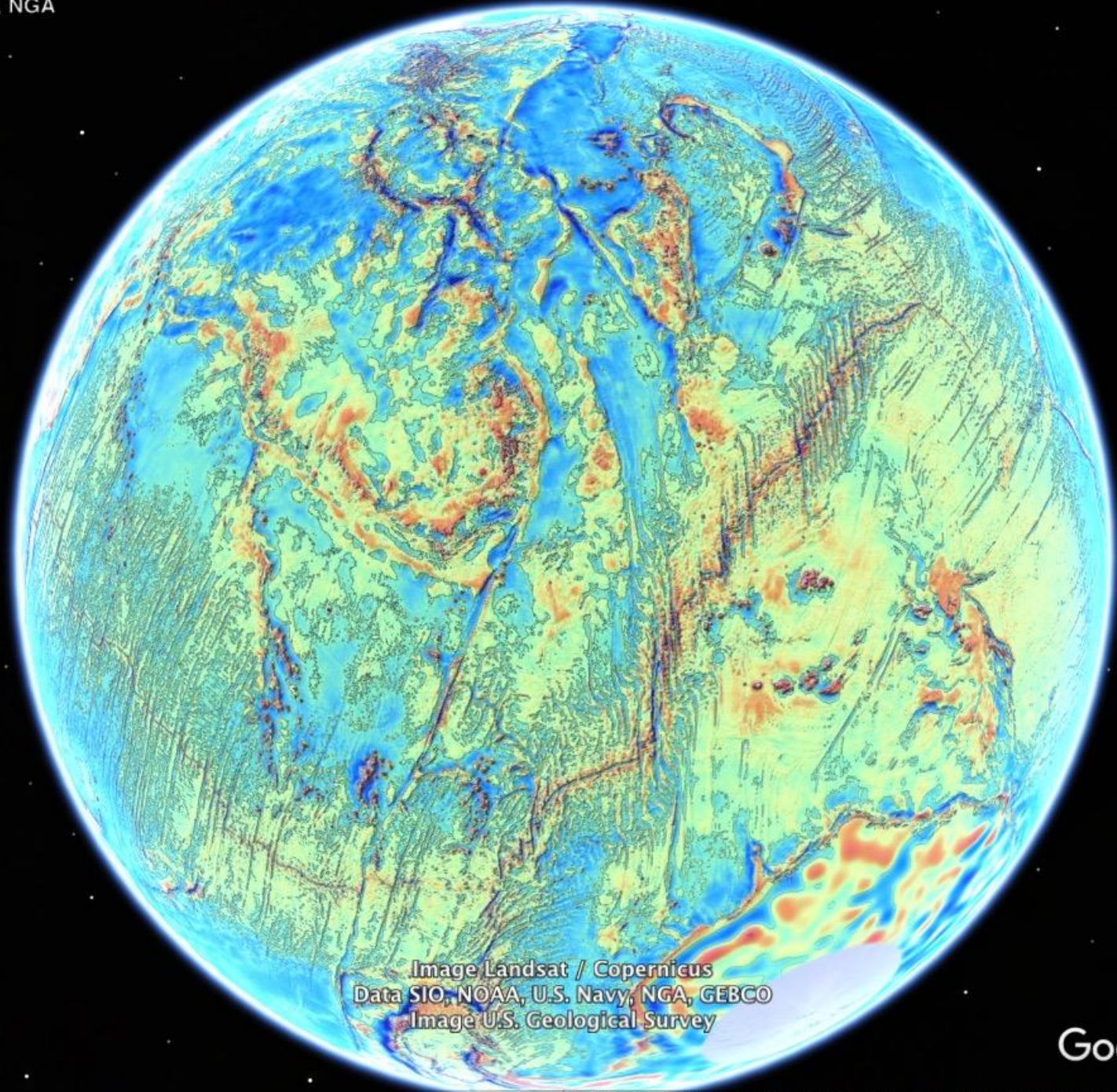


Image Landsat / Copernicus
Data SIO, NOAA, U.S. Navy, NGA, GEBCO
Image U.S. Geological Survey

Google Earth

Date des images satellite : 14/12/2015 37°55'43.33"S 28°27'00.47"E altitude 9939.38 km

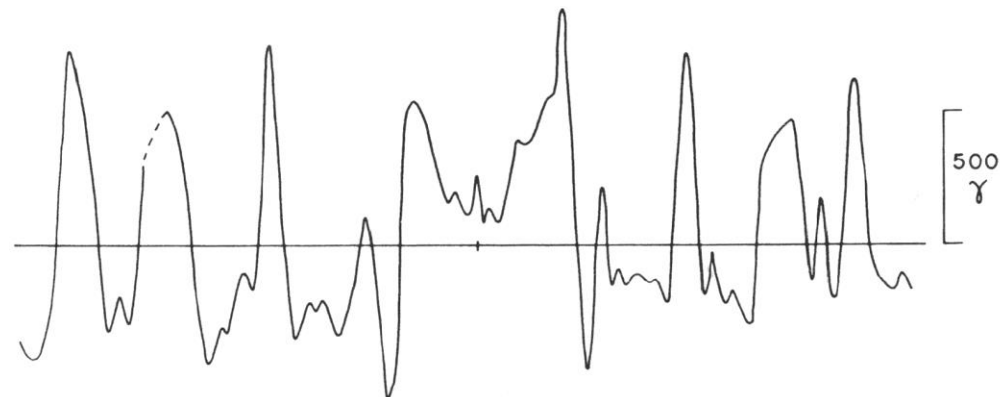
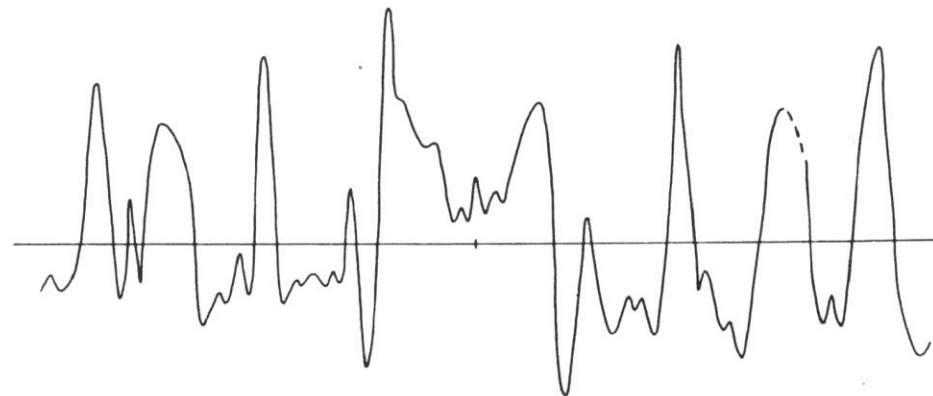
The Sea Floor Spreading model of Hess in 1960 had to be confirmed by its magnetic anomaly corollary *Sea Floor Spreading (SFS) = Magnetic Anomalies proposed* by Fred Vine and Lawrence Morley in 1963. But one had to wait the confirmation of the South Pacific profile of Walter Pitman in 1966 to establish it as a proven theory.

Yet, even in 1966, there were still major problems unsolved: we had to accept living without understanding why.

