COMMENTS ON EARTH SCIENCES: GEOPHYSICS
PETER J. SMITH and J. A. JACOBS, *Co-ordinators*

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Non-equivalence of Oceanic and Continental Heat Flows and other Geothermal Problems

Over the past decade terrestrial heat flow has gradually developed from being a predominantly observational or data gathering branch of geophysics to one where the data are being used either to form the cornerstone of a geophysical theory or to discriminate between rival theories. An earlier note by Sass[1] dealt with these aspects in a general way but I think there are some problem areas that should be pointed out and discussed.

Before the first oceanic measurements were made it was expected that the heat flow across the ocean floor would be very much lower than that across the continental crust since it was believed that about 75% of the heat flow was contributed by the radioactive elements concentrated in the granitic part of the continental crust. However, the results to date indicate that there is little difference between the mean oceanic and continental heat flow values. This presents a major problem since the implications are that the upper mantle beneath the oceans is significantly different from that beneath the continents. The great difficulty in reconciling the geochemical requirements with the heat flow data is too frequently glossed over and I feel that at this stage we should seriously re-examine some of the basic assumptions made in producing heat flow values.

One of the first problems we run into is how the mean heat flow values for oceans and continents should be computed. Lee and Uyeda[2] have used several methods. If the distribution is regarded as normal, the arithmetic mean for continental values is 1.4 μ cal cm^{-2} sec^{-1} (HFU) and that for oceanic values is 1.3; if the values are averaged over grids of equal area, which is a form of weighting, the values are 1.4 and 1.4. In all cases the modes from the histograms give values of 1.1. If a log normal distribution is assumed the mean values for continents and oceans are 1.4 and 1.2 respectively.[3] On the other hand, Polyak and Smirnov[4] argue that to obtain a mean heat flow value for the continents the mean value for each geologic province should be weighted according to the area of that province. In this manner they arrive at a value of 1.15 HFU for the average heat flow for continents whereas a similar process of weighting for the oceans using the data of Ronov and Yaroshevsky,[5] yields a mean value for oceanic areas that is significantly below that given by

Lee and Uyeda, but which is still slightly higher than the mean continental value given by Polyak and Smirnov. Unfortunately, although these methods lead to some uncertainty in the absolute mean values for continents and oceans, the apparent equivalence between the values remains no matter which method of averaging is applied.

Is it possible that there is some systematic effect which has so far been overlooked but which, when allowed for, would cause the mean oceanic and continental heat flow values to diverge?

It seems to me that far too little attention has been paid to long term air temperature changes of continental extent. We know a great deal about short period changes such as daily, annual and even those of a decade in extent; we are even obtaining some knowledge of changes with periods of the order of 100 years.[6] However, we really know very little about longer period changes and in particular those temperature changes that accompany the onset and retreat of ice sheets.

There is always some uncertainty about the time of onset and retreat of an ice sheet and about the relatively brief interglacial periods, but the most important unknown factor is the temperature change, and its form, accompanying the onset and retreat. It is generally assumed that the temperature changes are rapid and may therefore be regarded as a step function, but the magnitude of the step is the most critical unknown factor. It is usually assumed that the temperature at the base of the ice sheet is close to the pressure melting point, but here we would need to know the thickness of the ice sheet and whether the basal water was confined or unconfined. For instance, recent data from a 2164-meter deep hole drilled through the Antarctic ice sheet[7] gave a reasonable heat flow value (1.8) and indicated that the temperature at the base, where liquid water was encountered at the ice-rock interface, was −1.6 °C, which is close to the estimated pressure-melting point for confined water. However, data from a hole drilled through the Greenland ice cap[8] indicated that although a reasonable heat flow value (1.0) was again obtained, the temperature at the base of the sheet was −13 °C. This low temperature is close to that given by Radd and Oertle[9] for the pressure melting point of unconfined water systems at pressures equivalent to 2 km of ice. Crain[10] took a more pragmatic approach with the data he collected from the St. Lawrence valley. He experimented with different temperature changes and found that a figure of −4 °C for the temperature at the base of the Wisconsin ice sheet gave minimum scatter in his results. This would imply an ice sheet thickness of about 5 km or that the basal temperature is lower than the pressure melting point for confined water.

