Thermal Plumes in the Earth's Mantle

D. C. TOZER

School of Physics, University of Newcastle upon Tyne

Dr Tozer casts a critical eye over the concept of thermal plumes that is attracting a good deal of attention in the Earth sciences just now.

IN spite of declarations of belief by distinguished geophysicists at a recent Royal Astronomical Society meeting devoted to this subject that thermal plumes do not exist in the mantle[1], the matter seems to be of sufficient current scientific interest to merit a less theological treatment, and with this in mind I have been asked for my comments as an interested spectator.

I do not propose to waste more than the minimum of space on the question of the existence of thermal plumes, for the answer seems to depend on the way one chooses to use language. Besides, physical scientists are generally more interested in the usefulness of a concept rather than in any philosophical discussion of its reality or existence. When a geologist, at least the species that exists in the minds of physicists, if nowhere else, uses a technical term he seems to do so as a name for which the question of existence is a pure tautology. In particular there is no reason to think that the words "thermal plume" are now being used by them in any other than this sense to name collectively such diverse phenomena, inferences and ideas as, for example, that abnormally hot material rises from the bowels of the Earth to escape in the structures we call volcanoes; the process that produced the Hawaiian Islands; the pseudo-elastic structure of the deep interior of the Earth is not spherically symmetrical. As nobody has enumerated the criteria that have to be satisfied for inclusion under this particular name and, moreover, because in many minds such inclusion has come to be seen as an explanation of mysterious events, the number of "thermal plumes" shows the same tendency to increase with time as was shown in earlier epochs by "witches", "layers in the crust", "tectonic plates", and so on—my geological friends assure me that several tens if not hundreds of "thermal plumes" have now been discovered. One cannot deny on logical grounds this or any other attempt to classify geological phenomena, although on the grounds of previous usage, I think one can justifiably object to the nomenclature in this instance. I hope it is clear that a scheme to associate events or phenomena is no more a scientific explanation of their occurrence than is the fitting of a curve to a selection of experimental points, unless there is also a definite proposal as to the procedure by which it might be rer ted.

Question of Definition

I suspect that the rather loose usage of the term "thermal plume" sprang from a desire to have an original theory that would seem to make more immediate contact with geological observations than thermal convection theory, and an unwarranted eagerness to dismiss thermal convection as the theory that only predicted velocity fields in the mantle that were as smooth and symmetrical as they were geologically unacceptable. But there is no doubt that in its original meaning the term "thermal plume" did refer to convective flows—in a recent review by Turner[2] buoyant plumes and thermals are described as "a variety of phenomena related under the heading of turbulent buoyant convection from small sources" and in particular "a plume arises when buoyancy is supplied continuously". As a definition this description, with its tacit reference to plumes as flows of high Reynolds number, is too restrictive, I believe, to capture what one would like to express by the term "thermal plume" and at the same time too vague to decide whether dynamically similar flows might occur in the Earth's mantle. If one did take it as the definition, the fact that most authorities estimate the Reynolds number of mantle flows to be $\sim 10^{-20}$ could be taken as a clear judgment that thermal plumes are not a useful concept in this context. Perhaps a definition of thermal plumes which comes closer to expressing one's intuitive meaning is that it is a way in which one describes a thermal convective flow in which a velocity-dependent length, usually referred to as the thermal boundary layer thickness, is small compared with the linear dimensions of the system. This may be shown to be a convective flow for which the Péclet number $Pe = vL/K \gg 1$ where v is a characteristic speed of flow, K the thermal diffusivity of the medium and L the linear dimensions of the system. Taking from geophysical observation $v = 10^{-7}$ cm s^{-1} (3 cm yr^{-1}), $K = 10^{-2}$ cm^2 s^{-1} and $L = 10^8$ cm, we get $Pe \sim 10^3$, which certainly satisfies our condition. One could still raise the objection, however, that this is only a necessary condition for the existence of thermal plumes and that the flow would not, for the following reasons, match the "thermal plumes" envisaged in the mantle (see, for example, ref. 3). In the first place, one notices that the Prandtl number, $Pr, (= v/K)$ of mantle material is universally believed to be enormous ($\sim 10^{22}$) and, as $Pr^{\frac{1}{2}}$ gives the ratio of thicknesses of the viscous and thermal boundary layers, one sees that no large velocity gradients would be expected to exist at the limits of any attached or detached thermal boundary layer—a feature that has been taken to be characteristic of thermal plumes, and which is fairly clearly exhibited by high Péclet number flows in the atmosphere and ocean where the Prandtl number is 0.7 and ~ 7.0 respectively. Incidentally, the fact that a purely formal calculation of viscous boundary layer thickness in the mantle gives a length greatly exceeding the dimensions of the mantle would seem to belie some rather mystical talk that one part of the mantle is "decoupled" from another, but perhaps someone else could explain what is meant by that.

