THE CONCEPT OF A LITHOSPHERE

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RESUMEN

La actual tendencia de definir la litósfera como una región de propiedades elásticas características, puede distorsionar un importante aspecto cualitativo de los procesos tectónicos en un planeta. Si se admite a escala de laboratorio que un mismo material puede comportarse casi elásticamente (como un sólido) o casi viscosamente (como un líquido) según el régimen de carga, podemos sugerir el término "litósfera", para designar aquella región de un planeta (si la hay) que se comporta en forma casi elástica bajo la acción de los esfuerzos asociados al proceso de transferencia de calor. Al definirse una región de este tipo es necesario entrar a discutir el comportamiento anelástico del material planetario. Esta idea es desarrollada para el caso particular de la Tierra, aunque se hace referencia también a otros planetas. Una litósfera así definida es mucho más delgada que la basada en la profundidad de la "capa de baja velocidad", y su espesor puede ser varias veces mayor bajo los continentes que bajo los océanos. Esto afecta significativamente la estabilidad (y por ende la edad radiogénica) de la litósfera en ambas regiones. Se propone una explicación muy diferente de la capa de baja velocidad bajo los océanos.

ABSTRACT

The current tendency to define a lithosphere as an elastically distinct region is in danger of distorting an important qualitative insight into the tectonic processes of a planet. Just as we recognise on a laboratory scale that a given material can behave quasi elastically (solid-like) or quasi viscously (liquid-like) under different regimes of loading, it is suggested that the word lithosphere be used to describe that part of a planet, if any, that behaves quasi elastically under the stresses associated with the heat transfer process. The delimiting of such a region necessarily involves a discussion of the non elastic behaviour of planetary material. This idea is developed in particular for the Earth, although reference is made to other planets. Such a lithosphere is much thinner than one based on the depth of 'the low velocity layer' and is probably several times thicker under continents than oceans. This is very relevant to the stability, and hence radiogenic age of the lithosphere in the two regions. A quite distinct explanation of the oceanic low velocity layer is offered.

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In recent years the word 'lithosphere' has gained a wide usage in the geophysical literature, although it, and what has been claimed to be an equivalent term 'stereosphere' (Wyllie 1971 p. 63), have been used much longer by geologists. There also seems little doubt that the word 'crust' originally meant what many now wish to convey by the word 'lithosphere' (Jeffreys 1962 p. 204) but in a well meaning attempt to introduce precision into the use of a word that had and still has a widely understood, if qualitative, everyday meaning, seismologists exchanged the original geotectonic concept of a crust for one based on the velocity of earthquake P waves. The delimiting of a zone of the Earth in which that velocity is less than about 8 km/sec has now taxed the ingenuity of seismologists, as well as the public purse, for several decades, with the result that it is now clearer than ever before that 'Moho determination' is more of an esoteric art than a significant contribution to our understanding of geodynamics. I think this view could have been expected to follow from a definition that was based on nothing more profound than the cult of numerology and a continuing obsession with apparent discontinuities in the seismic wave velocity-depth relationship. In passing, we may note that the same outlook has now been used to define a lunar 'crust' of about 70 km thickness.

I have related this brief history of the word 'crust' since it seems to be particularly relevant to the present problem of defining what we mean by the Earth's, indeed any planet's lithosphere. Quite undaunted or perhaps ignorant, of the way the original meaning of 'crust' was corrupted by a poor definition, but still filled with a belief in the importance of the velocity distributions of earthquake waves for everyone's geodynamic problem, seismologists have insinuated the assertion that the lower boundary of the lithosphere is coincident with the top of the so called 'low velocity layer' – not to be confused with even lower velocity layers that lie over it which are now called 'the crust'! Such an assertion about the boundary of the lithosphere was suitably in accord with earlier estimates of a lithospheric thickness based on the idea of isostasy, and seemed to promise, if not actually yield, numerical results that would add both weight and precision to these earlier estimates. Once again a rough numerical coincidence has created a very real danger that a genuine insight into the Earth's non elastic behaviour will be obfuscated by irrelevant considerations of its quasi-elastic structure*. The 'low velocity layer' may also mark a minimum in the Q distribution of the Earth obtained at seismic frequencies, but as has been pointed out on many occasions by myself and other writers, the loss mechanism for small amplitude waves cannot be closely correlated with irrecoverable deformation under stress. No experimentalist measures the velocity and attenuation of ultrasonic waves as an alternative to 'creep' experiments in the study of the irrecoverable deformation of laboratory material – why try something similar in the study of Earth material?

It will be noticed that I have already tacitly insinuated my own view that the concept of a lithosphere has something to do with the spatial distribution of the long term rheological behaviour of planetary material in situ. Let me say at once that I cannot decide whether this is in accord with other modern attempts to define the Earth's lithosphere as the 'rigid' or 'strong' part of the Earth, though I believe its consideration lies close to the spirit of the original investigators who used the words 'crust' and 'lithosphere'. The difficulty lies in the meaning one attaches to such words as 'rigidity' and 'strength' in other person's mouths. Leaving aside as incomprehensible the modern school that describes the outer layers of the Earth as 'rigid'**, one has seen the lithosphere described as 'more rigid' than the underlying material. One could be forgiven for thinking that this means the modulus of rigidity assigned to the lithosphere is higher than that outside it. Of course, that definition cannot fix a finite lithospheric thickness (except in the physically unimportant case of exact elastic uniformity over a finite region), and

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* The reader should understand that I am only challenging the relevance of velocity determinations on their own to the concept of a lithosphere, and not the idea that we can learn something valuable from such seismological data (see later in this text).