Clearly there is a great deal we do not know about the temperature at the base of an ice sheet and therefore about the magnitude of the surface temperature change when the ice sheet retreats or advances.

Many of the data that have been given in the literature, and used in estimating means for various areas on the continents, have not been corrected for the onset and retreat of an ice sheet known to have occurred in the area. There is some justice in this neglect since it has been argued by the more cautious authors that it is better to quote the observed value of heat flow and simply point out that the area had undergone extensive glaciation, but that because of the uncertainties no correction had been applied; the more adventurous proposers of geophysical theories can then manipulate the data as they see fit.

To give some idea of the importance of knowing more precisely the magnitude, V, of the surface temperature change we can take the case of a typical 600 meter borehole in an area where the onset of an ice sheet occurred 100,000 years ago, the retreat occurred 10,000 years ago and the uncorrected heat flow value is 0.75 HFU. If $V = 5$, 10 and 15 °C the heat flow values corrected for the ice sheet effects are 0.95, 1.2 and 1.4 respectively. Interglacial periods, which are relatively brief, will reduce this effect somewhat but with this sort of uncertainty it is clear that considerably more attention should be paid to these problems. Rather than construct complex geophysical models to account for the apparent equivalence of oceanic and continental heat flows, I think it would be better to search first for effects, either geological or instrumental, which have hitherto been neglected or completely overlooked.

For instance, Crain[11] has pointed out that neglect of the ice sheet correction is most apparent in the Precambrian regions of Canada and the USSR. If appropriate corrections are applied to these regions it could lead to an increase in the heat flow value of about 30%, and he quotes some specific examples to support his argument. If one accepts the method of Polyak and Smirnov[4] for obtaining the mean continental heat flow by weighting the mean values for geological provinces according to their areas, then the mean continental heat flow value would be considerably increased. However, the neglect of climatic effects may be even more substantial than Crain indicates.

It seems reasonable to assume that the onset and retreat of an ice sheet is accompanied by temperature changes that are not restricted to the glaciated areas but which are global in character. Thus, for instance, even though the Western Australian shield was not subjected to Pleistocene glaciation, a climatic correction might be necessary to allow for relatively rapid changes in temperature which were a cause or a consequence of the onset or retreat of ice sheets elsewhere on the globe. In other words, there is a strong possibility that the mean equilibrium heat flow values presently quoted for continents are lower than they should be and, if corrected, the mean value might be significantly higher than the mean oceanic value.

Apart from the difficulties of pinpointing the thermal history of ocean bottom temperatures, another potential source of error leading to an apparent equality between oceanic and continental heat flow values is the possibility of convective heat transfer in conductivity experiments on some groups of oceanic sediments, thus leading to apparently high thermal conductivities. A simple examination of the needle probe theory (simple in principle but requiring a rather large and fast computer) shows that the temperature gradients in the vicinity of the probe far exceed the critical temperature gradient for water. The sediments will, of course, have a much higher critical gradient because of the inhibiting effects of the granular material but it will be dependent, amongst other things, on the permeability and porosity of the sediment. Zolotarev[12] has attempted to solve the similar problem of conditions for thermal convection in porous sedimentary beds. His inequality[9] for the onset of convection is dimensionally inhomogeneous, probably due to a typographical error resulting in the displacement of a gravitational acceleration term "g". Assuming the error is typographical, and substituting in his expression typical physical properties for a medium with a permeability of 0.2 darcy, it appears that we could expect convection to occur when gradients are greater than 0.02 °C cm^{-1}. In a typical laboratory experiment the gradients are closer to 1 °C cm^{-1}.

It might be argued that the needle-probe results were checked against results from a dividend bar apparatus[13] but an examination of the data shows that the temperature gradients in the divided bar apparatus are often of the same order of magnitude as those involved in the needle probe methods. In fact, it may well be that divided bar conductivities of some of the more permeable rocks are also systematically too high.