EAST PACIFIC RISE

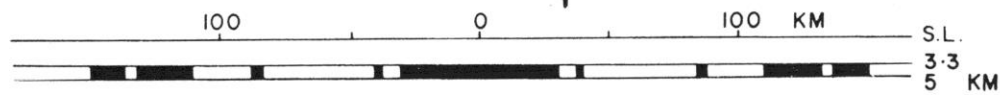
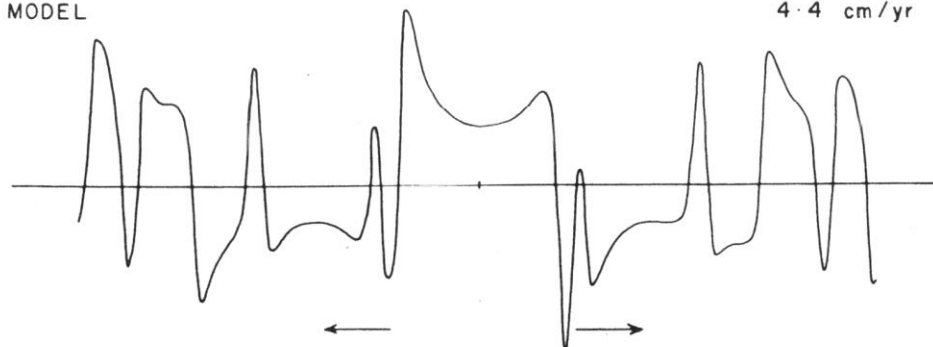
51° S

PROFILE REVERSED



MODEL

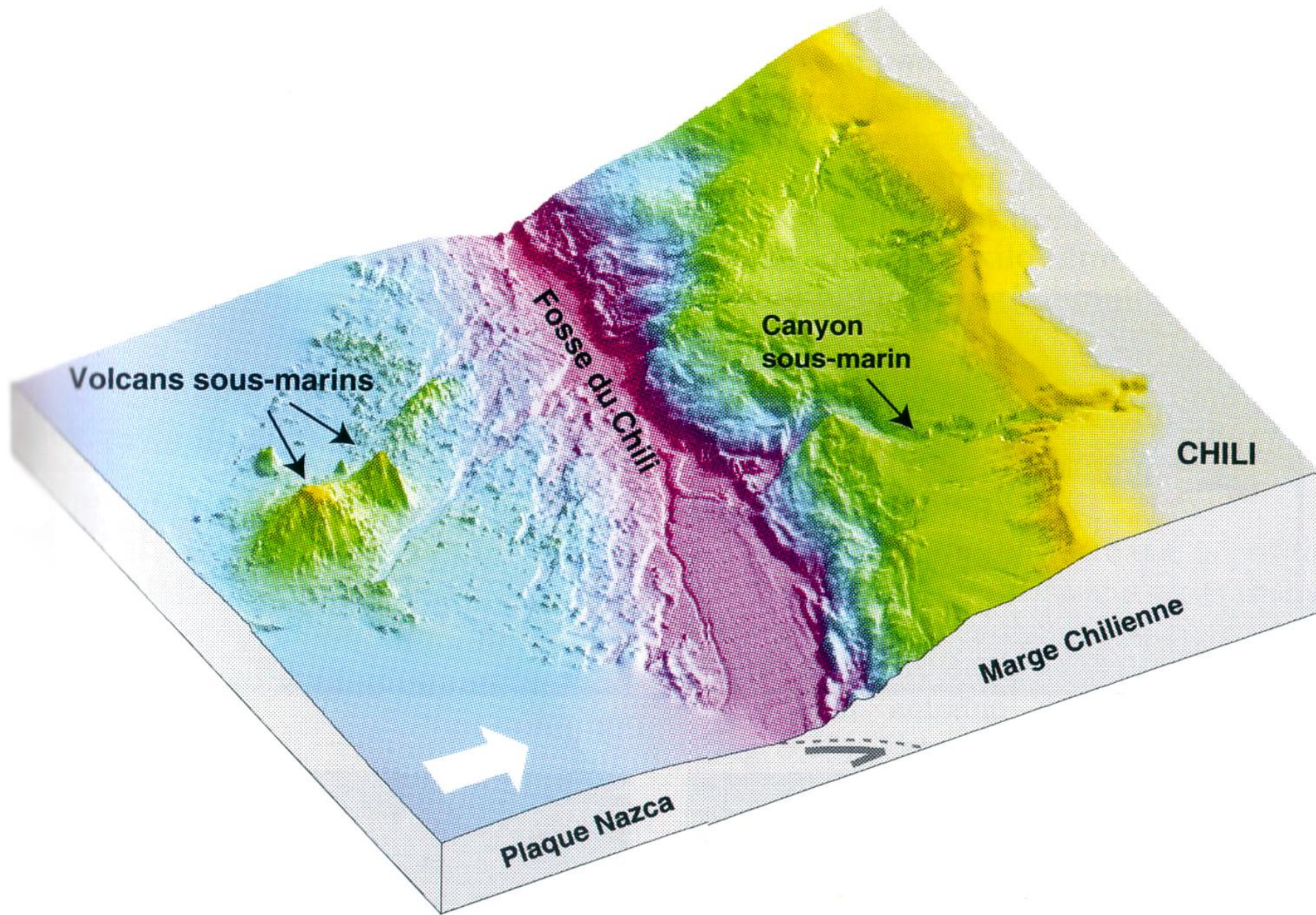
4.4 cm/yr



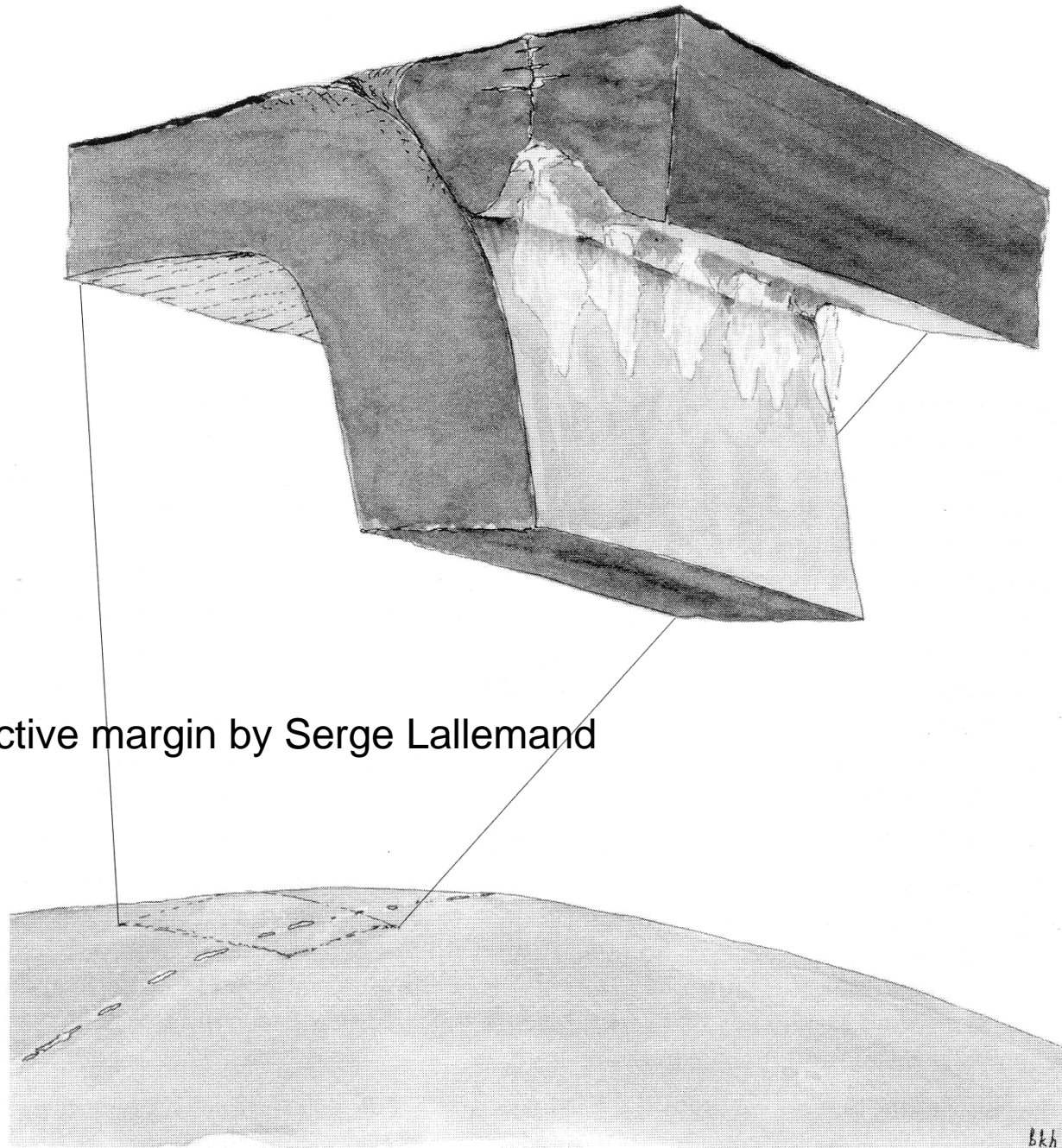
After Pitman (1966)