The second objection to thinking that the high Péclet number calculated here is sufficient reason for believing that the deep mantle has ascending plumes of hot material is based on an estimation of the thermal energy flowing into the mantle from the core. It will be recalled that one of the persistent difficulties of geomagnetic dynamo theory has been to find a source of energy that will sustain the necessary motions and at the same time be acceptable to geochemists. Translated into terms that are relevant to the present problem, one can call this the difficulty of supposing that less than $\sim 90\%$ of the heat flowing through the Earth's external surface has been generated throughout the mantle/crust region. Although there has recently been some change of attitude among geochemists about the possible potassium content of the core, it is still hard to believe that the mantle/crust system has anything like as much heat entering at the bottom as is measurably leaving it at the external surface, and it is because of this difference in the spatial distributions of heat supplied and lost that one must expect the concept of a plume as a detached thermal boundary layer to be more descriptive of cold descending material near the external surface than of rising material. This asymmetry of the flow in comparable circumstances of volume heating and surface cooling has now been clearly demonstrated in model experiments[4]. Of course, the more one is prepared to confine convective motions, such as by rapid change of rheology with radial distance, to the outer parts of the mantle/crust system, the easier it is to justify a significant heating from below and a description of that motion in terms of both rising and falling streams containing thermal boundary layers. But the extremely large Prandtl number of the situation should warn one that the rising stream is no ordinary thermal plume, and it is obvious that such a more or less upper mantle flow cannot match all the attributes ascribed to "mantle plumes" by geologists.

From these remarks, it will be seen that a decision to use the words "thermal plume" to describe only those convective flows that are dynamically similar to certain observed flow phenomena that have been named thermal plumes in the atmosphere and ocean leads to the conclusion that they do not exist in the mantle/crust material. This may be the source of some misunderstanding. Although some geophysicists may quite justifiably feel that lack of dynamical similarity is sufficient reason to ban the words in a geotectonic context, they have not been clear enough in their own minds to point out that a mantle flow which locally has a reasonably close but purely kinematic similarity to atmospheric thermal plumes is still not denied by mantle convection theory. In fact, as I have spent some years in advocating that in the most plausible formulations of the mantle convection problem very narrow columns of rising and abnormally hot material can be expected as part of the general circulation, I should like to describe briefly this view of the convection problem because it may help to define our subject matter and establish contact with general geophysical theory.

Two Factors

In a study of the Earth's response, and indeed that of the other planets, to any geochemically plausible distribution of heat sources, one is compelled to include *ab initio* the variation of material rheology with temperature and the production of heat during any material deformation. These two factors are present, of course, in all the well studied laboratory systems, the atmosphere and oceans, but it transpires that they have no great significance for the understanding of those systems, save in certain very restricted circumstances where the lifting of a degeneracy that would otherwise exist is of prime observational interest. They can be added to a theory that initially ignores them as a kind of afterthought. In contrast, with planetary problems the numerical value of the constitutive function representing the material rheology is very strongly influenced by the flow itself, rather than just by the boundary conditions. This adds to the complication of making a precise theoretical analysis and destroys the possibility of close dynamical similarity with any other system differing much in size. Limited calculation has already shown that the response of a planet to its heat sources is quite different from what has been expected from certain oversimplified methods of analysis, for example, by studying it as a problem in heat conduction theory or as equivalent to heat transfer in a body with a particular temperature-independent rheology. The salient results of the efforts to elucidate the steady state response for a set of planetary models that would cover the wide range of possible planetary materials can perhaps be best summarized in the language of control theory. One finds that the heat transfer process acts as a powerful negative feedback to keep the horizontally averaged numerical value of the rheology function below a certain depth (expressed purely dimensionally in units of viscosity) nearly independent of its particular dependence on temperature.

