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HUTCHISON, GALE, AND ARDEN REPLY— We are grateful to Wasserburg and Papanastassiou for their courteous and clear discussion[1] of model 'ages' arising from our paper[2] on some preliminary U–Pb data for the achondritic meteorite Nakhla.

We agree that a single-stage model 'age' computed from a primitive Solar System daughter/stable nuclide ratio $(D/S)_0$ defines the oldest limit for the most recent time at which the particular system became closed. This is stated in our paper on the Rb–Sr chronology of Nakhla[3]. We agree also that it is likely that model 'ages' greatly different from 4.6 Gyr constitute evidence for multiple stage open system evolution. This has already been stated by one of us[4]. We feel, however, that it is possible that the ordinarily accepted 'primitive' Solar System initial isotopic compositions might not apply to all objects in the Solar System. In our U–Pb studies we have had several indications that they are not universally applicable. Recently discovered anomalous ratios of two light elements[7] suggest that we may still have something to learn about isotopic abundances in the early Solar System.

It is still our view that model 'ages' should not be used without a clear statement of their meaning, as they are readily confused with true ages by those who are unfamiliar with the field of geochronology. Further, when only a limited number of model ages are available, the upper limit set may be so far from the true age as to be misleading. An example is provided by our own Rb–Sr work on Nakhla[3], where 15 Rb–Sr determinations on diopside, plagioclase, olivine and various magnetically separated mixtures, yield τ_{BABI} model ages ranging from 5.9 to 2.33 Gyr, whereas the internal isochron yields a true age of 1.24 Gyr.

Our data indicate that the Nakhla meteorite crystallised from a melt precisely 1.24 ± 0.01 Gyr ago[3] (not 1.37 Gyr ago[5]) with complete isotopic mixing. Because the U–Pb data demand a history of three (or more) stages (unless unusually low initial Pb isotope ratios are invoked) it is almost certain that a large scale chemical fractionation occurred at that time. This age is, of course, less than the whole meteorite model 'ages'. We reiterate that such a fractionation would have obliterated evidence of the earlier history of the parent body of Nakhla. 'Gross differentiation' of the parent body (ref. 5, abstract) could, therefore, have occurred before 3.6 Gyr, if one assumes that the later, 1.24-Gyr, event was due to volcanism or impact melting on a less than planetary scale, as is discussed in more detail in ref. 3. Wasserburg and Papanastassiou[1] correctly argue that Rb–Sr and U–Pb model 'ages' for "total" meteorite chips require a differentiation age for the total meteorite strictly younger than 3.58 or 3.68 Gyr respectively (in fact this time is 1.24 Gyr), but incorrectly assume that this argument may certainly be extended to the parent body of Nakhla as a whole.

The presence in Nakhla of water-bearing silicate has now been confirmed, but in addition, evidence of mild, post-crystallisation shock was found[6]. Presumably it was during this mild shock event that the Rb–Sr system was disturbed on a millimetre scale, as shown by the failure of meteorite chip data to lie on the mineral isochron. There is no longer a need to invoke either redistribution of Rb during or after atmospheric flight[3], or incomplete homogenisation of Sr 1.37 Gyr ago during dry metamorphism[5]. We are pleased that Wasserburg and Papanastassiou now prefer to agree with us in interpreting Nakhla as an "igneous rock with a simple history".

British Museum (Natural History), London, and
Department of Geology and Mineralogy, Oxford, UK

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North Sea phytoplankton

REID[1] described large scale changes in North Sea phytoplankton communities which have been detected since the mid-1960s by the Continuous Plankton Recorder survey. These changes were characterised by a marked decline in diatom populations, a possible increase in microflagellate abundance, and a decline in the zooplankton biomass. Reid concluded that eutrophication of North Sea waters and climatic changes could not, by themselves, easily explain these alterations in plankton communities.

I suggest that another set of factors, namely the presence of persistent industrial pollutants in the North Sea[2], may partially account for these changes. Many species of marine phytoplankton, particularly diatom species, show sensitivity to low levels of industrial waste products, including heavy metals[3], petroleum hydrocarbons[4], and stable chlorinated hydrocarbons such as polychlorinated biphenyls[5]. Alterations in the species composition of phytoplankton communities through selective toxicity of pollutants have been observed in the laboratory[5] and in freshwater ponds[6]. The predictions, based on laboratory research, suggesting that pollution-linked alterations in the species composition of phytoplankton communities would result in disturbances of zooplankton communities[5] (due to selective herbivory) are consistent with the observations described by Reid[1]. The degree to which the North Sea plankton are exposed to industrial wastes is subject to further study, though plankton in neighbouring waters are highly contaminated with organic pollutants[7,8]. (It is interesting that sublethal concentrations of pollutants can interact with various environmental factors to enhance pollutant toxicity to marine organisms, including phytoplankton.) Furthermore, North Sea animals higher in the food chain are heavily contaminated with industrial waste products[9], indicating their general presence in the resident biota.

Longhurst *et al.*[10], citing the natural variability of marine populations, commented that changes in oceanic plankton communities often cannot clearly be traced to marine pollution. Indeed, it is difficult to pinpoint any single environmental factor as the sole cause of community disruptions, because so many variables may interact to affect biological systems. I propose that the presence of persistent pollutants be considered one of the possible contributing factors influencing the plankton communities in the North Sea.

NICHOLAS S. FISHER

Woods Hole Oceanographic Institution, Woods Hole, Massachusetts 02543

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