It has also been argued that the measurements of heat flow at the preliminary Mohole[14] site confirm the validity of the oceanic heat flow methods. However, core descriptions[15] from the boreholes used indicate that even in the deepest hole the material consisted mainly of oozes right to the bottom; since the needle probe method was used to determine the conductivity of these oozes, the results are subject to the same errors as any other needle-probe method on oceanic sediments. In other words, the measurements of heat flow at the preliminary Mohole site confirm that the heat flow value derived from the first few meters of an oceanic sediment will not be significantly different from that derived over a couple of hundred meters of the sediment, but they do not provide conclusive evidence that the absolute values of heat flow are correct.

Some interesting results are given by Ratcliffe.[16] Although there is some ambiguity in how to interpret his figure 6, he shows a plot of conductivity versus water content for various artificial mixes of water and matrix material. The conductivity increases with porosity until the matrix material has to be held in suspension with a gel which can also be expected to inhibit convection; after this point is reached the conductivity decreases with porosity.

Furthermore, in his figure 3 there is a suggestion that at high porosities the "conductivity" of ocean sediments begins to increase with increasing porosity. It might be pointed out here that this problem is very different from the well studied one[17] of the transfer of heat by the vapour phase in a moist unsaturated porous material.

The importance of this potential instrumental source of error is probably not as great as that of the effect of neglecting the climatic temperature variations. It might be possible to get some indication of how significant this problem is by examining the existing data to see if there is any correlation between the heat flow values from some of the oceanic areas and the permeability and porosity of the sediments, but the only convincing evidence, one way or the other, would be results from a set of well designed experiments.

So far I have discussed two aspects which I feel have received too little attention. One of them, the long term global variations in climate, is potentially a major source of systematic error for continental heat flow values, while the other is basically an instrumental problem which is probably less significant and less systematic. However, there are a number of other potential sources of error, of a more local nature, but which I feel need more investigation.

For instance, we do not know how to identify positively the existence of underground water flows at depth, or in the oceanic sediments, and how significant they might be. We are not very sure of how significantly the topography and structure, particularly unknown structure, affects the results. It is only recently that attempts have been made to estimate how long a section of borehole is required to give a reliable value of heat flow which is representative of that borehole,[6,18] and how large an area can be represented by a mean heat flow value from a single borehole.[19] We do not have very clear ideas on whether some formations produce more heat than others because of high radioactive content or exothermic reactions. Although Garland and Lennox made some attempt in 1962 to correlate radioactive content with heat flow values[20] it is only recently that a number of groups have actively followed up their work.[21,22,23,24]

There may well be other possibilities that have not been mentioned. An examination of the discussion so far will no doubt indicate that I feel we are in danger of falling into a trap that is all too common in many branches of science—namely, that where two bodies of data are difficult to reconcile into a comprehensive theory, we often tend to make patchwork adjustments when we should be re-examining the basic assumptions. And, of course, one basic assumption that is made when we quote mean terrestrial heat flow values is that all the equilibrium heat flow values have been corrected for major sources of error.

An interesting example of the dangers of too much analysis on too few data followed by too much generalization, without sufficient regard to the basic assumptions, was given at the 1969 meetings of the IASPEI in Madrid. Spherical harmonic analysis of the heat flow data[2] resulted in a very high and broad heat flow anomaly over an area of northern and western Africa, even though there were no measurements on the continent within a radius of two or three thousand kilometers of the centre of the anomaly. The position of this anomaly, along with others, led to a considerable amount of work which made use of the apparent correlation between heat flow highs and geoid lows to postulate systems of convection in the mantle. Preliminary results from Ghana[25] indicate that the heat flow cannot exceed 1.3 and is more likely around 1 HFU; a result which might have been expected by analogy with other Precambrian areas. The effect of this value on the postulated convection systems requires no comment.