** I have pointed out elsewhere (Tozer 1972) that this school, which is closely associated with Plate Tectonics, seems to have difficulty in distinguishing a rigid body from the idea of a body undergoing an approximate rigid body motion.

if we extend it in a physically plausible way to include the seismologist's 'crust' in a lithosphere we finish by including the whole of the 'low velocity layer' as well. I suspect that was not intended. The lithosphere has also been described as being 'stronger' than the underlying material, an idea that was much in Barrell's mind when he coined the term 'asthenosphere' for the zone immediately under his lithosphere. As quoted by Jeffreys (p. 178) he appears to have been sufficiently clear in his own mind about the meaning of 'strength' to be able to say that the asthenosphere had a strength of the order one sixth that of surface rocks. Turning to a more modern writer (Orowan 1958) we read that the tensile strength of the Earth's 'crust' under a horizontal tension rises from a low value of between zero and a few tens of bars at the surface to several thousand bars at a depth of between ten or twenty kilometres, and that below this depth it falls again to very low values. Again we see the spectre of the seismologist's crust being excluded from the lithosphere, and an example of one of the more consistent laws of geophysics -- any two writers on the subject of 'strength' can only agree that its units of measurement are those of stress.

Although I have, perhaps, expressed the latter situation rather facetiously, I believe we can learn an important lesson from the idea of 'strength' that will serve us well in a definition of lithosphere. We all know that in innumerable cases where materials are used for say, constructional purposes, the introduction of the concept of 'strength' into a rheological constitutive equation to describe a stress beyond which there is a sudden, irreversible change in the configuration of the structure is one of great utility, but we also recognise, at least in our more lucid moments, that one cannot regard such a strength as an intrinsic characteristic of the material. If the temporal domain of the loading is sufficiently changed, e.g. made oscillatory or of indefinite duration, structures can fail at arbitrarily low stresses. Similarly it may be demonstrated that if the spatial scale of a system constructed of any typical material is changed by a large enough factor, a complex change of behaviour is to be expected. This follows from the fact that if physical data obtained at one particular length scale, e.g. that of the laboratory specimen, is viewed in the context of the continuum mechanical theory to which it all belongs, there are no exact laws of dynamical scaling (see also Tozer 1972). We can make the general observation that the parameters, which are rather misleadingly called 'the physical constants' of a material, are not a type of label that one can attach to a particular substance wherever it is found, but are merely a convenient and approximate way of expressing the reproducibility in the reponse of limited amounts and configurations of that substance to stimuli that are also limited in amount, duration and time. This fact should make one exceedingly wary of those who claim to have measured *the* properties of the Earth in the laboratory, and in particular, it suggests that a definition of lithosphere has to be based not only (as above) on the value of some physical constant that purports to describe a certain aspect of the rheology of planetary material in situ, but also with some regard for the stress environment of that material.

If our definition of a lithosphere is to have the widest utility an important preliminary question is to ask whether there are any types of (qualitatively similar) rheological behaviour that one can expect any matter to exhibit on an arbitrary macroscopic length scale. If we restrict our attention to materials with a so called fading memory of their past configurations*, and consider only very small or very slow deformations of this material, it may be proved with very general arguments (see for example Truesdell and Noll 1965) that they will exhibit linear viscoelastic behaviour, which tends in the limit of slow deformation to a Newtonian viscosity. This is an explanation of why these particular constitutive equations have had such widespread utility in the interpretation of material behaviour if one is prepared to limit the size or rate of deformation. Of course, such general arguments are silent on what one can call a small or slow deformation and apart from the limiting Newtonian form, it leaves the character of the viscoelasticity at the discretion of the

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* Without such a postulate, a materials science becomes impossible since every bit of matter would presumably behave in a different way to every other after several billion years of geological history.

observer who wishes to interpret his observations. However, this being a linear constitutive relationship it suggests that a certain characteristic time exists for any material that marks a transition from essentially elastic to viscous behaviour, and in accordance with this general feature of matter, I propose to define a lithosphere as any region in which the ambient stresses only produce strains small compared with unity in one characteristic time of the material. One could conceive, even for isotropic viscoelastic materials, of two independent types of lithosphere – the first based on a consideration of the volume strains produced in a characteristic time that is defined by the ratio of an effective bulk modulus and the second or volume coefficient of viscosity, and the other on a study of the non hydrostatic stresses and the behaviour in shear. However, I shall confine my attention exclusively to the latter type.

We now turn to the discussion of the possible existence and extent of such a lithosphere in the Earth, questions that can only be resolved in the context of specific models that fix the non hydrostatic stress environment and the shear viscoelasticity of in situ Earth material. It will be assumed that an effective shear modulus distribution has been found from a study of disturbances in the seismological frequency band, at least in the mantle of the Earth*. It should be no surprise that controversy would arise among creatures who only have a finite time to observe any system, about the behaviour of Earth, indeed of any planetary material, in the limit of slow deformation. Some figures for effective viscosities of Earth material in situ have been derived from a study of the response to ice loads applied to the external surface and the damping of the Chandler motion, but to my mind a much more convincing insight into slow deformation behaviour

*It may puzzle some readers that I treat this as an assumption after being told that seismologists 'know' the rigidity modulus of the Earth to $\sim 5\%$. Such an assignment of uncertainty is entirely model dependent, a fact that is clearly seen by reference to the core. Is the absence of signals corresponding to S wave propagation in the outer core to be attributed to an elastic medium with rigidity modulus of zero ($\pm$?) or that these particular waves are very heavily damped in a medium which has an effective rigidity modulus of conventional value ($\sim 10^{12}$ C.G.S. units or some other unknown value)? This illustrates my point about physical constants being only a way of representing behaviour in a limited domain – that they are in a sense subjective in both quality and value.

of Earth material has come from a study of terrestrial heat flow at the external surface and a theory of the way such heat has been transferred from its internal radiogenic sources. The attractiveness of this relatively indirect approach stems directly from the fact that certain features of a solution to this heat transport problem that are pertinent to the idea of a terrestrial lithosphere (as defined above) show no geologically significant dependence on any physically plausible change in the choice of the necessary input data**. For reasons that seem to be connected with the unthinking use of solutions to truncated and/or approximate versions of this heat transport problem, e.g. using heat conduction theory or convection in a constant viscosity fluid, it is not widely recognised how strongly the absolute temperature field is coupled to the choice of a function representing the dependence of the slow deformation behaviour, i.e. the effective viscosity, on the local thermodymanic conditions. The problem is complicated in its details and one will have to refer to other papers for those details (Tozer 1970, Tozer 1972) but the crucial point can be made by reference to an oversimplified Earth model in which the viscosity decreases according to the same exponential function of temperature ($e^{E/kT}$) everywhere in the interior. Since the heat sources are independent of temperature, it is easy to see that any heat transport process acts in such a way as to keep the numerical value of the viscosity function above a certain amount. What was exluded a priori in the other theories of the heat transport process was any indication of how rapidly the heat source density has to increase in order to maintain horizontally averaged (H.A.) viscosity values less than about 10^{21} poise throughout a significant fraction of the interior of such a planet. One can turn this argument around to imply that if for any reason thermal conditions corresponding to effective viscosity values $\leqslant 10^{21}$ poise ever existed throughout