The subduction problem

**A convergence zone
in which the only known tectonic
indicators
at the surface at the time
were extensive**



Lallemand, 1999



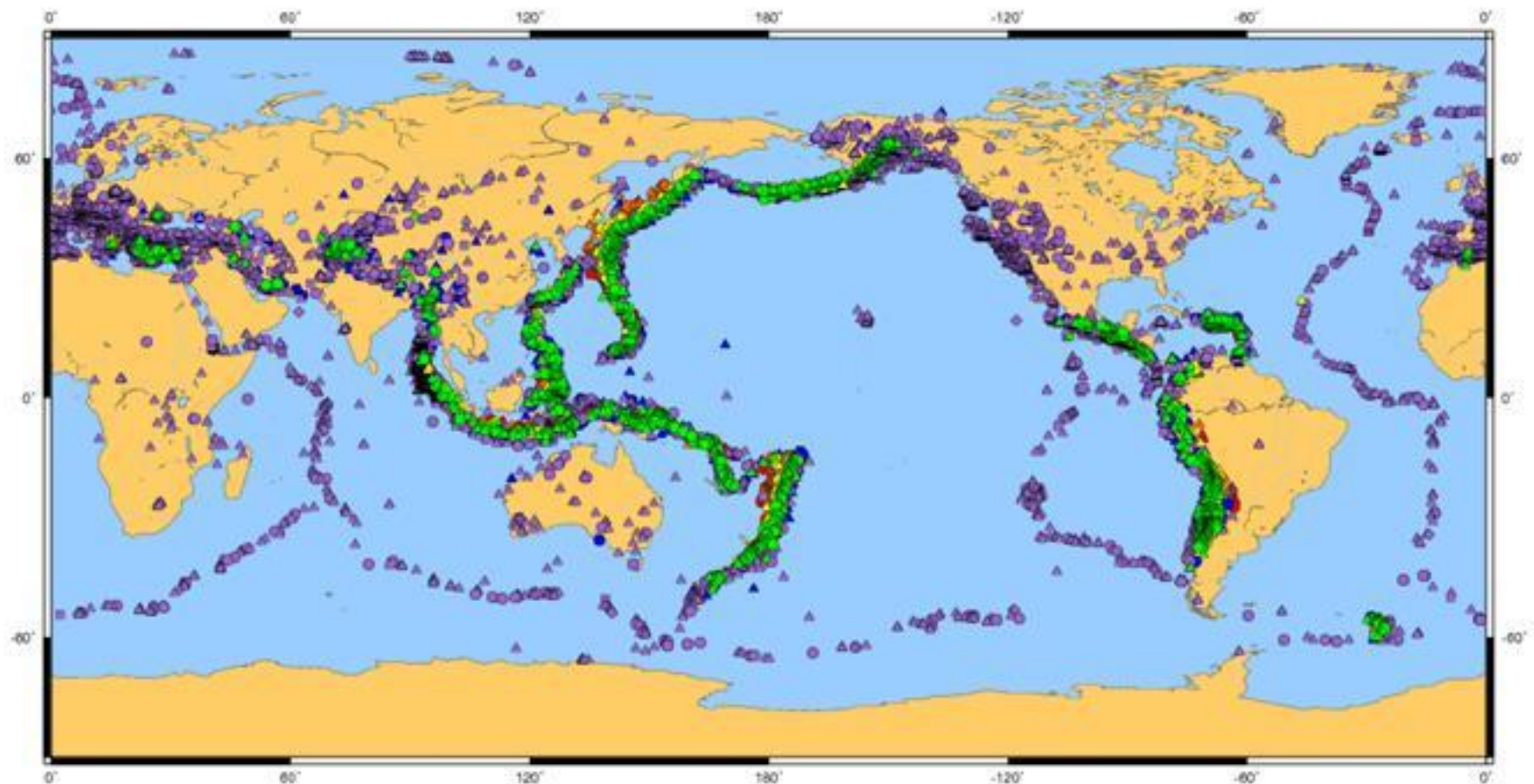
Cartoon of active margin by Serge Lallemand

Bkh 98

The kinematic problem

No one for one correspondence
between
extensional and converging zones

2004/10/10 to 2005/10/10



Legend:



7 (31840) 7.7-8 (38) 8-8.9 (7) 8-8.9 (3) >= 9 (8)

	7 (31840)	7.7-8 (38)	8-8.9 (7)	8-8.9 (3)	>= 9 (8)
FINGER	*	◆	◆	◆	◆
GED	*	■	■	■	■
WHP	*	▲	▲	▲	▲
WHP	*	●	●	●	●

**Why did I fail to adopt sea floor spreading
in our heat flow research in 1965
(Langseth, Le Pichon and Ewing, 1966)?
The failed energy test**

*Jean Francheteau and I both believed that
the model presented in the 1966 paper,
written by Langseth, yourself and Ewing,
provided the spark that set off the whole
Plate Tectonic revolution.*