Fortunately, the future is not completely black. In spite of some nagging problems there are many ways in which heat flow data can be usefully employed. For instance, if we wish to compare the heat flow values from two areas within a few hundred kilometers of each other and we know that both areas have undergone a similar phase of Pleistocene glaciation, it does not matter much that there is a great deal of uncertainty in the correction for the climatic effects. Even though we are unsure of the absolute value of the heat flow the relative values may still be usefully compared. In recent years, there has been a trend towards a more analytical use of the heat flow data, the imaginative work of Lachenbruch[23] and of Birch *et al.*[24] being excellent examples. On the theoretical side, a great deal of attention is being paid to the thermal and other geophysical consequences of sea floor spreading and plate tectonics, the most recent and most elegant work being that by Minear and Toksöz.[26]

To misquote a much overused phrase of the political commentators—the mood should be one of cautious scepticism.

A. E. BECK

References

1. J. H. Sass, Comments on Earth Sciences: Geophysics **1**, 8 (1970).
2. W. H. K. Lee and S. Uyeda, Ch. 8 in *Terrestrial Heat Flow* ed. W. H. K. Lee, Am. Geoph. Un., Washington (1965).
3. R. W. Girdler, p. 549 in *Mantles of the Earth and Terrestrial Planets* ed S. K. Runcorn, Interscience, London (1967).
4. B. G. Polyak and Ya. B. Smirnov, Geotectonics (USSR), No. 4, p. 205 (1968).
5. A. B. Ronov and A. A. Yaroshevsky, p. 37 in *The Earth's Crust and Upper Mantle*, ed. P. J. Hart, Am. Geophys. Un., Washington (1969).
6. A. E. Beck and A. S. Judge, Geophys. J. **18**, 145 (1969).
7. A. J. Gow, H. T. Ueda and D. E. Garfield, Science **161**, 1011 (1968).
8. B. L. Hansen and C. C. Langway, Antarctic J. of the U.S. **1**, 207 (1966).
9. F. J. Radd and D. H. Oertle, Nature **218**, 1242 (1968).
10. I. K. Crain, *The influence of post Wisconsin climatic changes on thermal gradients in the St. Lawrence lowland*, M.Sc. thesis, McGill University (1967).
11. I. K. Crain, Earth and Planetary Sci. Letters **4**, 69 (1968).
12. P. P. Zolotarev, Inzhenernyi Zhurnal **5**, 236 (1965).
13. R. P. Von Herzen and A. E. Maxwell, J. Geophys. Res. **10**, 1557 (1959).
14. R. P. Von Herzen and A. E. Maxwell, J. Geophys. Res. **69**, 741 (1964).
15. W. R. Riedel, H. S. Ladd, J. I. Tracey and M. N. Bramlette, Bull. Am. Assoc. Petrol. Geol. **45**, 1793 (1961).
16. E. H. Ratcliffe, J. Geophys. Res. **65**, 1535 (1960).
17. J. M. Kusmak and P. J. Sereda, Soil Sci. **84**, 291 and 419 (1957).
18. J. H. Sass, R. J. Munroe and A. H. Lachenbruch, Earth and Planetary Sci. Letters **4**, 293 (1968).
19. E. D. Mustonen, *Micro-geothermal survey, Lake Dufault, Quebec*, M.Sc. Thesis, University of Western Ontario (1967).
20. G. D. Garland and D. H. Lennox, *Heat flow in Western Canada*, Geophys. J. **6**, 245 (1962).
21. R. D. Hyndman, L. B. Lambert, K. S. Heier, J. C. Jaeger and A. E. Ringwood, Phys. Earth Planet. Interiors, **1**, 129 (1968).
22. J. H. Sass, P. G. Killeen and E. D. Mustonen, Can. J. Earth Sci. **5**, 1417 (1968).
23. A. H. Lachenbruch, J. Geophys. Res. **73**, 6977 (1968).
24. F. Birch, R. F. Roy and E. R. Decker, Ch. 33 in *Studies in Appalachian Geology (Northern and Maritime)* ed. E-an Zen, W. S. White, J. B. Hadley and J. B. Thompson, Jr., Interscience, Washington (1968).
25. A. E. Beck and E. D. Mustonen, *Heat Flow in Ghana*, Abstract 58, Volume 1, I.A.S.P.E.I. program, Madrid (1969).
26. J. W. Minear and M. N. Toksöz, J. Geophys. Res. **75** (8), 1397 (1970).