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** The concept of physical plausibility means choosing numerical data that lie in the ranges of laboratory data pertaining to rheology, expansion coefficient, thermal conductivity, specific heat, radiogenic heat source density. For the reasons already given, it is arguable whether physical plausibility of this type is an essential feature of a theory of such large scale processes, but it is certainly gratifying when such a label can be attached to a theory that relates so much purely geophysical data.

much of the interior of a planet with otherwise physically plausible properties, it would relax to give viscosities $\sim 10^{21}$ poise in times of a few hundred million years. Since such relaxation times are short compared with the half lives of the principal heat producing isotopes and the ages of planets, one is much more confident than with the older theories of discussing present thermal conditions as a quasi steady state problem. Of course, this particular model of the Earth is oversimplified in that it is impossible to interpret the existence of the seismologist's 'core', and it makes no allowance for the probable effect of hydrostatic pressure in modifying the dependence of viscosity on temperature. Nevertheless a lower bound $\sim 10^{20}$ poise is still valid for the H.A. viscosity in the outer parts of more sophisticated models that do have a 'core'*. When we come to discuss the actual rather than the lower bound to the effective viscosity in the quasi steady state we find that in all plausible Earth models there is an extremely rapid decrease of H.A. effective viscosity in an outermost shell some tens of kilometres – perhaps from values as high as 10^{60} – 10^{70} poise at its external surface. This decrease with depth is suppressed once values as low (!) as 10^{21} – 10^{22} poise are attained – owing to the rapidly growing efficiency of the heat transfer process at these viscosities. Since this is true for quasi steady solutions to all the plausible Earth models, one confidently expects that the heat transport process 'stabilises' viscosity values of this order down to depths of several hundred kilometres, but for a planet of the Earth's mass, there is no clear indication whether such values could be sustained against the effects of hydrostatic pressure at greater depths. The complicating factors are the magnitude of the increases in creep resistance that one can plausibly associate with the increases of density in its 500 – 1000 km depth range, and the magnitude of the decline in radial heat flux as the core-mantle boundary is approached. For these reasons, one cannot dismiss the possibility that

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* Interesting conclusions can be drawn from the fact that a 'liquid' core can only survive at the present day, if surrounded by material with such enormous viscosities at the temperatures at which core material is 'liquid'. This can be made one of the best arguments for believing the Earth's core has a much lower melting point than pure iron.

effective viscosity may gradually rise throughout the lower mantle by as much as a few powers of tens.*

Associated with all these quasi steady solutions of the heat transfer problem are mass transfer velocities $\sim 10^{-7}$ cm/sec and (H.A.) strain rates that are $< 10^{-14}$ sec^{-1} at all depths in the crust and mantle. Using a figure of 10^{12} dynes/cm^2 as representative of the effective rigidity throughout the whole of this region, it is readily shown that providing the predicted effective viscosities are less than 10^{26} poise, these heat transfer solutions which are based on the ideas of viscous rather than viscoelastic hydrodynamics, are self consistent. Apart from some slight doubt about the lower mantle, this condition is valid throughout all but an outermost shell of the Earth, and it may be shown that the lack of self consistency in this shell does not lead to significant errors in the (H.A.) viscous solutions at greater depth. If we now take the strain rate of 10^{-14} sec^{-1} that is predicted for the quasi viscous parts of the interior as the basic strain rate of large scale geodynamic processes then the associated lithosphere of the Earth is coincident with this shell in which viscosities are $> 10^{26}$ poise. Of course, one realises that due to effects of stress concentration not included in the heat transfer model, e.g. ice loading, erosion etc, the local geodynamic strain rate can differ considerably from 10^{-14} sec^{-1}, but a lithosphere that is defined by the strain rate of these large scale motions is probably what modern users of the term have meant to imply. In any case since effective viscosity is varying at such an enormous rate in this particular range of depths, the actual thickness assigned to the lithosphere is not significantly sensitive to ones choice of a figure for the strain rate.

Although the dynamics of a lithosphere is probably of little consequence for the general conditions deep within a planet, a prediction of how seriously a planet's external surface is deformed by

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* Some calculations among the plausible models show that the highest viscosity values could be attained in the 1000-1500 km depth range, and there is a fall in the lower mantle. This follows from the suppression of convective motion if creep resistance rises suddenly in the transition zone, with a consequent rise in the (steady) radial temperature gradient. Given sufficient heat from the core, the viscosity falls and even convective motions can start again in the lowest mantle with $\eta \sim 10^{22}$ poise.

the heat transfer process is probably the most direct way in which that theory can be tested by an observer. Unfortunately, there is, as yet, no detailed theory that enables us to make such a prediction in an arbitrary case, and in fact, it seems likely that no satisfactory prediction could be made with a theory that only ascribed linear viscoelasticity to planetary material. However, I believe the very thinness of the terrestrial lithosphere under the oceans, as defined by a strain rate of 10^{-14} $\sec^{-1}$, makes firm prediction possible. Calculation of the thickness of an oceanic lithosphere, particularly if there is at least a few tenths of percent water in sub oceanic material, give figures of less than 10 km, which must imply large stress concentrations if the lithosphere were only quasi-elastically deformed by the underlying motions. Calculation indicates that tensional stresses of several kilobars would have to be sustained elastically. If one takes the typical stress drops ($\sim$ 1 kb) associated with earthquakes as an indication of the stress at which failure of the quasi-elastic model occurs, or, a fortiori, Orowan's estimate of strength in this range of depth, the resulting contradiction indicates the assumption of a purely elastically deforming oceanic lithosphere is incorrect. One senses that for lithospheres of this thickness or less, the surface material will show, with little attenuation, the mean motions of the less viscous underlying material. Relative speeds $\sim 10^{-7}$ cm/sec are predicted for the Earth and the residence time of material in the lithosphere ($\sim$ turnover time of the external surface) is of the order a few hundred million years.