John Sclater, March 9 2018

Langseth, Le Pichon, Ewing, 1966

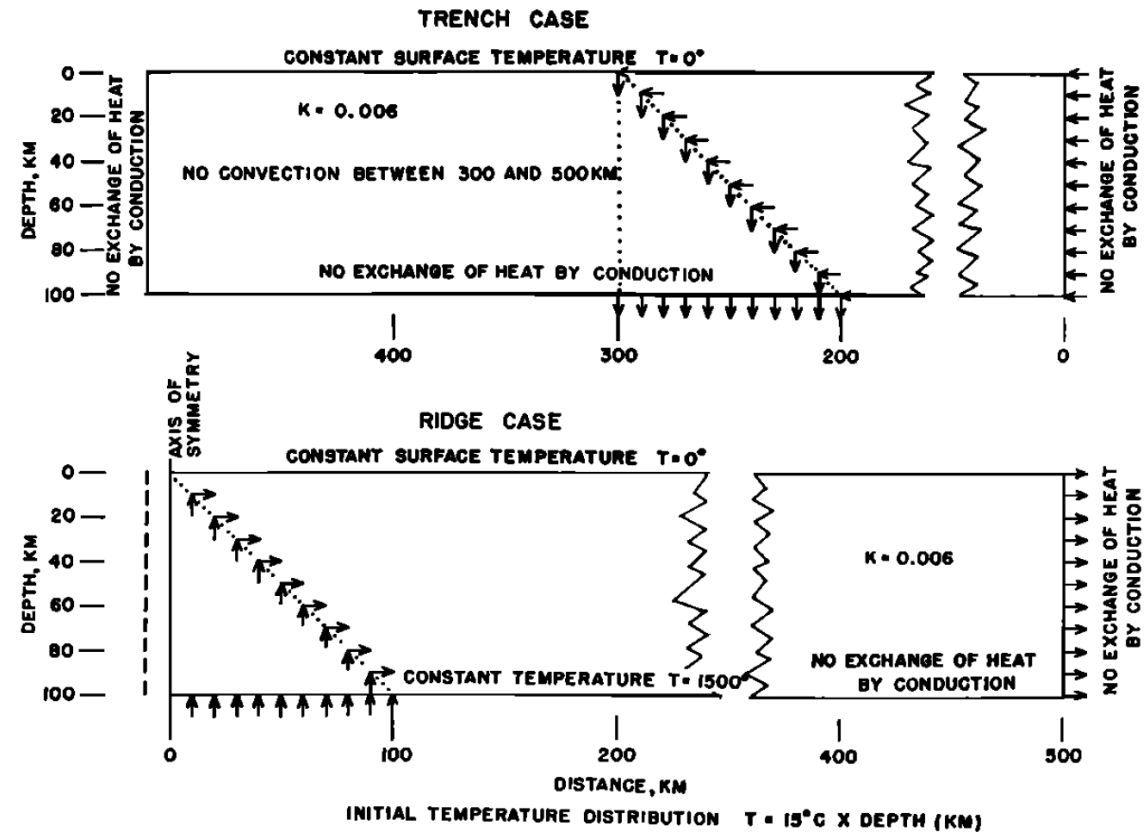


Fig. 15. Models used for numerical computations of distribution of temperature in the case of rising and sinking convection currents reaching the sea floor.

In SFS, the crest of the ridge always reaches the same height (provided that $V > 0.5$ cm/yr,) whereas the slopes of the flank depend on the velocity. (Langseth et al. 1966)

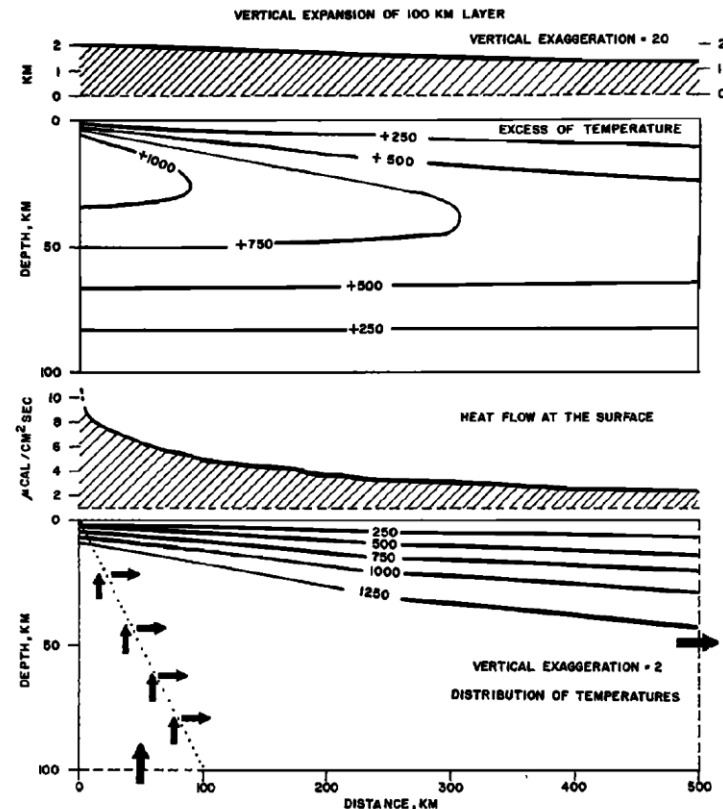


Fig. 16. Distribution of temperatures and heat flow over the ridge for a rate of spreading of 2 cm/yr. *Bottom*: The distribution of temperatures with depth versus distance from the axis of the ridge, with the corresponding heat flow. *Top*: The excess of temperature over the equilibrium temperature in the absence of convection currents and the corresponding vertical expansion.

Average HF expected for 1 cm/yr over 1000 km width 3.1 for 1 cm/yr vs 1.6 measured for MAR and 2.5 for EPR (Langseth et al. 1966)

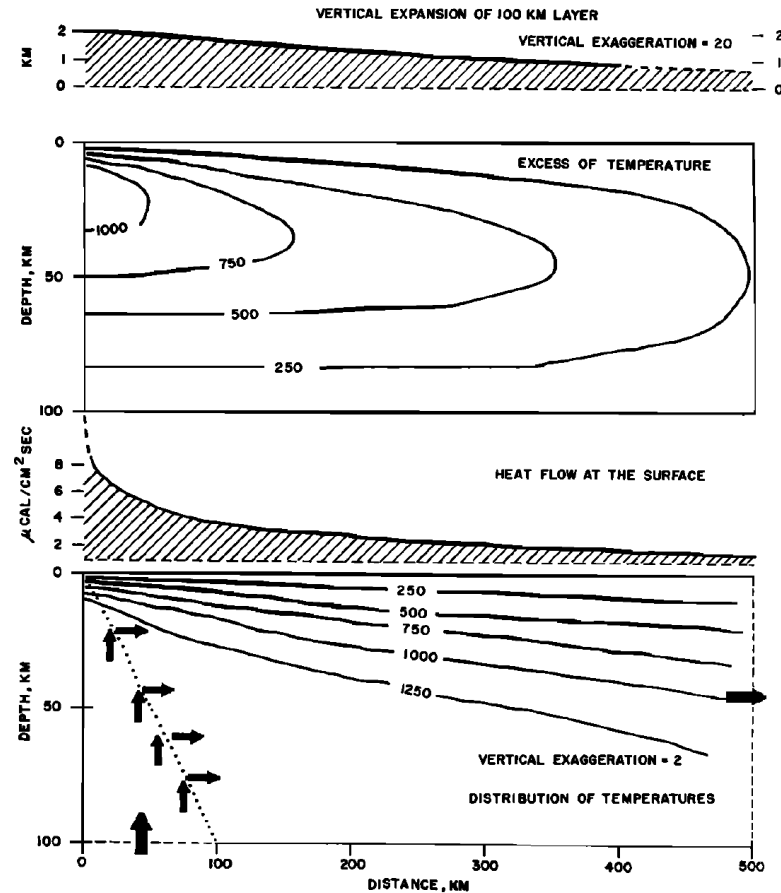


Fig. 17. Same information as Figure 16 for a rate of spreading of 1 cm/yr.

