melting model, and Gill states that such a model requires low temperature salic melts to be sweated off from the mantle earlier than basaltic liquids. In east Africa, great effusions of phonolitic magma were among the ealiest manifestations of volcanism. In this connection, it is also worth noting that the difficulties of explaining the simultaneous availability of basic and salic magmas are just as great on a fractionation as on a partial melting model.

Yoder[2] has outlined a fractional melting mechanism to account for the contemporaneous occurrence of magmas of highly contrasted composition—in particular basalt and rhyolite, although there is no reason why similar arguments could not apply to alkaline basic and salic magmas. In a modified version of his earlier ideas, Bailey[3] has proposed that plate tectonic processes could give rise to uneven stress distribution, resulting in gentle warping of continental plates. If low melting constituents were drawn into the arches, the resulting lower density mantle would provide a density contrast for continued uplift as observed in major continental alkaline provinces.

Finally, whether or not these comments constitute a valid reply to the points raised by Dr Gill, the central thesis of my paper is unaffected: alignments of individual magmatic centres within a magmatic province can only very rarely be attributed to movement of the lithosphere over a stationary mantle hot spot.

Yours faithfully,
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1 Gill, R. C. O., *Nature*, **247**, 25 (1974).
2 Yoder, H. S., *Am. Miner.*, **58**, 153 (1973).
3 Bailey, D. K., *J. Earth Sci.*, **8**, 255 (1972).
4 Wright, J. B., *Nature*, **224**, 565 (1973).

Second Law of Thermodynamics

SIR,—In a recent article[1] Hillel proposes a statement of the Second Law of Thermodynamics which he claims is consistent with the time reversal symmetry of the microscopic laws of physics. Because the issues raised in this article nicely illustrate some of the fundamental misconceptions which continue to bedevil the subject of temporal asymmetry, I wish to make the following response.

In stating his form of the Second Law: "The entropy of a closed system tends to change monotonically with time", Hillel confuses the assumed underlying isotropy of time itself (absence of intrinsic preferred orientation) with the symmetry of physical processes with respect to time, a confusion which is in fact almost universal. The isotropy of time itself always enables us to replace the words "increasing entropy" by "decreasing entropy" in thermodynamic statements, for this merely amounts to an inversion of our conventions "later than; earlier than"—permitted provided these conventions are not referred to other physical processes (such as K^0 meson decay) which are asymmetric in time independently of thermodynamics. Hillel's remarks are thus a statement about time and not about thermodynamics.

Indeed, his is not a statement of the Second Law consistent with the symmetry of microscopic processes with respect to time, as alleged. For it is precisely the stated monotonic change of entropy in time which is the asymmetry of thermodynamics and which is apparently in conflict with the reversibility of atomic motion (Loschmidt's paradox). The fact that we are free to call the change an increase or decrease cannot alter the fact that the change is still asymmetric in time. Such a situation is well known to be incorrect, because it contradicts Poincaré's theorem, according to which all states in a closed system will be closely revisited eventually. Most laboratory systems soon reach equilibrium, after which fluctuations occur, during which the entropy both increases and decreases. These fluctuations often lead to observable effects such as Brownian motion.

A truly time symmetric statement of the Second Law, which is certainly necessary, has always been available:

If a closed system is in a random state of low entropy it is overwhelmingly likely to have been in a higher entropy state just before that moment and to be in a state of higher entropy just after that moment.

This statement resolves Loschmidt's paradox and is consistent with Poincaré's theorem. It also becomes consistent with our observations when it is appreciated that real systems are not permanently closed, but are branch systems from the main environment formed at a finite time in the past. Such systems are formed in low entropy states, and simply do not exist prior to their formation. Consequently, the monotonic entropy change referred to by Hillel is actually a consequence of the asymmetric formation of branch systems, which itself is related to the condition under which the universe emerged from the "big bang". It is actually possible to understand this condition in detail in terms of reaction thresholds of elementary particle processes on the one hand, and the peculiarities of gravitational thermodynamics on the other. I refer the reader to my forthcoming book for a full discussion of these cosmological topics[2].

Yours faithfully,
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1 Hillel, A. J., *Nature*, **242**, 456 (1973).
2 Davies, P. C. W., *The Physics of Time Asymmetry* (S.U.P., in the press).

DR HILLEL replies: On the basis of classical dynamics, one can show that a system in a random state of low entropy is overwhelmingly likely to be in a higher entropy state just before that moment and also just after. This is put forward by Davies as a statement of the Second Law which has always been available in the literature. It is, however, a theoretical statement which is still commonly thought to be at odds with the observations which we summarise in the Second Law. This is precisely the basis of Loschmidt's paradox. The theory allows the possibility of a system "prepared" at a certain time, whose entropy was decreasing monotonically at earlier times. This seems to be inconsistent with observations and is forbidden by the usual statement of the Second Law. At this stage it is argued that in practice the system cannot exist before its preparation. I do not find this argument helpful. It does not remove the asymmetry but merely restates it. Is it possible, in principle, to observe such a monotonic decrease in entropy, and if so would the observations be physically acceptable to us? I argue that the answer is yes and that our statement of the second law should be modified accordingly. The statement I propose is consistent with the reversibility of the laws of mechanics (as embodied by Davies's theoretical statement). Poincaré's theorem is not relevant when considering statistical tendencies during times much shorter than the Poincaré time.

I do not accept Davies's distinction between the isotropy of time itself (absence of intrinsic preferred orientation) and the symmetry of physical processes with respect to time. A physical law is symmetrical under time reversal if, for any allowed sequence of events, the time reversed sequence is also allowed by the law. My proposed statement was designed to satisfy this criterion, because thermodynamic observations are symmetrical in this sense, and that is why a thermodynamic time reversal produces no observable effect other than an apparent inversion of our verbal conventions "later than; earlier than".

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