Although one is encouraged by the agreement of such predictions with many observations in oceanic regions, one must also be impressed with the fact that there are several large areas of the Earth's surface formed of rock that has resided in the cool conditions of a lithosphere for ten or twenty times longer -- much too frequent and large scale a phenomenon to be explained by the workings of chance. Since such old lithospheric material is invariably to be found in continents it suggests that the process which has generated the basic continental-oceanic relief of the Earth is also one that has made the lithosphere in the 'high' areas peculiarly immune to entrainment or deformation by the underlying flow. The same fact is also brought out by the way we speak of 'continental drift' – the coherence of these 'high' areas over periods of time in which the external surface as a whole has profoundly deformed. It is in the discussion of this basic geological fact that one is most aware of ones inability to predict how thick a lithosphere must be before it will remain intact under the forces impressed by the heat transfer process.

I believe some important clues about this problem can be gleaned from our present knowledge of other planets, and to introduce that subject, I would like to consider very briefly the lithospheres of bodies of different size and roughly similar properties to Earth material. For the same external surface temperature the H.A. thickness of a lithosphere defined by the strain rates of the quasi steady heat transfer process is roughly inversely proportional to both the radius of the planet and to its heat source density*. On the hypothesis that the latter can be attributed to the decay of the isotopes of uranium, thorium and potassium one predicts that the heat source density and hence inverse lithospheric thickness would have decreased by more than a factor of five in the last few billion years. For the Earth this only means that the lithosphere was even more likely to be disrupted in the past (see Sutton and Watson 1974), and the problem of the survival of some of its lithosphere for billions of years is that much more acute. However, if one looks at smaller bodies one might expect to see examples of lithospheres that have become quite generally stable to disruption at some time in the past. At this stage it is necessary to remind oneself that the argument is only as simple as this providing the planet remains spatially homogeneous in its properties throughout its history. However, the question of spatial homogeneity is related to that of lithospheric stability in an interesting way, since it is obvious that the possibility of significant differentiation on a planetary scale is contingent on the

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* Another way of looking at this is to say the lithospheric thickness is inversely proportional to external surface heat flow. Obviously simple proportionality must go wrong for the smallest bodies, but calculation indicates that it is sensibly true for bodies of Earth material with radii >1000 km. Bodies less than about 700 km radius have essentially zero strain rate associated with the heat transfer process at the present time.

existence of a lithosphere that is strong enough to resist entrainment and mixing with the rest of the planetary material that is moving in response to the flow of heat. In contrast to this tendency of the heat transfer process to remove a pre-existing or incipient chemical differentiation, one should note that the rate at which separation of different phases can occur is absolutely controlled by the rate at which the undifferentiated material acquires the necessary rheological behaviour*. We have seen that the H. A. viscosity is kept at a value in excess of 10^{20} poise which is much too high for differentiation to occur homogeneously. Recognising that primary material may only contain density differences on a scale of centimetres and that differentiation velocities have to exceed those of the heat transfer process if remixing is to be avoided, one estimates viscosities $\leqslant 10^{10}$ poise in a differentiation zone, which must therefore be very highly localised. There is evidence based on the laboratory study of convective flows, that these highly exceptional rheological conditions will be produced by viscous dissipation of the large scale mass flows of the heat transfer process, and preferentially near the sides of any piece of lithosphere that might be thicker than average. This could be the reason why there are still only a small number of large pieces of very old terrestrial lithosphere. It should be noted that the tendency of a planet to chemically differentiate will not affect the time at which 'stable' lithosphere appears i.e. lithosphere in which material will reside indefinitely, as much as its subsequent rates of thickening and lateral growth. Both the partitioning of the uranium and thorium fraction of the heat source density and that of the low melting point (relatively creep sensitive) phases into a stable lithosphere will make its rate of growth rather faster than would be predicted from the purely temporal decay of the radiogenic heat source density. Of

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* This remark has to be qualified if the rate of gravitational energy release due to differentiation becomes large enough to reduce the viscosity significantly. Then one may get an auto catalytic effect and catastrophic rates of differentiation. This 'run away' of a chemical differentiation could happen during a profound reorganisation of the larger objects in the solar system, e.g. the Earth's core formation, but is probably negligible with regard to silicate differentiation into a lithosphere. Rough estimates of gravitational energy released in forming a terrestrial lithosphere are less than 1% of the radiogenic heat released throughout geological time.

course, it is this temporal decay of the heat source density which ensures that any planetary lithosphere, based on its heat transfer strain rates, will eventually grow inward to include all the planetary material. When this has happened we might well describe the planet as 'dead'. However, the duration of its 'life' and the degree of chemical differentiation that has been achieved at the moment of 'death' is a very rapidly increasing function of its external radius. For example a body of terrestrial size and chondritic like material would have a 'life' $> 5.10^{10}$ years whereas one less than about 800 kms radius would already be 'dead'.

With these thoughts in mind, let us now see how extra-terrestrial evidence bears on the question of how thick a lithosphere must be to avoid disruption by the motions of the heat transfer process. Both Mars and the Moon, smaller bodies than the Earth, with roughly the same mean external temperature, show topographic evidence of possessing much older surface material than much of the Earth's lithosphere, although the most valuable evidence must be the actual dating of lunar rocks and the measurement of heat flow on the lunar surface. The dating suggests that the lunar lithosphere stabilised more than three billion years ago, i.e. at a time when the surface heat flow was perhaps four times the present lunar value or twice the present mean terrestrial heat flow. The problem is therefore not so much to explain the instability of the lunar lithosphere before this time, as to understand why it came to an end at such an early date. I believe the answer to that problem lies in the relative water content of the rocks forming the outer parts of these bodies. Even at the few tenths of weight percent level water is very potent in reducing the creep resistance of silicate systems – hence its use as a 'fluxing' agent in experimental petrology. However, in contrast to the Earth, the chemical analysis of lunar surface rocks reveals only a rheologically insignificant water content, and following a suggestion of Pandit and Tozer (1970), it now seems to be widely accepted that the curious diffusive propagation of seismic waves in the near surface material is only possible if this virtual absence of water seen in specimens is quite typical of surface material. A further clue to the role played by water in the timing of lithospheric stabilisation is provided by the relatively crude mapping by radar of the topography of the Venusian surface. If this evidence of cratering is taken as indicative of a very old stable lithosphere on a roughly Earth sized planet, one has to explain how it was stabilised despite a surface temperature ~ 500°C. This points very strongly to the absence of rheologically significant amounts of water in Venusian rocks, a conclusion that is supported by the insignificant amount of water, compared with the Earth's hydrosphere, that is in the Venusian atmosphere. The planet Mars is particularly relevant to these ideas in that its surface shows areas of cratering (several billion year old lithosphere?) as well as signs of free water flow. As yet one cannot use such data quantitatively but it seems likely that age determinations over the Martian surface and the determination of its surface heat flow and seismic propagation characteristics would provide a crucial test of these ideas.

