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SLOWING THE BUILDUP OF FOSSIL CO_2 IN THE ATMOSPHERE BY IRON FERTILIZATION: A COMMENT

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Can the buildup of anthropogenic CO_2 in the atmosphere be reduced significantly by fertilizing the surface southern ocean waters with iron? There are many issues that need to be addressed in answering this question. Is iron scarcity indeed the dominant process limiting biological uptake of carbon, as has been suggested by Martin [e.g., Martin et al., 1990]? What is the role of zooplankton grazing and light supply? What would be the impact on southern ocean ecology of iron fertilization and its accompanying reduction of subsurface ocean oxygen levels? How does the environmental and economic cost of a fertilization effort compare with the cost of other mitigation strategies focussing, for example, on anthropogenic emissions? Addressing these highly complex issues will require a substantial investment of research resources. It is important that we convince ourselves each step of the way that such expenditures are justified.

An important contribution to the debate on this issue has recently been made by Broecker, who has concluded from a series of model studies that even if iron fertilization worked perfectly, the reduction in CO_2 of the order of 50±25 ppm that would result after 100 years of fertilization would not be significant [Broecker, 1990; Peng and Broecker, 1991]. This is for an atmospheric CO_2 that would otherwise increase to 500 ppm over that time span, and represents a reduction of 34% in the increase that would occur starting from the present atmospheric CO_2 of 355 ppm. (Broecker [1990] gives an estimate of 40 ppm based on earlier work containing an error that was subsequently corrected by Peng and Broecker [1991]. However, the latter paper asserts that the percentage change in atmospheric CO_2 resulting from iron fertilization is approximately constant at 10±5% of the final atmospheric CO_2, regardless of what scenario is chosen for future anthropogenic emissions. The results of Joos et al. [1991a] show that this does not hold up very well. I use here the one specific example Peng and Broecker give, of a 50-ppm reduction in the case of an atmospheric CO_2 increase to 500 ppm, since it is consistent with earlier work of theirs which we were able to confirm.) A "perfect" iron fertilization is one in which biological production and export of organic matter are able to proceed all the way to total depletion of surface phosphate. Broecker goes on to say that, while research in this area should go forward, he doubts whether practical application will ever become a reality.

I agree with Broecker that practical application is unlikely. Problems and issues such as those alluded to in the first paragraph will probably stop this idea in its tracks long before we get to the application stage. On the other hand, in model calculations carried out in collaboration with F. Joos and U. Siegenthaler of the University of Bern, we obtained a much higher atmospheric CO_2 reduction of 90 ppm (60% of the increase beginning in 1990), as compared with the 50-ppm reduction obtained by Peng and Broecker for a comparable CO_2 emissions scenario. The difference of 40 ppm is mainly due to a different choice of the area where fertilization is assumed. We fertilize the entire region of the southern ocean containing high nutrients, i.e., 16% of the world ocean area [Joos et

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al., 1991a], whereas Peng and Broecker choose an area fraction of 10%. Broecker [1990] gives as an explanation for Peng and Broecker's choice of 10% of the ocean area the fact that the southern ocean will be in darkness much of the year. But if we attempt to account for wintertime darkness by stopping the fertilization over half the year, the effect of iron fertilization on atmospheric CO_2 is reduced only from 90 ppm to about 74 ppm [Joos et al., 1991b]. Thus the effect of wintertime darkness appears to be much smaller than the effect of a reduction of the area fraction from 16% to 10%.

The scenario that gives an atmospheric CO_2 of approximately 500 ppm after 100 years is a perhaps unrealistic one in which anthropogenic emissions are fixed at their 1990 levels until the year 2090. A "business as usual" scenario, which extrapolates present growth levels into the future, has atmospheric CO_2 increasing to 770 ppm by 2090. In such a case, iron fertilization would reduce atmospheric CO_2 by 107 ppm. This is larger than the 90-ppm iron fertilization uptake of the constant emission scenario, but it is now only 26% of the total increase. The effect of iron fertilization on the CO_2 increase is not a constant percentage, as Peng and Broecker assert. The most important point of these calculations, however, is that even if the iron fertilization strategy does achieve practical application, it is most effective when coupled with a reduction of anthropogenic emissions.

We cannot afford to have an unlikely quick fix iron fertilization strategy, with all its attendant problems, draw attention away from the far more effective and reliable strategy of emission control through such measures as increased fuel efficiency and biomass fuel substitution. However, I do not feel comfortable arguing that models give us clear evidence that iron fertilization would not have a significant impact on atmospheric CO_2. If the impact were 9 rather than 90 ppm, or 5 rather than 50 ppm, I would agree with Broecker's comment in this regard, but such is not the case. The information at hand convinces me that it is important for us to put the effort into a broadly conceived program to study the functioning of the carbon cycle in the high latitudes. Such a program would be of relevance not just to the iron fertilization issue, but also to the problem of understanding the low CO_2 levels of the last ice age, and to predicting possible changes that might result from indirect effects of greenhouse warming on high-latitude circulation and biology.

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