

overproduction of APP and/or reduction of lysosomal activity might accumulate the cytotoxic fragments within neurons, which eventually degenerate and die. In my view, 'extracellular' amyloid plaques could be harmless traces of dead neurons in the brain of Alzheimer's disease and related disorders.

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Cooling in the late Cenozoic

SIR — Raymo and Ruddiman in their Review Article¹ propose an explanation for the late Cenozoic climate cooling. Their approach, built from a comparison of data trends, is an inspired beginning. But it remains ultimately conceptual and primarily verbal, and thus omits an essential test of assembling a matter-conserving, dynamic numerical system. Because Raymo and Ruddiman contrast their concept (global weathering rate depends on rock supply) with opposing results drawn from a variety of quantitative models (global weathering depends on CO₂ supply), it is crucial that their hypothesis be subjected to similarly quantitative tests. Quantitative models of the carbonate-silicate geochemical cycle require an explicit system of equations for the fluxes of carbon to and from the ocean-atmosphere system and for atmospheric CO₂. I believe that if the authors were to place their hypothesis in the framework of a quantitative system model, major difficulties would appear.

Raymo and Ruddiman claim that uplift of the Tibetan plateau over the past 40 million years (Myr) provided a source of highly-weatherable silicate rock, which enhanced the rate of removal of atmospheric CO₂ to produce lower CO₂ and cooler climate. They recognize but do not satisfactorily answer the conundrum that an enhanced global weathering flux to the oceans requires an additional source of carbon dioxide to the atmosphere. Although they propose a lessening of carbon burial as one process (and potential others) that "would have partially counteracted a drawdown of atmospheric CO₂", a partial balance is nowhere near good enough. For example, were today's sinks of atmospheric CO₂ only 10% larger than the sources, CO₂ would be completely removed from the atmosphere-ocean system in a mere 5 Myr.

Perhaps Raymo and Ruddiman intend the net imbalance from the simul-

taneous, rather large perturbations of sources and sinks to be slight enough to weave reasonably-bounded CO₂ changes over 40 Myr? To my mind this would be threading an improbable needle. Although the time history of the Tibetan uplift is not available, Raymo and Ruddiman need to demonstrate that their concept in principle — using a model with explicit mass fluxes and reservoirs, and with numbers they cite, such as a 40% increase in weathering — does not lead to catastrophic drops in CO₂. I suspect that they will be forced to formulate some direct relation between atmospheric CO₂ and weathering, as used ubiquitously in the quantitative system models. Such models routinely produce outputs of temperature and isotopes for comparing to the geological record. I look forward to seeing actual numerical output produced by the Tibetan plateau hypothesis in the context of a dynamic system model.

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SIR — Raymo and Ruddiman¹ propose a false dichotomy between climate hypotheses in which variations in atmospheric CO₂ are driven either by accelerated weathering accompanying tectonic uplift or by changes in CO₂ outgassing. Furthermore, they propose that mountain uplift is the primary control on the long-term global chemical weathering rate, and they discount the role of the CO₂ dependence of terrestrial silicate weathering as a major feedback control on atmospheric CO₂ content. We believe that their paper confuses a number of important issues.

Higher atmospheric pCO₂ may lead to higher temperatures, increased runoff, enhanced soil microbial activity and greater area coverage by vegetation at high latitudes, all tending to accelerate mineral dissolution rates. Hence, silicate rock should weather more easily when the atmospheric CO₂ concentration is higher. Because silicate weathering consumes CO₂, this would produce a negative feedback that could modulate long term atmospheric CO₂ content^{2,3}. The calcium and magnesium released to rivers by the chemical weathering of silicate rocks, after entering the ocean and reacting with seafloor basalt, are predominantly buried as carbonates. Hence, on long timescales (>1 Myr), the chemical weathering of silicate rocks must roughly balance the flux of carbon available to be buried as carbonate. In the absence of variations in CO₂ outgassing and for a constant organic carbon sub-cycle, factors such as tectonic uplift, if

it allows silicate rocks to weather more easily, would tend to decrease atmospheric pCO₂ without having any long-term effect on the global chemical weathering rate. To increase the long-term rate, an additional source of carbon is required.

Raymo and Ruddiman propose that the primary source of additional carbon leading to enhanced weathering rates in the mid-to-late Cenozoic is diminished organic carbon burial, which is equivalent to net organic carbon oxidation. (Contrary to Raymo and Ruddiman's characterization, the use of organic burial in carbon cycle modelling is not "new"^{4,5}.) However, the timing of changes in the Cenozoic carbon isotope record⁶ do not closely correspond to changes in the strontium isotopic record. If terrestrial silicate-weathering rates were to have steadily increased by 40% over the past 40 Myr as proposed by Raymo and Ruddiman, then the net oxidation of this amount of organic carbon would require that O₂ levels were about twice as great as today 40 Myr ago. There is no independent evidence for this extreme situation. Enhanced burial of sedimentary pyrite is the primary possible non-atmospheric source of O₂ for net organic carbon oxidation. However, sulphur isotopic evidence points to little change and possibly some decrease in late Cenozoic pyrite burial rates^{9,10}.

The isotope evidence that Raymo and Ruddiman cite could be explained by processes they did not consider. For example, the carbon isotope signal after 15 Myr may not only reflect diminished organic carbon burial, but rather could be partly a consequence of diminished biological carbon isotopic fractionation^{11,12}. (If the late Cenozoic trend has been towards lower pCO₂, then plants from later in this period should show less selectivity towards lighter carbon isotopes than did plants from earlier.) Also, the mid-to-late Cenozoic strontium isotope signal could be the result of the liberation of radiogenic strontium from old silicates during collisional orogenesis¹³, rather than from an increase in overall weathering rates.