My thesis in Strasbourg on April 21 1966

My thesis concluded that SFS was not possible because of the failure of the energy test . (Langseth, Le Pichon and Ewing, J. Geophys. Res., 71, 1966)

Dan McKenzie one year later divided by three the temperature of the asthenosphere to obtain the proper heat flow. (McKenzie, J Geophys. Res., 72, 1967) This 550°C temperature was physically impossible and did not predict the proper decrease in topography. But it agreed with SFS which was then accepted.

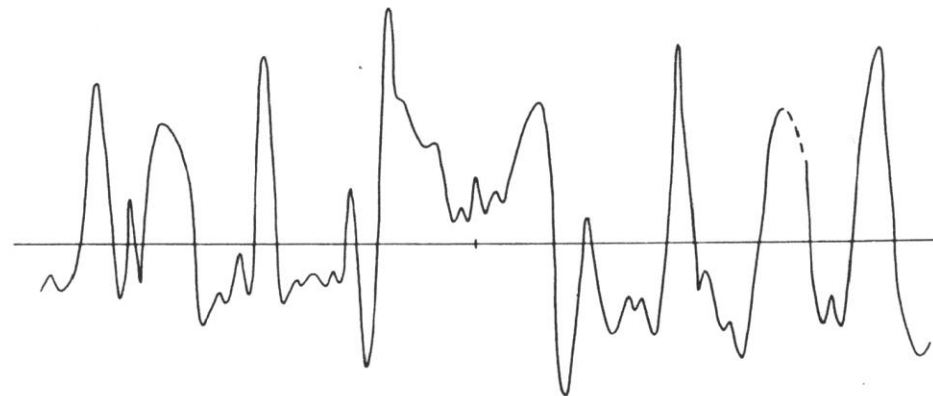
Crisis: my conversion to sea-floor spreading

Thus while I had defended the incompatibility of heat flow with sea floor spreading during my thesis in France in early 1966, I discovered at my return in Lamont on April 26 1966 the magic profile of Pitman. Sea-floor spreading imposed itself to me. Yet I did not know why the energy test failed.

EAST PACIFIC RISE

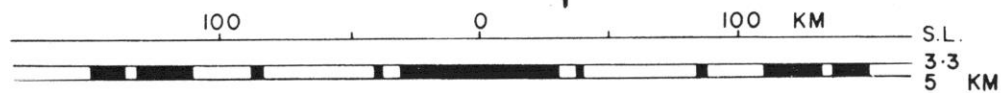
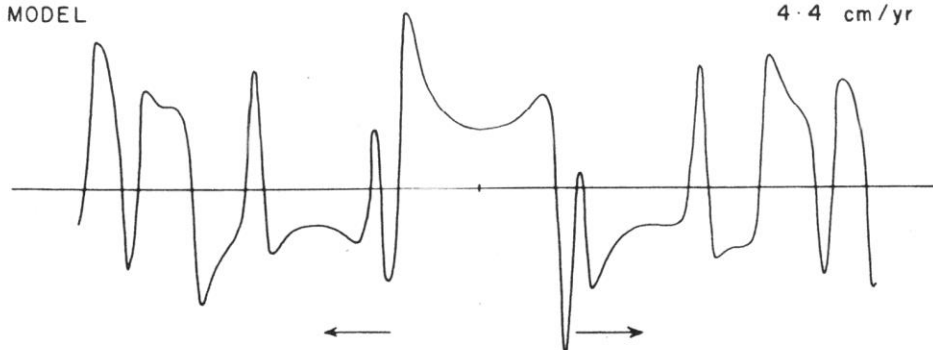
51° S

PROFILE REVERSED



MODEL

4.4 cm/yr



After Pitman (1966)

The birth of Plate Tectonics

- I date this birth from the formal proposal of Earth spherical plate kinematics in April 1967 by Jason Morgan and in September 1967 by Dan McKenzie.
- This required no new theory: just apply spherical geometry to the displacement of plates whereas Tuzo Wilson in 1965 had only discussed plate kinematics on a plane.
- But no quantification was possible without spherical plate kinematics.

My paper “Sea-floor Spreading and Continental Drift”, a test of the rigidity of plates

Its first sentence:

“It has long been recognized that if continents are being displaced on the surface of the Earth, these displacements should not in general involve large scale distortions, except along localized belts of deformation.”

The context of my “Sea Floor Spreading and Continental Drift” paper

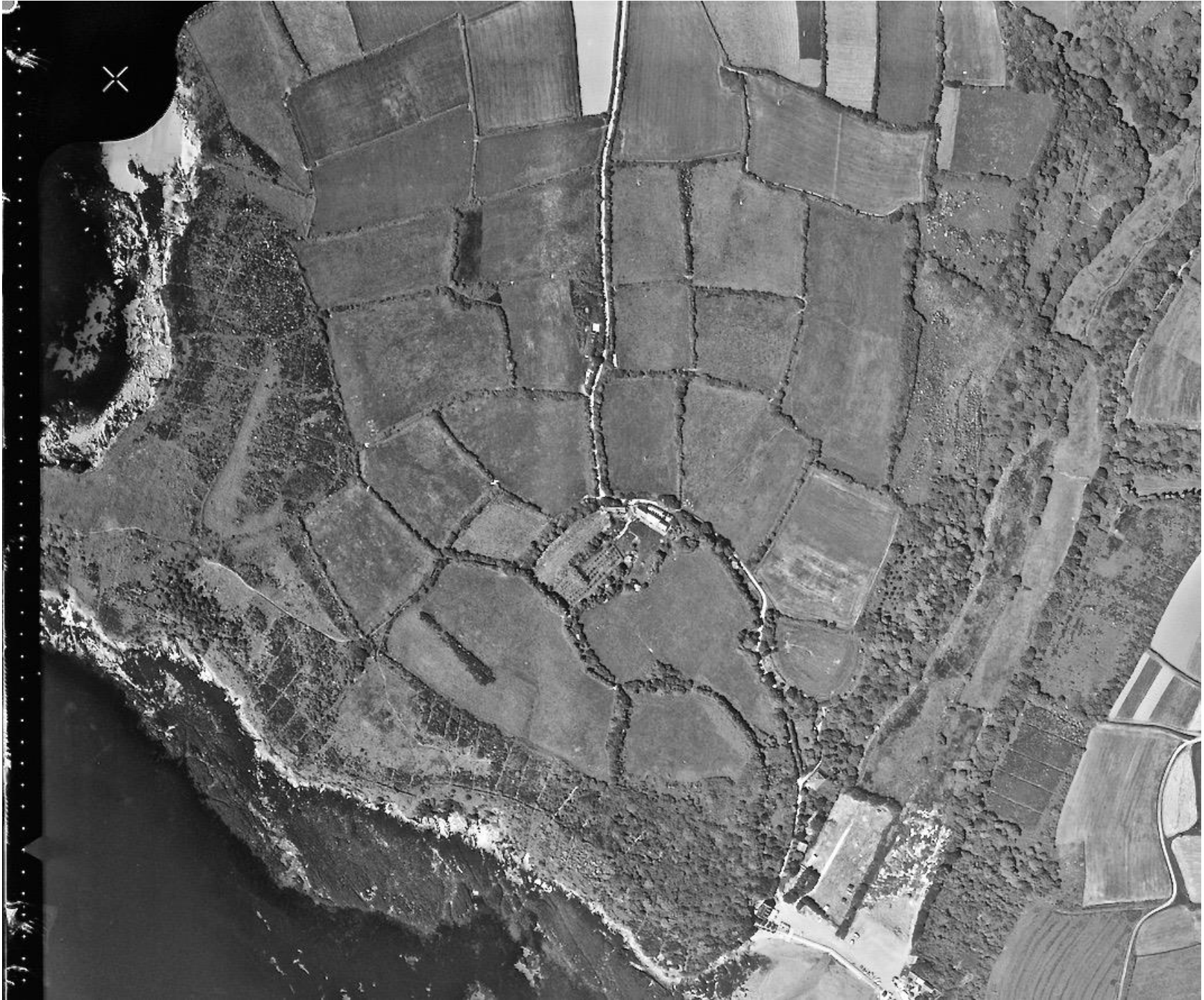
By June 1967, I was already negotiating my return to France. I wrote this paper knowing that I would be gone at the end of the year and would not have the possibility to exploit the new paths I was opening.