The existence of oceans and the water content of volcanic eruptions clearly shows that the process of chemical differentiation on Earth includes a dehydration of the interior, and there is no doubt that if this water were remixed with the upper mantle it would cause a dramatic lowering of its creep resistance. Of course, we do not know whether all the oceanic water has come from the interior or the initial water content of Earth material, but if one accepts the fact that water not previously constituting the hydrosphere is still being evolved and the extra terrestial evidence, already given, that an entirely dehydrated Earth would have already developed a completely stabilised lithosphere, it becomes interesting to explore the assumption that dehydration and the other chemical differentiation has only appreciably changed the composition of these parts of the upper mantle lying under continents. It has been suggested before that the differences in quasi-elastic properties of the sub oceanic and continental mantles (see also below), which extend to depths of a few hundred kilometres, are only compatible with poor mixing of these regions, and that this sub continental mantle must be carried along during the movements of continental drift. Similarly the rough equality of mean oceanic and continental heat flow has its simplest explanation if the high concentration of heat sources, seen in the superficial layers of continents is compensated by a deficiency in an underlying region from which it is not separated by drift movements.

Obviously the acceptability of these ideas must be judged by the plausibility of the constraints that they put on the rheological response of the various regions to long continued stress, or the necessary constraints they place on the distribution of effective viscosity. In Fig. 1 are shown the results of calculations to illustrate the magnitude of rheological effects and the differences that could arise in the temperature-depth curve* as a result of selective hydration and differentiation of sub continental and sub oceanic mantle. Curves 0' and C', drawn dotted at the greatest depths, are typical of the numerous temperature-depth curves that have been computed for the oceanic and continental shield regions on the implausible assumption that the creep resistance is everywhere sufficiently high to prevent any thermally induced motions. This traditional way of calculating temperatures, the 'state of rest' solution of the convection problem, is obviously inconsistent with the current view of a dynamically deforming upper mantle, whatever its cause, and it is only given here in that it serves as a standard of reference and to show that it provides an upper bound to calculations that try to incorporate a plausible degree of creep resistance. The differences in curves 0' and C' are attributable to the rather smaller surface heat flow observed in continental shield regions and the rather deeper burial of radiogenic heat sources under the oceans. We shall see that the sign of this temperature difference can be reversed below a certain depth (~ 40 kms in the case shown) if the creep resistance of sub oceanic material is less than that under the shields and not large enough to stabilise the 'state of rest' temperature gradient. As already implied the choice of a plausible creep resistance always results in the hydrodynamic instability of these state of rest solutions and the problem is to find the H. A. characteristics of a stable

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*It must be kept in mind that these curves now represent separate horizontal averaging over continental and oceanic regions. Much higher temperatures than those shown are predicted to occur very locally, particularly in downgoing material.

solution which necessarily entails mass movements. The results of calculation to find these stable solutions for sub-oceanic and sub continental shield regions are shown as curves 0 and C. The effective viscosity functions (see caption) that were chosen for these regions are an attempt, using certain empirically observed regularities, to represent the effective viscosities of a water saturated basalt (solidus ~ 700°C) in the sub oceanic region and an anhydrous dunitic rock (solidus ~ 1400°C) under the continental shields. These calculations show that both the 0 and C curves bifurcate from the state of rest solution (0' and C') at that depth at which the effective viscosity of the respective material had fallen to a value $\sim 10^{21}$ poise, a depth which is obviously reached first for the material with lower creep resistance. At greater depths the temperature gradient is about twice the adiabatic gradient. The vertical arrows, labelled by the letters of the curve to which they refer, mark the depth at which the effective viscosity has fallen to 10^{27} poise, a position which may be taken to mark the base of a lithosphere at the strain rates and effective viscosities characteristic of these solutions to the heat transfer problem in the two regions *regarded separately* (see below). The particular figures of 7 and 39 kilometres given for the H. A. thicknesses of oceanic and continental lithosphere should not be noticed as much as the thinness of the former and the large ratio (~ 5.5) of the two thicknesses. It has already been noticed that an oceanic lithosphere of this general thickness offers no appreciable resistance to disruption by the underlying movements and the velocities of sea floor spreading are expected to be of the same order as these relative velocities which can confidently be predicted for the sub lithospheric material ($\sim 10^{-7}$ cm/sec).The large ratio of continental and oceanic lithospheric thicknesses that can be produced by such relative dehydration and sialic depletion of primary materials combined with the evidence pointing to the importance of dehydration in stabilising other planetary lithospheres, goes a long way to explain the much longer* residence time of material in the continen-

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*Were it not for processes of sub aerial erosion carrying continental material to ocean basins, one might say indefinite residence time.