Further indication of the strength of this feedback is shown by calculations of the rapidity with which a postulated non-steady state situation relaxes to the steady state. This turns out to be typically of the order of a few hundred million years, though the non-linearity of the problem makes it not always possible to characterize relaxation from an arbitrary initial state quite this simply. The situation in the Earth is further complicated by the influence of hydrostatic pressure on the rheology, but calculation showed quite clearly that the average "viscosity" below a depth of a few tens of kilometres and above a few hundred kilometres would be tightly constrained to be about 10^{20} to 10^{21} poise (ref. 5). Although this particular figure and other predictions of the steady state solutions give a quantitative interpretation and coherence to a wide set of terrestrial observations, such a strong constraint on upper mantle rheology seems at first to make the existence of magma, that is, material with "viscosity" at least 10^{14} times less than the figure just mentioned and the *sine qua non* of geologists' "thermal plumes" (?), even more problematical than with the earlier theories of a planet's response to its heat sources.

Magma Formation

Ever since Lord Kelvin showed that the Earth as a whole has a higher rigidity at tidal frequencies than that of steel at room temperature, scientists who have tried to answer the problem of magma formation can be broadly grouped into two classes. In the first were those anxious to preserve a spherically symmetric view of the Earth's interior who were forced to assert that Earth material could exist in a state that simultaneously combined the requisite rigidity and viscosity, whereas the others saw magma generation purely as the result of some local anomaly in material composition. I believe a less *ad hoc* explanation of magmatism that makes it a part of the overall global response has now been found in an interplay of viscous dissipation of the internal motions and the decrease of "viscosity" with a rise of temperature. It may be shown that the ratio of the heat produced by the viscous dissipation of a motion to a "primary" heating that is assumed responsible for the existence of that motion increases with the size of a system and approaches 10^{-1} in a typical planetary situation. A value of $\sim 10^{-8}$ in any practical thermal convection experiment ensures that the interplay referred to never leads to any observable consequences there, but I believe it is possible to make some safe conjectures about the results of scaling upwards in size at fixed Rayleigh number, the empirical configuration that at the moment seems the closest simulation of a planetary situation—I refer to an internally heated plane layer with Rayleigh number $\sim 10^6$ and Prandtl number $\sim 10^2$ that is cooled from above and thermally insulated below. One can expect that because any plausible planetary "viscosity" function decreases in value with increasing temperature, the spatial inhomogeneities that are present in the rate of shear field on a laboratory scale become both more enhanced and concentrated as the depth of the layer is increased. As it is now known that on a laboratory scale the average rate of shear throughout descending material in internally heated flows at such Rayleigh numbers is much higher than elsewhere[4], one expects the effect of viscous dissipation in reducing the "viscosity" and sharpening the peaks of the rate of shear distribution to be first manifest there, and that there would be some concomitant redistribution of the heat flow at the upper surface that relatively enhances its value over descending material. It is possible to show that for layers whose depth exceeds about 10 km and those average viscosity is $\sim 10^{20}$ poise, the shearing within the descending material would have evolved to a type of quasi-discontinuous slip across a zone that is extremely narrow compared with the depth and which contains material with a "viscosity" that is typical of magma (with all that that implies about a temperature anomaly). One senses that such zones might well grow out of relatively small inhomogeneities of a flow that was at least on the scale of the upper mantle, but the asymmetry of the situation that is due to volume heating and surface cooling leads to the prediction, probably the most characteristic of this theory, that magma production is principally associated with descending material.