I had the mission to create in the future Centre Océanologique de Brest a multidisciplinary research department with the biologist Lucien Laubier.

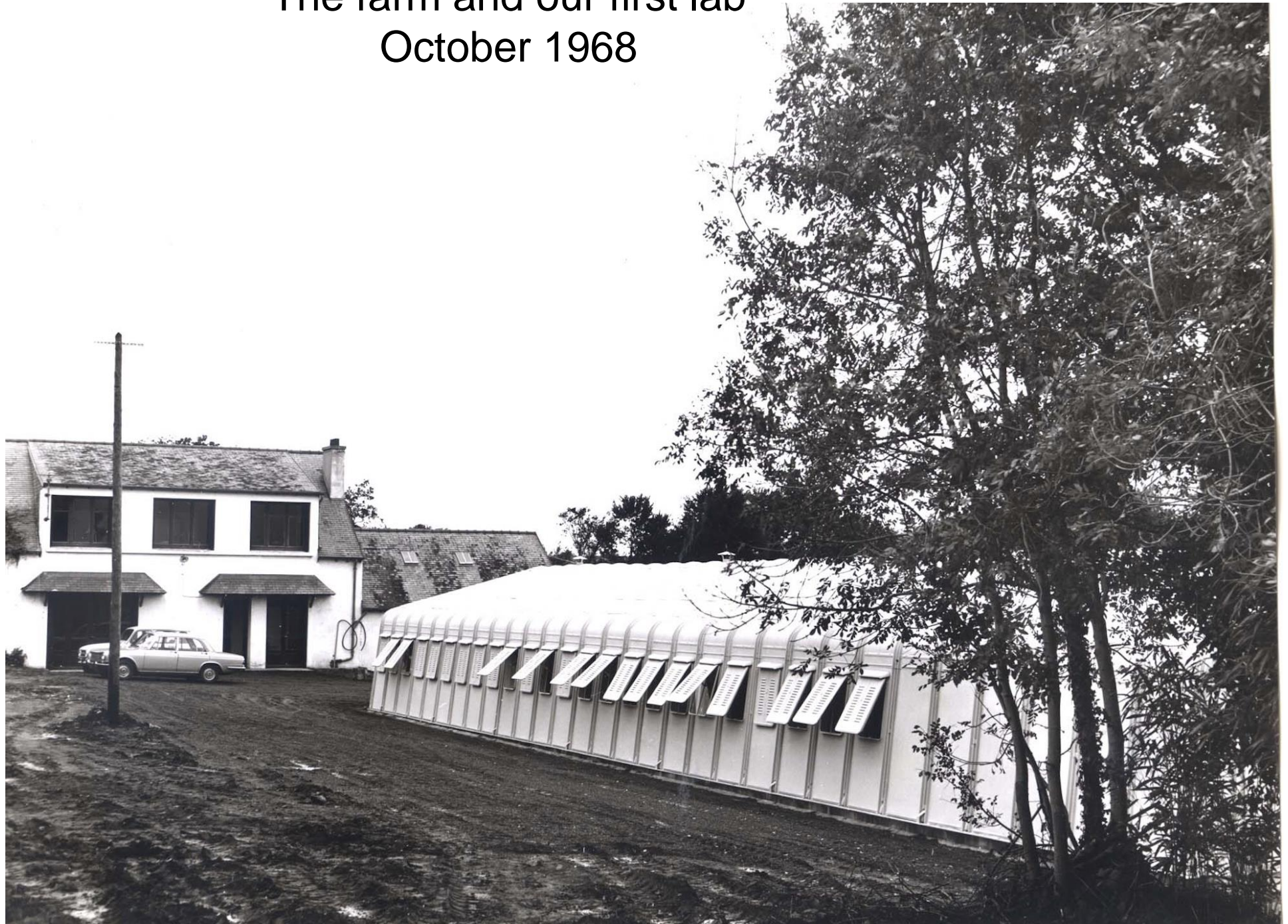
My ambition: a French Lamont.

Centre Océanologique de Bretagne 2018





The farm and our first lab
October 1968



My exclusive source for the Sea Floor Spreading paper was Jason Morgan extended outline (Late April 1967) of his communication on April 19 1967 at the AGU “Rises, trenches, great faults and crustal blocks”.

***His message:
It is easy to quantify
the relative motions of plates
on the spherical Earth
and it works.***

I dropped everything to
quantify the motions of plates.

But none of my usual coworkers were interested in joining me. They did not consider this a priority.

Note that this was also true of all those who listened to Morgan's talk and of the nine scientists (in addition to myself) who received the extended outline.