tal lithosphere. Even if we were to regard the material constituting either lithosphere as having uniform strength the factor 5.5 reduces to about half a kilobar the tensile strength of continental lithosphere material required to prevent disruption – a value rather less than the stress changes seen in many earthquakes. However, if one accepts Orowan's view that the uppermost few kilometres of a lithosphere has virtually no strength in tension, the relative capacities of the two lithospheres to withstand the hydrodynamic forces associated with the heat transfer process is considerably greater than would be inferred by this simple comparison of lithospheric thicknesses. In this picture of essentially vertical differentiation two additional factors promote the relative stability of continental lithosphere. It is known that if sialic materials were transported from the pressure-temperature conditions at depths ~ 100 km to those in the vicinity of the external surface they undergo greater density changes (due to phase change) than the ultrabasic (dunitic) material which is necessarily displaced to greater depths. Hence, there is a net expansion of a column of material undergoing such 'vertical' differentiation and perhaps rather remarkably, both the sialic differentiate and the underlying dunitic residuum are less dense than the undifferentiated (eclogitic?) starting material. This net expansion gives the continental lithosphere an overall buoyancy in the undifferentiated material that exists elsewhere and seems quantitatively capable of explaining the basic relief of the external surface*. The other factor that could help to maintain a continental lithosphere intact can only be appreciated if we examine the consequences of bringing together these separate solutions of the heat transport problem for sub oceanic and continental regions. It will be noticed that if a continental lithosphere were to 'drift', undifferentiated sub oceanic material would necessarily be drawn under it, and in any plausible model where effective viscosity is strongly temperature dependent, one recognises two important consequences of this effect. The sub oceanic material is cooler at the same depth than sub oceanic material (Fig. 1) and when moving

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*This correlation of height with lithospheric antiquity seems to be also true for the Moon and Mars.

under a continually drifting continent it will keep the temperature-depth curve below that indicated by the purely continental solution (curve C, Fig. 1). Conversely the thicker continental lithosphere acts as a better thermal 'blanket' than the oceanic lithosphere and the oceanic material which finds its way under a continent will have its effective viscosity reduced below the figure of 10^{20} poise that characterises the solutions for chemically homogeneous regions. Both these effects (by reducing the shearing stresses on the underside of a continental lithosphere that has been thickened by such cooling) lead to relatively greater stability of a continental lithosphere than is predicted from the separate solutions. It is hard to calculate how effective viscosity reduced below the figure of 10^{20} poise that movements and effectively thicken during the drift process, but it seems quite possible that the differences in sub continental and sub oceanic material that are indicated by the quasi elastic properties in the outer few hundred kilometers could only have been maintained if effects such as these were present. For instance it will be seen from Fig. 1 that if oceanic material (solidus ~ 700°C) went under a drifting continent at any depth greater than ~ 70 km it would, at least eventually, be heated above its solidus, and have an effective viscosity $< 10^6$ poise. This suggests that the 'lubricating' layer of oceanic material under a continental lithosphere might be extremely thin compared with the overlying lithosphere, a feature that would also explain why the external surface heat flow has not been appreciably disturbed by drift movements from the 'normal' value, i.e. that which would exist if no differentiation had occurred.

Lastly, I come to a brief discussion of the 'low velocity' layer, the phenomenon which in a sense has motivated this discussion. For a possible interpretation of this structure, I have drawn in Fig. 1 the dehydration curve of amphibole. One could have cluttered the diagram with other dehydration curves (see for example Wyllie 1971) but I believe the position and character of this curve give it a pre-eminent importance among dehydration curves in trying to interpret the existence of a low velocity, low Q layer. It must be immediately noted that only the relationship of this dehydration curve to curve O has any significance, since the sub continental temperature-depth curve, C, has been calculated on the assumption that this material is completely dehydrated. It will be noted that the H. A. temperature in oceanic regions is calculated to be so low that during the movements of a convective circulation any water which can form amphibolite phases above 500°C will alternatively pass, on traversing a depth $\sim$ 100 km, from or to these hydrated phases to create or reduce intergranular water. A little reflection on this situation should convince the reader that if one were to start from any initial composition which had more than enough water to form amphibolite phases, this distinctive maximum of the amphibole dehydration curve would make a barrier to dehydration proceeding beyond a certain point. This point would be reached at that water content which makes the smallest concentration of amphibolite phases that can exist in equilibrium with free water at temperatures $< 500°C$ and pressures < 30 kb. I have already emphasised that material at much higher temperatures than the horizontal average is expected to be produced very locally as a result of viscous dissipation in convective flows of this large scale, and it is this abnormal heating that makes the barrier presented by the amphibole dehydration curve 'leaky'. This material, containing free water that can escape from the rock, passes 'over the top' of this barrier*. Given enough time and long lived radiogenic heat sources all material will eventually pass over this barrier, but it is possible to calculate that this could not have happened yet in any terrestrial planet, and will never happen in objects much smaller than the Earth. In the case of the Earth, one supposes that it is just the sub continental material which has been through this abnormally hot state and hence lost virtually all its water, whereas the oceanic material is still on a 'plateau' of a graph of water content versus time that would exist at the value of water content indicated above. At first one would not expect that such a small content ($< 1\%$) would have any identifiable effect on those

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* It is only this exceptionally hot material which finds it way to the external surface in volcanic eruptions, a fact which has wrongly convinced earth scientists that its temperatures are typical of the upper mantle generally.

physical parameters of planetary material that are observable in situ, but laboratory work has shown (Pandit 1970) that if liquid water is present in rocks at only the tenths of weight per cent level, it reduces the 'Q' by a factor ~ 10 and perhaps more surprisingly, alters the elastic constants by several percent. However, it should be added that these experiments did not refer to dehydration reactions such as directly interest us here, but only to measurements of these properties under conditions of vacuum where very small changes of water content could be precisely controlled (see also Nowatzki 1972). Nevertheless the significance of small quantities of such a low viscosity phase as water is clearly demonstrated by these measurements, and their relevance to lunar seismic problems has already been mentioned. I suggest that this is also the cause of the 'low velocity' layer under the oceans since the appearance of intergranular water from amphibolite dehydration occurs at just the same depth, and the increase of Q at greater depths can be interpreted as due to the freezing of this liquid water by the higher hydrostatic pressure. This hypothesis also rather beautifully interprets the observation of a low Q 'core' in the Moon, where water could well be trapped 'behind' the barrier of an amphibole dehydration curve (Tozer 1974).

