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Tectonics and Continental Drift

J. Goguel

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XVI. Tectonics and continental drift

By J. Goguel

Service de la Carte Géologique de la France, Paris

Interpretation of tectonic features through horizontal movements has opened the way to the idea of continental drift. Argand precedes Wegener. One should speak of continental-block drift rather than of continental drift.

But this interpretation has been open to question. Gliding tectonics explain horizontal compression and cover folding, resulting from a basement upheaval. The importance of the great transcurrents faults is that the horizontal displacements they prove are not subject to such discussion.

One of the main facts brought out by tectonics is the chronology of the movements. The idea of individual, more or less world-wide, phases of discontinuous activity, should be discarded.

The isostatic equilibrium of the crust is one of the most important facts to take into account. It is likely that the existence of the asthenosphere, inferred from vertical displacements, also makes horizontal movements possible.

Isostatic equilibrium of the crust is consistent with any set of horizontal stresses, which must be the cause of horizontal deformations.

Inquiry about the causes of such horizontal stresses leads only to very hypothetical conclusions. The best guide is the evaluation of the amount of energy involved. The energy produced by earthquakes if of the order of 10^{25} ergs/y, and the power needed to allow tectonic phenomena seems not to be much higher. Geothermal flow amounts to 10^{28} ergs/y. Thus, we can see how tectonic phenomena may derive their energy from geothermal flow. Two mechanisms have been suggested: contraction by cooling may build up stresses, but not provide the necessary amount of energy. But the convection current hypothesis seems to work.

All the models of convection currents that have been considered share these two characteristics:

(*a*) Between ascending and descending branches, the deviation of density or temperature from their mean value at the same depth are very small; thus, structure of the Earth as a whole may be studied without taking these deviations into account.

(*b*) All these models involve velocities so small that inertia plays absolutely no part.

I favour the idea that convection currents have been discontinuous, both in time and space.

Wegener's idea of continental drift was, to a large extent, suggested by the magnitude of the horizontal displacements assumed by the Alpine geologists such as Marcel Bertrand, Schardt, Lugeon who had unravelled the part played by nappes in the Alpine structures. Later, the ideas of Argand, himself a follower of Wegener, added strong support.

When one tries to reconstruct the drift, not of the continents as they stand today, but of their parts, those of the relative displacements which are related to mountain ranges are of special interest, because it is possible to survey their structure, and thus, to get an estimate of the movement, and to fix the time at which it happened. This is easier than for continents separated by oceans.

Since Wegener's time, Argand's interpretation of mountain ranges, and especially of the Alps, has been subjected to much discussion, and I do not believe that there is now a single geologist who would accept displacements of the same magnitude. By gliding tectonic, one can explain large horizontal displacements and folding of a sedimentary cover, as a consequence of a vertical upheaval of the basement. A few geologists, such as Van Bemmelen, even extend gliding tectonic to crystalline basement, and thus, they do not need to accept any deep horizontal displacement.

I would not myself go so far, because I think a horizontal crushing of the basement is the best explanation of its deformation, but we must admit, this is a matter on which there is no unanimous agreement.

This gives a special interest to the great strike-slip faults, because they give an instance of undoubtedly large horizontal displacement.

The rifts, such as the Rhine graben, or the east African rifts, seem to be produced by extension but this is open to argument. At the most, it is a small extension, no more than a few kilometres. It may be that any larger extension would bring an invasion by the sea, as in the instance of the Red Sea.

On the whole, support for continental drift by tectonics seems less convincing now than at the time of Argand and Wegener. We must attempt to combine all the evidence from the structures of mountains, from the major strike-slip faults, and from rifts, in order to get an overall picture of the strain undergone by the continents, which must be taken into account for any reconstruction of continental drift. Thus, Wegener has admitted a large displacement of India with respect to the rest of Asia in relation to the growth of Himalaya. Recent attempts have taken into account rotations and strike-slip displacements, but the magnitudes are much smaller than in continental drift. I have tried to do this for the western Alps (Goguel 1963), assuming we have, in France, only a lateral recoil, induced by the thrusting toward the north in the Central Alps, thrusting that is interrupted near the French border. In a similar manner, Aubouin (1960) has studied the strike-slip displacements between the eastern Alps and Dinarids. Even if these reconstructions are open to discussion, such attempts seem promising.

The suggestions of Carey, as to the deformations undergone by continents on a world-wide basis, still need to be checked against local structures.