**The concept was too new
to be adopted universally right away.**

*Seven months of solitary work
from May to November 1967:*

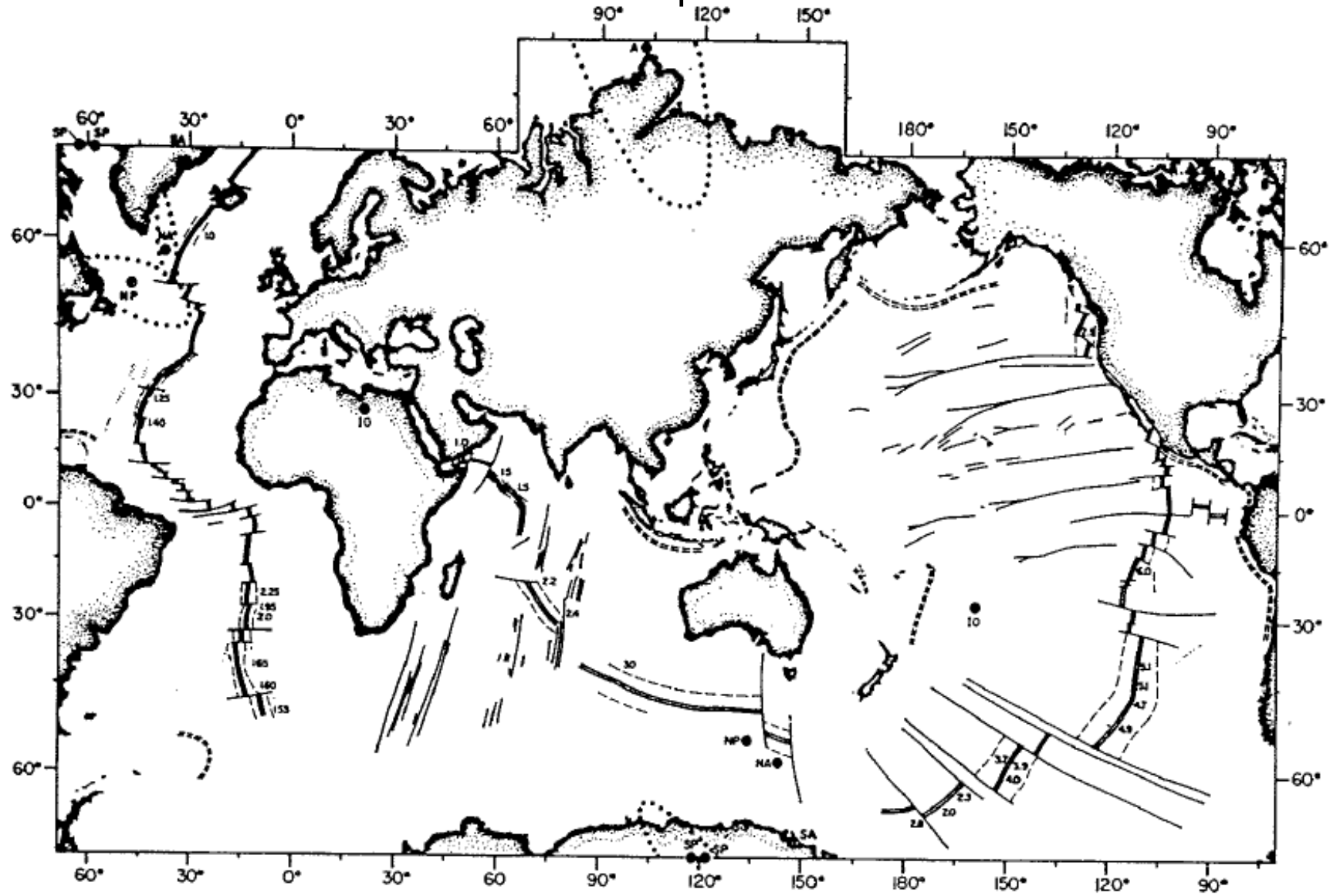
I opened three new directions knowing
that I would not be able to exploit them.

1: quantification of five openings of
oceans, June and July, ***first
demonstration of the absence of Earth
expansion***

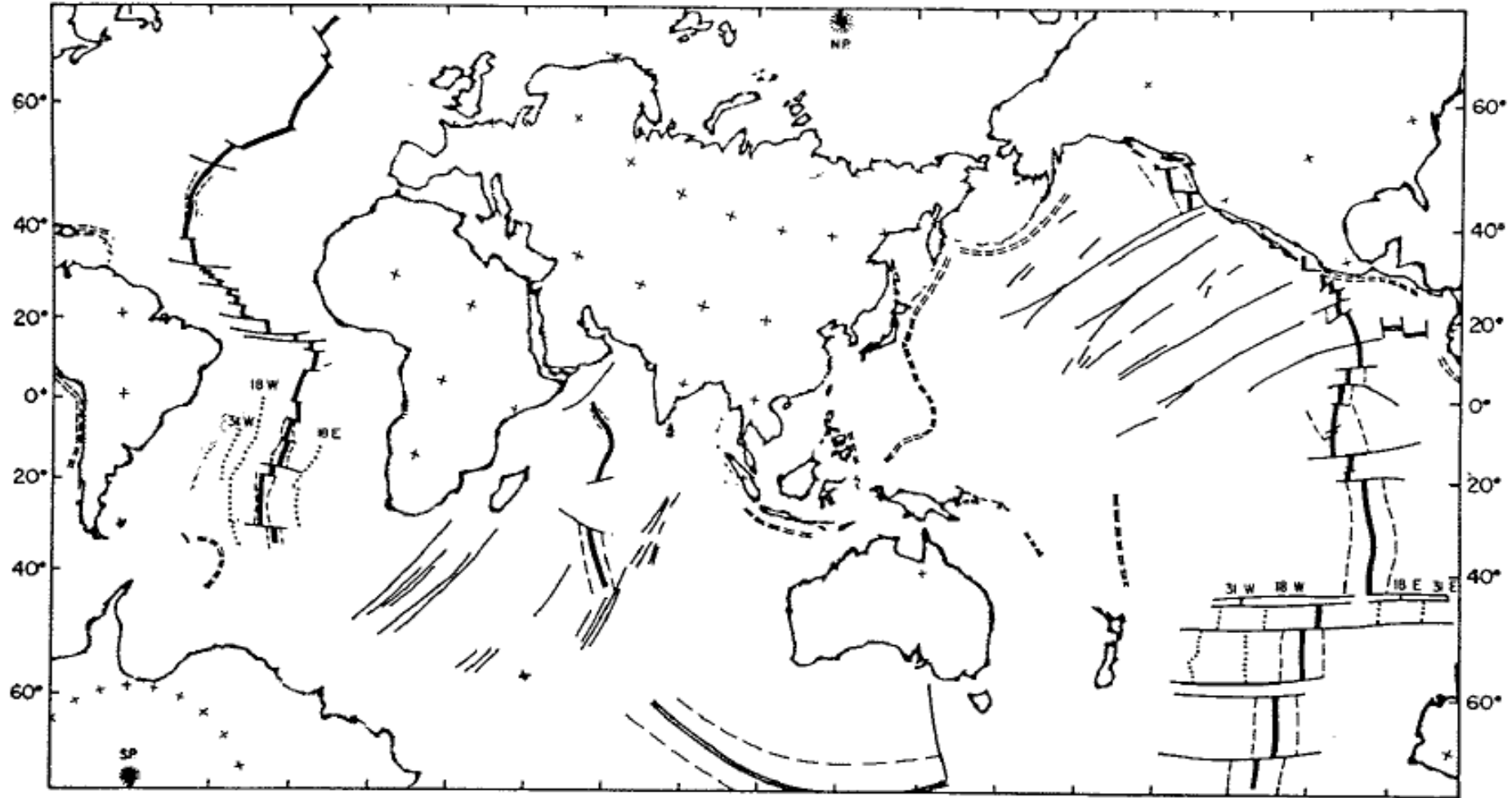
2: closure of Earth plates circuit, August
and September, ***first global model***

