scientific literature of instances when a change in the properties (or the loss) of a cultured cell line has made it difficult to reproduce an originally highly reproducible and quantifiable phenomenon. Such circumstances do not necessarily negate the original observations — the date cannot disappear — but make it essential to look for the same phenomenon in other biological systems. By doing this, a synergism has been confirmed between $\text{Ins}(1,4,5)\text{P}_3$ and $\text{Ins}(1,3,4,5)\text{P}_4$ in controlling intracellular Ca^{2+} in lachrymal cells^{10,14}, *Xenopus* oocytes¹⁵, *Aplysia* bag cells (L. Fink, J. A. Connor, R. F. I. and L. Kaczmarek, manuscript in preparation) and permeabilized lymphoma L-1210 cells (P. Cullen, A. P. Dawson and R. F. I., manuscript in preparation).

As to the issue of a lack of dependence on external Ca^{2+} of sea urchin egg activation, this again we have agreed with^{3,13}, and the wording in our review¹ in no way implies that the $\text{Ins}(1,4,5)\text{P}_3$ – $\text{Ins}(1,3,4,5)\text{P}_4$ synergism has to be due solely to the entry of Ca^{2+} . To the contrary, evidence from lachrymal cells¹⁴, *Aplysia* and lymphoma cells indicates that $\text{Ins}(1,4,5)\text{P}_3$ and $\text{Ins}(1,3,4,5)\text{P}_4$ can also be synergistic in mobilizing intracellular Ca^{2+} . This, as we stress in the text and in Fig. 3 of ref. 1, we believe to be a principal effect of $\text{Ins}(1,3,4,5)\text{P}_4$, and Ca^{2+} entry¹⁰ is probably modulated indirectly, perhaps by a Ca^{2+} –entry mechanism controlled by a Ca^{2+} store^{16,17}. That $\text{Ins}(1,3,4,5)\text{P}_4$ controls the access of $\text{Ins}(1,4,5)\text{P}_3$ to some intracellular stores^{3,12,13}, is still only an hypothesis, but it is one founded on firm experimental fact.

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Meltwater Younger Dryas upheld

SIR—Shackleton¹ suggests that the oscillatory relaxation of ice age conditions around the North Atlantic of Broecker *et al.*^{2,3} is but a pretty house of cards which has been shaken down by a recent study⁴ of sea level and oxygen isotope chronologies at Barbados. As an early proponent^{5,6} of a physical link between runoff patterns from North America and climatically significant ocean circulation transients, I am concerned that unwarranted assumptions about the physics of the situation may colour the discussion of the isotope data. Fairbanks assumes (and Shackleton accepts) association of unique climatic states with specific runoff patterns and also that we may apply equilibrium concepts to a process whose time-scale is comparable with that on which the deep oceans respond to significant changes of circulation and composition. I shall address the first point and briefly comment on the second.

Detailed models of the global climate incorporating external forcing conditions (geography, atmospheric composition and the solar constant, for example) show that there can be several equilibrium states associated with radically different states of the global ocean circulation. This outcome of the models arises because the oceanic salinity distribution, in the absence of the explicit feedback on the atmospheric–terrestrial hydrological cycle, is extremely sensitive to the large scale oceanic transport rates.

The oceanic processes involved are shown most clearly in a model study by Bryan⁷, who found several equilibrium states in a pole-to-pole spherical sector basin (of Atlantic-like dimensions) forced by meridionally symmetric wind, temperature and freshwater fluxes. State switching in this model can be achieved by a brief transient freshwater injection. Extending this result, Manabe and Stouffer⁸ showed that coupling global oceanic and atmospheric general circulation models, with currently conventional resolution, yields a system with climatic hysteresis (intransitivity). Depending on the initial conditions, either a state like the present climate ensues, or else one bearing a strong resemblance to the Younger Dryas situation as described by Broecker *et al.* That is characterized by a blocked thermohaline circulation in the North Atlantic; once induced, it does not require a sustained anomalous freshwater source. On this model, meridional water vapour transport to high northern latitudes is somewhat less in the cold northern climate case, which undercuts a key element of the Fairbanks–Shackleton argument, but leaves us with the problem of what terminated the Younger Dryas, rather than of

what caused it.

We may have posed the wrong question. Perhaps the Allerød–Bolling phase is the anomaly, brought about by a temporary southward displacement of the meltwater output which allowed, in turn, premature enhancement of the Atlantic thermohaline circulation and an associated high-latitude warming. If so, the end of the Younger Dryas is in fact the proper termination, possibly aided by decline in meltwater drainage from the Baltic region. Attempts to resolve such questions based on oceanic paleodata are complicated by the similarity of the time-scales of the expected composition transients in the interior ocean and of the climate fluctuations, meaning that tracer signatures may be distorted by (more or less transient) global patterns of oceanic mixing and advection.

The interpretation of Fairbanks's data should also take into account influences from the Southern Hemisphere, since the tropical Atlantic surface waters are ventilated, in our present climate at least, by the southern hemisphere source of the upper thermohaline circulation branch^{9,10} (and also influenced by drainage from the Amazon and Orinoco rivers). That implies that attempts at paleodiagnostics at high time resolution (less than 1,000 years) should be undertaken in the context of global circulation simulations.

Shackleton's final challenge to Broecker, to divine another ingenious model, seems misplaced. The immediate problem appears to be not with the model, but with a misapprehension of what it must accomplish.

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Scientific Correspondence

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