CONCLUSIONS

An attempt has been made to develop the idea of a lithosphere as that part of a planet which behaves more like a solid (quasi-elastically) than a liquid (quasi-viscously) under the conditions of stress that arise from the heat transfer process. It is suggested that this concept of a lithosphere is nearer the original meaning and possibly the present tectonic meaning than that of seismologists who define it as just a quasi-elastically distinct region. Once one recognises the importance of the creep resistance in developing the idea of a lithosphere, the small changes in quasi-elasticity (~ 10%) that so intrigue seismologists are seen to be utterly negligible in comparison to the changes (a factor larger than 10^{30}) that occur in the effective viscosity in the same outer regions of the Earth and other terrestrial planets.

The slow recognition of how the temperature dependence of creep controls the heat transfer process has delayed many new insights into geodynamic problems and some of these that are connected with the strength and stability of a lithosphere as defined above are explored. In particular, it has become much easier to understand why sea floor spreading velocities are comparable to those that can be predicted with some confidence for that part of a planet's interior that is behaving quasi-viscously. Furthermore, it is possible to understand why at least some of the material constituting a continental lithosphere has been able to reside near the external surface for times that are much longer than sea floor materials in the oceanic lithosphere. The role of water in the development of a lithosphere is emphasised, and in comparison with the results of conventional but obviously outdated heat conduction calculations, the much lower internal temperatures in appropriately dynamic Earth and planetary models give a new prominence to hydration reactions in deeply buried planetary material. For example, it has now become possible, indeed highly plausible, to suggest that the low velocity, low Q layer observed in sub oceanic material for disturbances of seismic frequency, is due to a peculiarity of the amphibole dehydration equilibrium curve that allows water to be retained as an intergranular liquid for long periods in those parts of a planet that are subjected to pressures in the range 30–100 kb. Hence we see that the interpretation of a tectonic lithosphere can be quite distinct from seismological layering, and it is misleading to make definitions that bring them together.

Another feature that is brought out by discussing planetary dynamics as an aspect of the heat transfer problem is the progressive trend in the character of lithospheric deformation that occurs with increasing planetary age. Let us take as our model homogeneous masses of roughly chondritic composition with the radiogenic heat source density we associate with uranium, thorium and potassium some 4.5 x 10^9 years ago. For bodies exceeding about 1000 km radius the lithosphere is initially so thin as to be constantly disrupted by the hydrodynamic forces generated by the heat transfer process, and even the very small fraction of planetary material that enters the lithosphere will only spend ~ 10^8 years there before remixing with the rest of planetary material. After a period of time that increases rapidly with external radius for values greater than 1000 km, the decay of radiogenic heat sources will bring about a thicker lithosphere that is not subjected to this continual renewal of its material and a period of increasing chemical differentiation can commence. As might be guessed, the lithospheric thickness of small objects is the most sensitive to changes of the heat source density, and only a small proportion of the material that is being incorporated into the continuously thickening lithosphere of an object of say 1500 km radius or less will have had time to pass through the exceptional rheological conditions necessary for silicate differentiation. For an Earth sized object the decay of radiogenic heat sources would not, on its own, produce a stable lithosphere for $> 10^{10}$ years, particularly if there is any water about, and this gives time for a much higher proportion of the starting material to experience the rheological conditions necessary for differentiation. However, this greater potentiality for differentiation cannot be realised until at least some stable lithosphere has been formed. It is suggested that a peculiarity in the equation of state of primary planetary material that gives an overall buoyancy to differentiated regions has enabled a small number of regions of stable lithosphere to form, and that these are of necessity the areas of highest relief on the external surface. This is the state of the Earth today and for the next several billion years.

Since the lightest elements that concentrate in the lithosphere (and hydrosphere) tend to be those with the lowest creep resistance, the creep resistance of the deep interior will show an increase with time. This is a region in which the heat transfer process maintains roughly the same effective viscosity, ~ 10^{21} poise, regardless (within wide limits) of the creep resistance, and hence the internal temperature will show a secular increase. It can be demonstrated that for the first few billion years of existence of a planet that is large enough to differentiate significantly (radius $>$ 1500 km) this effect more than compensates for the cooling due to decay of the heat sources. The accompanying thermal expansion means that after an object has developed a stable lithosphere over its entire external surface, the dominant non hydrostatic stress within it becomes tensional. It is suggested that the Moon and Mars have already reached this state, as shown by the rifts in their surfaces, and if Mercury is devoid of water, one predicts tensional features will also be seen in its topography.