The most important information that geology can give us, is the time sequence of the movements, or, at least, of those movements which have been involved in building mountain ranges. I want to stress the character of this time-sequence, because it has often been misinterpreted. It comes only from preconceived ideas that one has often spoken of world wide tectonic phases, implying discontinuity in the movement. A closer study of the stratigraphic data shows that we must keep to the idea of long lasting periods of tectonic deformation, at a most variable speed, with paroxysms, which may occur at different times in different places along the length of the range. For distant continents, the periods of deformation, even in the broader sense (i.e. Alpine, Variscan), can be quite different. Such is the case between Europe and the Far East. The whole study of the tectonic time sequence should be reconsidered on such a basis.

These are, on the whole, the data about which we can be fully confident. But you would be deceived, if I stopped here, so I shall try to go further, relying more and more on hypothesis, or poorly warranted assumptions. While doing so, it is most important to be fully aware of the part of conjecture, included at each step in our hypothesis.

One firm basis is provided by isostasy, and the fact that isostatic equilibrium re-establishes itself after severe erosion, or thick sedimentation. This means that vertical displacements occur through the deformation of matter lying a few tens of kilometres deep, and were induced by changes at still shallower levels. It may be useful to point out that this depth, at which occurs the strain by which isostatic equilibrium is restored, is

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indicated by gravimetry in a very crude way only, and that it can only tentatively be connected with the Mohorovičić discontinuity indicated by seismology.

Starting from this, we may venture a few hypotheses:

First, the Asthenosphere, that is the matter that can yield easily to allow re-establishment of isostatic equilibrium, may perhaps allow also horizontal displacement. Here is the point where we find the most important dividing line between the many theories that have been suggested for the interpretation of deep tectonics. They fall into two groups: on one side, the matter beyond a certain depth is believed to allow slow deformations, or creep, even under small stresses; on the other hand, such creep is believed to be negligible, even at rather large depths; the matter of the upper mantle would thus be able to sustain large stresses for a long time. Such ideas are supported by eminent scientists, but I do not understand very well how they can explain isostatic equilibrium.

Then, I should like to remind you of a theorem in mechanics, which states that the equations of elasticity, for a plane plate, fall into two groups: the one connects normal displacements and forces, the other, stress and strain in the plane of the plate. If we apply this to the crust (and it can be shown that the curvature brings about only a slight correction), it means that isostatic equilibrium expressed by the first set of equations, can be maintained, whatever may be the horizontal stresses and strains. Thus, any sort of tectonic deformation, induced by horizontal stresses, can go on, without any change in isostatic equilibrium. This suggests that we should look, as a cause for tectonic deformation and eventually, of continental drift, for phenomena able to build up horizontal stresses in the crust.

Such an inquiry is highly hypothetical but, at least, we may rely on the magnitude of the mechanical power involved in some phenomena. It is convenient to write down some figures: power involved in seismic activity has been estimated as 10^{25} ergs/y.

If we accept a rather easy creep, except for the rocks near the surface or at shallow depth, a very crude estimate of the energy involved in tectonic deformation gives a figure of about the same order of magnitude. But, if one accepts that, even at a large depth, the rocks are able to sustain large stresses with little or no creep, one must accept much larger values; geothermal flow is equivalent to 10^{28} ergs/y;
radiation received from the sun is equivalent to 5×10^{31} ergs/y;
energy dissipated by oceanic tides is of the order of 10^{27} ergs/y.

Radiation from the sun is absorbed at the surface, and is the energy source of almost all the atmospheric and surface phenomena. I cannot see how the energy of tectonic deformation could come from there. In the same way, the energy of tides is dissipated as heat, or in erosion.

It is not possible here to review all the processes that have been suggested to explain tectonic deformation. The greatest number do not seem to give the right amount of energy, and I think tectonic and earthquake energy can only be derived from the geothermal flow. Thermodynamics shows how this mechanical energy, of some 10^{25} ergs/y can be derived from a thermal flow of 10^{28} ergs/y.

As far as I know, only two processes have been suggested for this derivation: contraction by cooling and convection. But, even if we can show that contraction is inadequate, this

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is not a demonstration of convection currents, because we can never be sure our review of the possible processes was complete. At least, we must remember that we should keep to the idea of convection currents only in its most broad form.