It is unfortunate that the purely technical difficulties of handling a problem that involves such a wide range of length scales have precluded a more detailed development of this theory of magmatism, but among its most attractive features that seem to match observation and which should be sufficient bait for any intrepid theoretician are: (1) A natural explanation of the non-random distribution of volcanoes and their positional correlation with descending material. (2) A resolution of the rigidity-cum-viscosity paradox by the prediction that almost all upper mantle material is constrained by the heat transfer process to have a viscosity $\sim 10^{20}$ poise, and that the thermal/rheological condition of matter that we call magma can only obtain in extremely narrow zones "thermal plumes?". (3) The rate at which the total viscous dissipation can produce magma, that is, reduce the viscosity of mantle material from $\sim 10^{20}$ to $\sim 10^{6}$ poise, is a few cubic kilometres annually—a figure that has also been estimated from geological observation.

I would like to think that such figures for the total magma production rate would have a calming influence on the much publicized problem of the site of the Hawaiian Islands. It will be recalled that their location at the centre of what has come to be known as "a rigid plate" has caused much consternation, though I cannot help thinking that this problem is rather like that of a man who insists that the Earth is flat, and who is constantly perturbed by the disappearance of ships over his horizon. The volume of material that comprises the Hawaiian Islands is indisputably enormous to the human mind, but represents an exceedingly small fraction ($\sim 0.1\%$) of the magma that would have been produced in the period of their formation and existence. I cannot think of any other problem involving finite deformation theory where failure to account for the disposition of such small fractions of a total mass flux i regarded as a theoretical failure or indeed of such scientific importance.

New or Old?

Having accepted the invitation to write an article that was supposed to comment on the theoretical respectability or otherwise of thermal plumes in the mantle, I have found it extremely difficult to decide whether theoretical geophysics was being asked to comment on a new phenomenon or on the current jargon for the very old and real problem of magma production. One has been constantly in the position of having to define what others mean by the title in order to make intelligible comment and I am very conscious that, in trying to do so, I may well have knocked down or elevated my own Aunt Sallys. My fears in this respect were not helped by a recent editorial in *Nature*[6], which commented that some phenomena that had been attributed to "thermal plumes" might well prove explicable in more conventional geological terms. It would be most helpful if someone would explain in terms that are meaningful to geophysicists in what respects the conventional geological pictures of rising magma differ from "a thermal plume". I would wholeheartedly agree with P. J. S. that "something is clearly going on" and that it is highly desirable to test alternative explanations of the data[6], but if that is to amount to more than a pious hope, the protagonists of "thermal plumes" have to stop using the words as a name for phenomena or states of mind—for example, "that which makes the situation around Iceland more complex"—and frame a definition that is amenable to test.

1 *Nature*, **243**, 192 (1973).
2 Turner, J. S., *Ann. Revs. Fluid Mech.*, **1**, 29 (1969).
3 Deffeyes, K. S., *Nature*, **240**, 539 (1972).
4 De La Cruz Reyna, S., *Geofis. Int.*, **10** (2), 49 (1970).
5 Tozer, D. C., *J. Geomag. Geoelec., Kyoto*, **22**, 35 (1970).
6 *Nature*, **243**, 491 (1973).

Applicability of Plate Tectonics to Pre-Mesozoic Time

J. C. BRIDEN

Department of Earth Sciences, University of Leeds

Plate tectonic theory has provided a synthesis to account for the geological development of the Earth's crust during the past 10% of its history. Doubts are now being expressed, however, about the applicability of this theory to the origin of some zones of deformed Precambrian rocks.

THE plate tectonic model[1] is, in the first instance, an instantaneous description of present-day crustal behaviour. It is recognized that the crust and the top few tens of kilometres of the underlying mantle (together known as the lithosphere) behave as a small number of rigid plates in motion relative to each other and to the rotation axis of the Earth[1-4]. Virtually all seismic, volcanic and tectonic activity is localized near the margins of these plates and is associated with differential motion between them. The margins are of three kinds. At extensional margins two adjacent plates are moving apart as new oceanic crust is created, in general accompanied by formation of an ocean ridge. At convergent or destructive margins