

theory of an HIV-PrP relationship. Furthermore, available sequence data allow the possibility of unequal rates to be excluded by relative rate tests<sup>7</sup>. For these reasons, the burden of proof of an HIV-PrP relationship still lies with Haseltine and colleagues.

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## Biological regulation of climate

SIR—The requirement for a biological regulation of the climate, according to Charlson *et al.*<sup>1</sup>, is first, that the source of most of the cloud-condensation nuclei (CCN) over the remote oceans is dimethylsulphide, and second, that the salt particles always found in the marine atmosphere make a negligible contribution to the CCN. They state that sea-salt particle concentrations “. . . at cloud height are typically not more than 1 cm<sup>-3</sup>. . .”. We believe that the particle concentrations are at least an order of magnitude more.

The classic sea-salt particle-size distributions obtained by Woodcock<sup>2</sup> at cloud base altitudes as a function of wind speed do indeed show concentrations of only about 1 cm<sup>-3</sup>, but his sampling method did not allow him to collect particles smaller than 2- $\mu$ m radius as a droplet of seawater. However, it was clear from the slope of his cumulative curves that smaller salt particles existed. Later, with refinements to his sampling method<sup>3</sup>, he extended his size distributions down to nearly 0.2- $\mu$ m radius and found salt particle concentrations of about 15 cm<sup>-3</sup>. His earlier wind-speed dependent cumulative curves<sup>2</sup>, if extrapolated, converge at a concentration of about 15 cm<sup>-3</sup>. This

suggests that salt-particle concentrations of around 15 cm<sup>-3</sup>, due to the long residence time of the small salt particles, will change little with changing wind speed.

There is further evidence for sea-salt particles contributing significantly to the CCN. From measurements along the shore in Hawaii, Blanchard<sup>4</sup> deduced that the salt particle flux to the atmosphere in a breaking wave is about 4,000 cm<sup>-2</sup> s<sup>-1</sup>. Recently, in laboratory experiments to model a breaking wave at sea, Cipriano *et al.*<sup>5</sup> obtained nearly the same value, 3,500 cm<sup>-2</sup> s<sup>-1</sup>. Assuming that only 0.2 of these were large enough to be activated as CCN at 0.5 per cent supersaturation in clouds, and making other reasonable assumptions for the per cent whitecap coverage at sea and particle residence time, Cipriano *et al.*<sup>5</sup> calculated that the measured flux was sufficient to produce concentrations of sea-salt CCN of 18 cm<sup>-3</sup> in the lowest kilometre of the atmosphere.

Finally, measurements made from aircraft by Dinger *et al.*<sup>6</sup> in the trade winds near Puerto Rico of the volatility of the CCN showed that above the temperature inversion all CCN are volatile, but below the inversion the fraction of CCN that is non-volatile increased as they approached the sea. Concentrations of at least 20 cm<sup>-3</sup> were found at cloud base altitudes, and they assumed this to be sea salt.

It may be true that the source of most of the CCN is dimethylsulphide, but with a steady-state background concentration of sea-salt particles of 15–20 cm<sup>-3</sup>, all of which can serve as CCN, a biological regulation of the climate is less obvious.

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CHARLSON AND WARREN REPLY — We wish to make four points. First, regulation of climate by CCN from biologically generated dimethylsulphide does not require “that the salt particles always found in the marine atmosphere make a negligible contribution to the CCN”, as stated by Blanchard and Cipriano. Our hypothesis requires only that the biologically produced CCN from dimethylsulphide alters the particle number concentration sufficiently to produce climatologically important shifts in albedo. Changes of only 30 per cent in the CCN numbers appear to be important to climate.

Second, we do not wish to enter into a debate as to whether the sea-salt CCN concentrations in clouds are 1 or 10 cm<sup>-3</sup>.

What is more important is what fraction of the measured total droplet concentration of 30–200 cm<sup>-3</sup> is nucleated on sea-salt and what fraction is nucleated on dimethylsulphide-derived sulphate. Even if sea-salt CCN concentration was 20 cm<sup>-3</sup> (which we believe is too high) it appears that sulphate CCN is still present in sufficient abundance to control the overall droplet concentration and hence the albedo.

Third, we chose to reference the excellent review of data on CCN, sea-salt and sulphate number concentrations by Pruppacher rather than to lengthen an already long paper. We certainly would have included the pioneering work of Woodcock, Blanchard and others had we had the space. We too believe the preponderance of opinion is that the fraction of droplets nucleated by sea-salt is small.

Finally, we believe that Blanchard and Cipriano would concur that more data on actual CCN composition and concentrations would help to refine the quantitative aspects of this intriguing hypothesis.

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## Contributions to a quest

SIR—My principal concern with the News and Views article entitled “The end of the quest” by Paul M. Vanhoutte (*Nature* **327**, 459–460; 1987) is his omission of reference to the pertinent research conducted in this laboratory which provides strong experimental evidence that endothelium-derived relaxing factor (EDRF) is nitric oxide (NO). Instead, only Robert F. Furchgott is referenced as having proposed that EDRF may be NO. The reference quoted was a monograph to be published in *Mechanisms of Vasodilation* (ed. Vanhoutte, P.M.; Raven, New York), which represents a talk that was delivered at a symposium held in Rochester in July 1986. What is disconcerting about Vanhoutte’s omission of our own contribution is that we also have a monograph to be published in the same book. This represents a talk that I delivered at the same symposium. Our monograph specifically addresses the close similarities in the biological properties of EDRF and NO, and contains our hypothesis that EDRF is NO or a closely related nitroso species. As Vanhoutte organized the symposium and edited both monographs, it is particularly distressing to us that fair and proper recognition was not given to our major contributions in this exciting area of research.

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