3: ***first finite reconstructions*** based on
magnetic anomalies, October

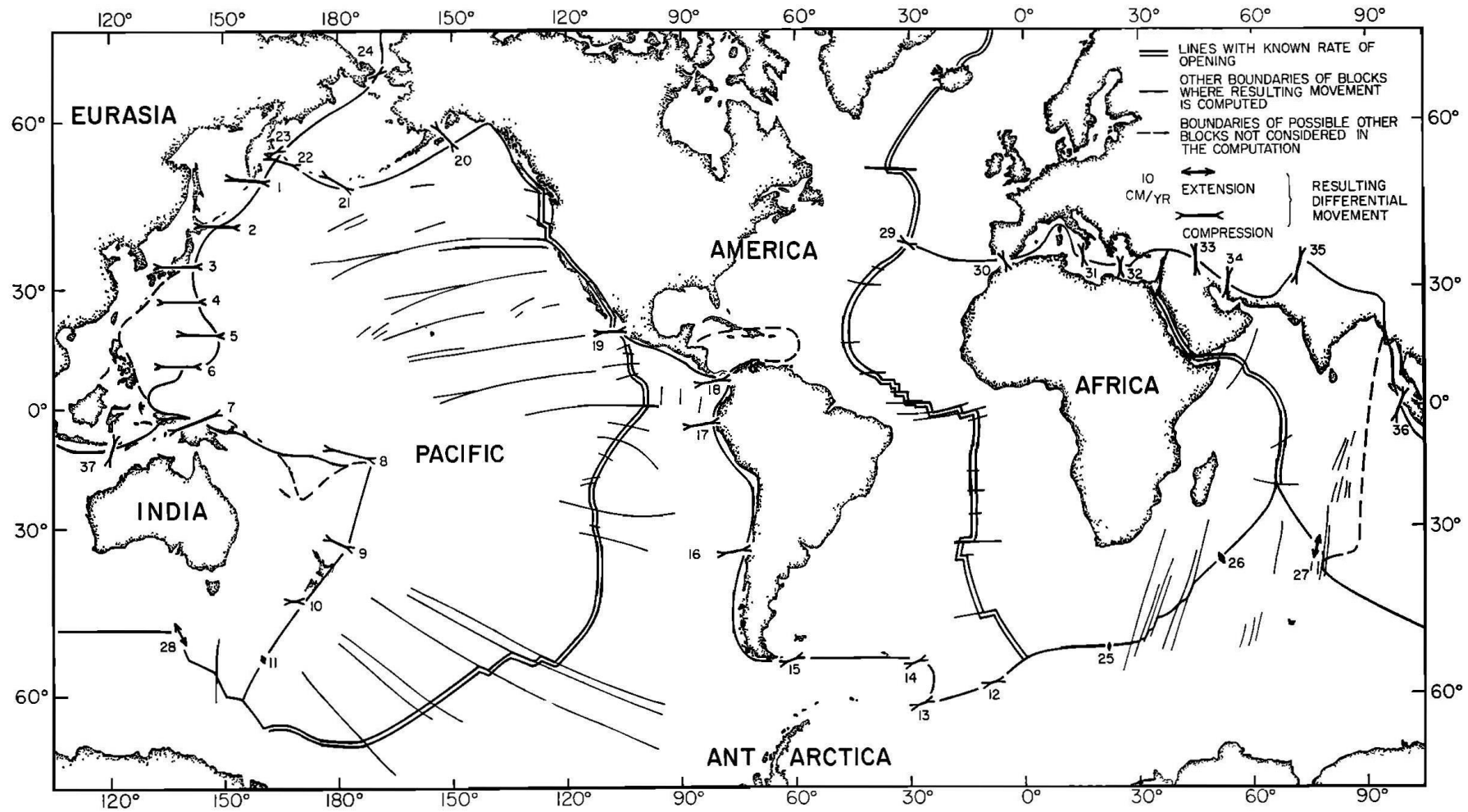
Data for six plates model



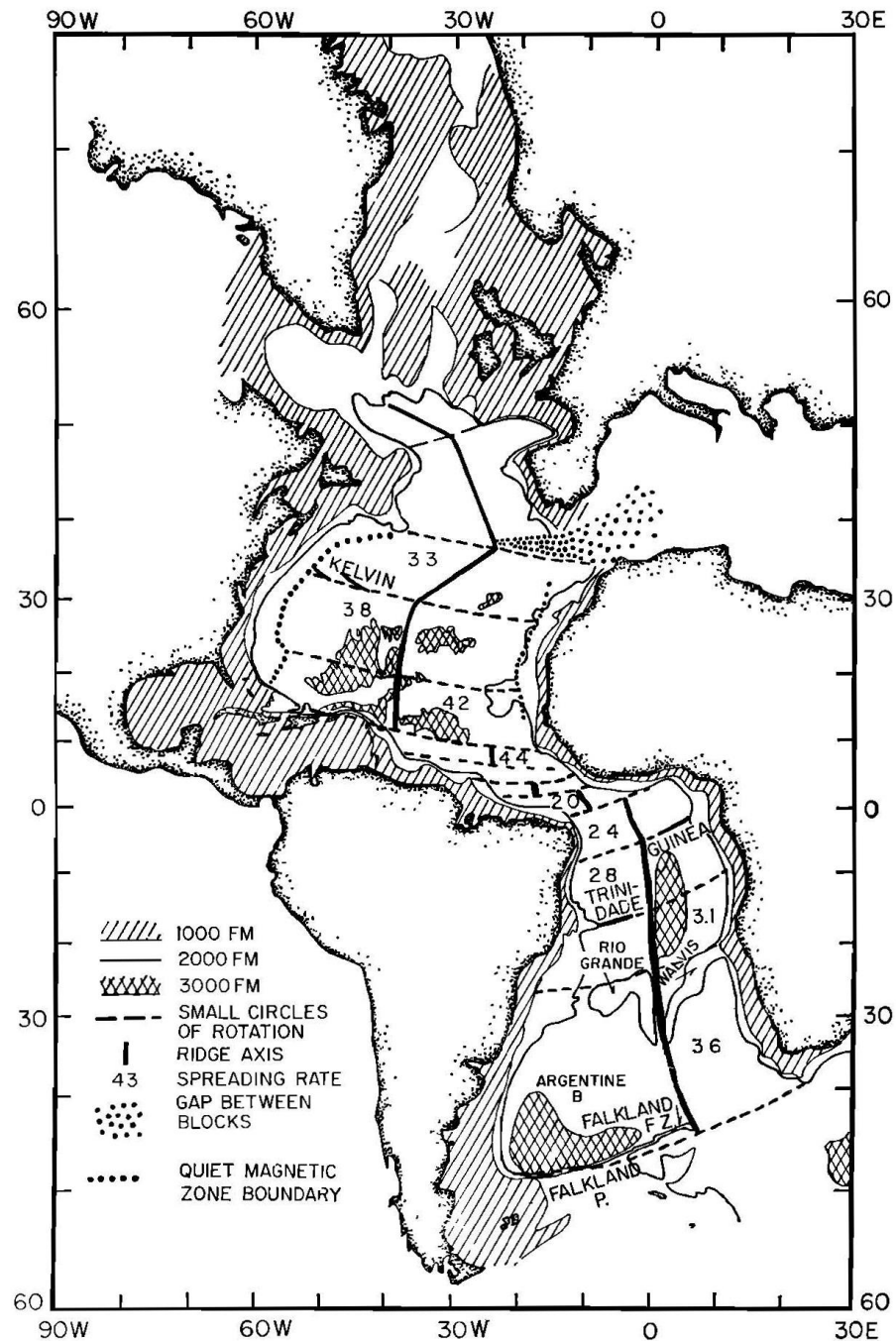
1 No Earth Expansion



2. First global kinematic model



**3. First finite
reconstructions
based on
magnetic
anomalies
Reconstruction
at anomaly 31
(70 Ma)**



The theory of Plate Tectonics

After January 1968, I was cut out from all Lamont data and my priority was building this new oceanographic lab. Consequently, I first turned to Plate Tectonics theory with Jean Francheteau and Jean Bonnin. This resulted in the publication in 1963 of our book “Plate Tectonics”, the first Plate Tectonics manual.

.

“I find it virtually impossible to find fault with this book.”

Fred Vine

The book was a manifest about Plate Tectonics that would then guide my research:

1 kinematics in the North Atlantic Ocean

1969-1970

2 accreting plate boundaries with FAMOUS

1973-1974

3 consuming plate boundaries with HEAT (1979) and Kaiko (1983-1984)

4 continental tectonics especially within Greece and Turkey starting in 1979