The suggested contraction by cooling is inadequate. We are not at all sure that the Earth is cooling and, if it is cooling, at what rate. Even if such a cooling is admitted, it would mean only a slow decrease in temperature at a depth up to a few hundred kilometres. Mechanical stresses thus induced would grow proportionally to the time, while available elastic energy will grow as the square of time. It would be necessary to accept that these stresses at depths of hundreds of kilometres, can persist without being reduced by creep, for hundreds of millions of years, to get an amount of energy of the order of 10^{25} ergs/y. If we admit this absence of creep at depth, we need much more energy to explain tectonic processes.

With convection currents, it is easy to build up models, for which it can be shown that the right amount of energy can be produced even when using, for the viscosity of the mantle, the highest figures suggested by the time lag in restoration of isostatic equilibrium. Thus, we may accept the general idea of convection currents. But such computations do no give any clue in favour of this particular model that has been used. It is likely that any other would do as well.

However, all such models seem to share a few characteristics:

First, we have to assume only slight deviations, either for temperature, or density, between ascending and descending currents (for instance, a few tens of degrees, or a few thousandths for density), and stresses that are very small, when compared to the pressure. This allows any study of the equilibrium of the Earth, either mechanical or thermal, without taking into account convection currents. The small deviations they may induce have not been detected in the seismic waves velocities. They may be related to the slight undulations of the geoid surface, whose magnitude should allow us to set an upper limit to the deviations in density.

Secondly, in any of these models, the speeds are very small, and thus, kinetic energy is absolutely negligible. This kinetic energy may be of the order of the amount of energy used in one-thousandth part of a second. In other words, the mechanics of convection currents in the mantle is a mechanics with no inertia; we only have to take into account equilibrium between acting power, and internal friction or other forms of passive resistances. This is completely different from what we can observe at our scale of time and length, where inertia plays an important part, and allows waves, eddies, whirlpools, and so on. We should not apply to mantle convection currents what we have learnt from such observations. Moreover, we must be ready to accept that such currents may start or stop at any time, for slight alterations in surrounding conditions.

The actual convection currents may be very different from anything that had been suggested, and I would not dare to venture a description. I shall only try to suggest a few characteristics.

We should expect the relation between stress and strain velocity not to be linear. If the amount of crystals has previously increased, so as to chill the matter of the mantle, whenever deformation begins, internal friction brings local reheating, and thus, melting of crystals, with a sharp drop in viscosity; thus, further movement will be much easier along

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surfaces of previous deformation. When started, deformation becomes easier and easier, and this may explain phases of movement, succeeding long periods of rest. Moreover, with no inertia, velocity may be very irregular, movement can start or stop at any time, and we may assume that this should explain the timing of tectonic processes, and perhaps also, if not the individual volcanic eruptions and earthquakes, at least the shift in the main places and times of such activities.

Movement may also be discontinuous in space. I would not expect an even distribution of strain velocity, and I think any use of spherical harmonic functions to describe convection currents should be discarded, as not warranted by the non-linear relation between stress and strain velocity. Moreover, harmonic solutions would be valid only for the start of the movement, assuming initial condition to be quite uniform, which is most unlikely.

I would rather expect narrow strips of strong shear, in which heating by internal friction has remelted crystals and reduced viscosity and between which blocks would move almost without internal deformation. But such strips must be very numerous, and branch one from another in a very intricate way, since the overall deformation can bring only very small changes in the external shape of the Earth.

In any model, it is usually assumed that the chemical composition is uniform. But, the number of complete turn-over of the convection currents, at any one place, since the birth of the Earth, may have been very small, and I believe we should try to combine the idea of convection currents, with the idea of chemical differentiation, advocated by several scientists, such as Urey or Beloussov. This may include segregation of heavy parts, such as iron, settling at the bottom, and leaving lighter matter, to find its place in ascending currents, or separation, near the surface of light, or even volatile matter, that can rise to the surface and play a part in volcanic or magmatic activity, while the denser part of the matter would feed a descending branch. We may think of any combination, between such differentiation currents and steady state, purely thermal convection currents, occurring in a matter with a fixed chemical composition.

As to the places and times of actual currents, a number of suggestions have been made. I would rule out any picture, based on an harmonic pattern. Other models have been offered, based either on the distribution of oceans and continents, ascending currents being assumed either under continents or under oceans, or linked to the distribution of the most recent mountain ranges. I would favour this last alternative, at least for continental areas. What we need is a more accurate reconstitution of the time sequence of actual continental strain, and I think this would give us the closest approximation to the actual pattern of convection currents.

If continents are drifting, or have drifted, I am ready to admit that the same forces, responsible for tectonic deformation, are also acting to move them. But what we know about these forces is not a very great help to answer the question.

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