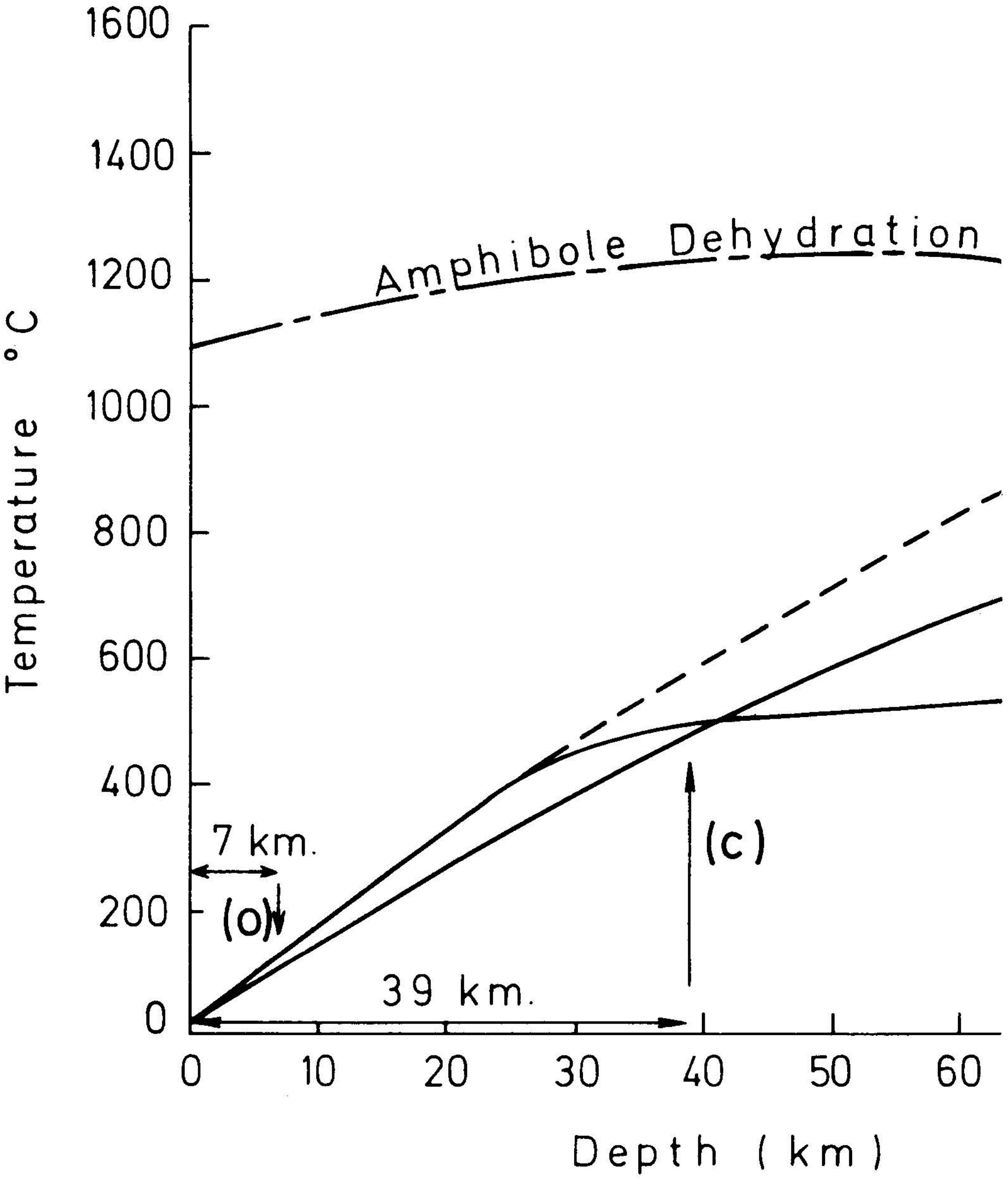
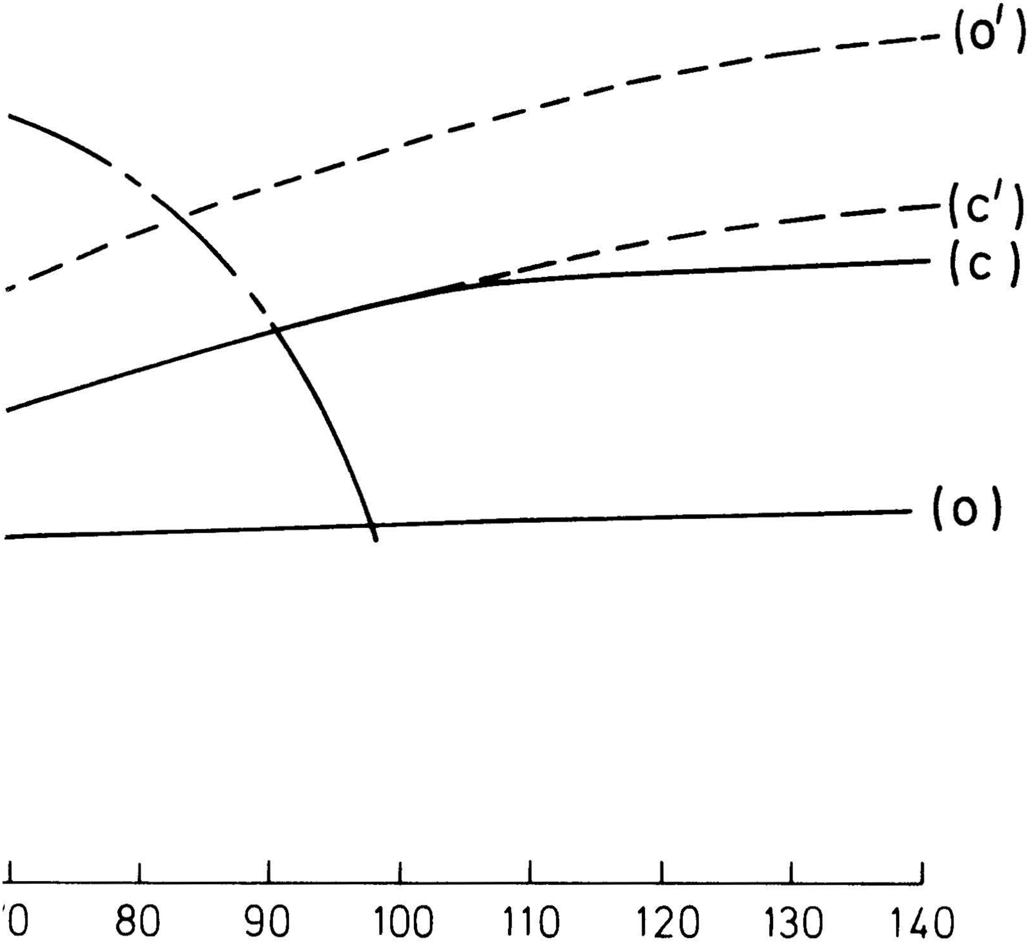


Figure 1. Temperature-Depth curves, horizontally averaged in an Earth composed of materials with different rheologies. Curves 0' and C' refer to sub-oceanic and sub-continental conditions if the creep resistance is everywhere high enough to preclude hydrodynamic instability and motion. The difference of 0 and C can be entirely atributed to different heat source density distributions and external surface heat flows in the two regions. Curves 0 and C are curves for an Earth composed of materials with more empirically plausible creep resistances. Curve 0 is based on a viscosity function $\eta = 10^2$ T exp (21000/T) and curve C $\eta =$



10^4 T exp (34000/T). These are respectively the estimates of effective viscosity in materials with a basaltic composition having $\sim 1\%$ H_20 and that of a dry dunitic rock. (Other necessary parameters have been given conventional geophysical values.) Vertical arrows mark the depths on respective curves at which the viscosity has fallen to 10^{27} poise. This can be taken to mark the base of a a lithosphere. As also explained in the text, certain modifications are necessary if these materials exist simultaneously as the sub-oceanic and sub-continental materials.

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