Organic matter burial cannot act as the feedback on atmospheric pCO₂ because factors other than atmospheric pCO₂ (for example, nutrient supply to the oceans) control the rate of organic matter burial¹⁴. The other two possible feedbacks suggested by Raymo and Ruddiman also have serious deficiencies. First, there is more evidence for authigenic calcium silicate formation in marine environments before 40 Myr ago than over the past 40 Myr (ref. 15), indicating that enhanced weathering could not have been balanced by enhanced authigenic calcium silicate formation in the late Cenozoic. Second, cal-

culations indicate that seafloor basalt weathering rates, near the pH of sea water, should be very insensitive to changes in atmospheric CO₂, precluding this process as an important CO₂ feedback mechanism¹⁶. Hence, in the Raymo and Ruddiman model, atmospheric CO₂ could vary widely because the CO₂ concentration is not fixed by a set of feedbacks.

A more inclusive model for the evolution of atmospheric CO₂ should attempt to consider quantitatively all major processes affecting the carbon cycle, including the role of plant evolution as it affects weathering¹⁷, changes in the position of continents relative to climate zones¹⁸, the importance of marine versus terrestrial burial of organic matter¹⁹, the transfer of carbonate from the shelves to the deep sea²⁰, the effect of uplift on weathering²¹ and so on. These factors are already considered in a preliminary manner in an existing model¹⁷.

Raymo and Ruddiman have drawn attention to one potentially important factor in long-term climate change. Still, there is no need to propose a dichotomy between tectonic uplift and CO₂ degassing, because no one factor can explain atmospheric CO₂ changes over all time. Significant changes could well result from a simultaneous combination of any number of forcing factors.

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RAYMO AND RUDDIMAN REPLY — The "dichotomy" we really present is between carbon cycle models assuming a positive dependence of silicate and carbonate weathering rates on atmospheric CO₂ and temperature²²⁻²⁷ and those that do not^{21,22}. In all BLAG-type models, including Berner's², chemical weathering rates decrease as global temperatures fall (and vice versa). This is the result of a dimensionless feedback function included in these models. Viewed in isolation from other factors, this temperature-weathering feedback is of course correct. But we have suggested that an entirely different factor (tectonic uplift) is a more important control of chemical weathering in the real world. Modern river studies^{23,24} support our view that chemical weathering is highly sensitive to uplift-related factors (exposure of folded, faulted, and largely

unweathered rock; uplift induced precipitation and runoff). Likewise, proxy records^{21,25-28} indicate an increase in global chemical weathering rates over the past 100 Myr, a time of global cooling.

Both Volk and Caldeira *et al.* regard our challenge to the earlier view of chemical weathering as "conceptual" in nature, and Volk states that our views should be held up to the quantitative scrutiny that more detailed ("matter-conserving") models can provide. But their criticisms miss a key point implicit in our paper. Such models can only be used for verification if their key parameterizations and assumptions are sound. Matter-conserving numerical models supporting our view have been built^{22,29}. We do not consider the fact that these models work to be "an essential test" of our hypothesis. Rather, a numerical model has been constructed which provides fewer conflicts with existing geological data (the true test).

A second key issue is the need for carbon fluxes sensitive to CO₂ levels and/or climate to provide a negative feedback that avoids a runaway 'icehouse' or 'greenhouse'. Instead of the temperature-weathering feedback, we point to the likelihood of carbon fluxes from other components of the global carbon cycle, such as the organic carbon subcycle¹, basalt weathering²², or some place as yet unforeseen, playing such a role. Contrary to the specific criticisms of Volk and Caldeira *et al.*, small variations in the fraction of carbon buried as organic matter versus carbonate (from ~25 to ~20%) can provide enough CO₂ to balance a 40% increase in the chemical weathering of silicates, carbonates and organic-rich rocks. To oxidize this much carbon would require oxygen levels within ~10% of Berner's own

estimates of Eocene O₂ levels³⁰. With the exception of a small downturn over the past 10 Myr, sulphur isotopes have been increasing for the past 100 Myr (refs 9, 10, 31) making this postulated oxygen consumption even less of a problem. To attribute the bulk carbonate δ¹³C data of Shackleton to changing photosynthetic fractionation factors would require a decrease much larger than suggested by data (>10‰ over 20 Myr, compared to 5–7‰ change observed over the past 100 Myr; refs 32, 33). And, contrary to the statement of Caldeira *et al.* we did reference previous attempts to model the organic carbon cycle (including refs 5, 7) and attributed our divergence of opinion to the use of different assumptions and data sets (see ref. 34 versus ref. 8).

The Walker and Berner carbon-cycle models revolutionized our thinking of long-term climate evolution and the global carbon cycle. However, mounting evidence suggests a world more complicated than this generation of models would imply. We are sure that our critics would agree that science advances by close scrutiny of both models and data. In this case, we believe sufficient modern (and palaeo) data exist to raise serious questions about the chemical weathering parameterization of the BLAG model (and similar models) of long-term climate change.

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- Raymo, M. E. & Ruddiman, W. F. *Nature* **359**, 117–122 (1992).
- Walker, J. C. G., Hays, P. B. & Keating, J. F. *J. Geophys. Res.* **86**, 9976–9982 (1981).
- Berner, R. A., Lasaga, A. C. & Garrels, R. M. *Am. J. Sci.* **281**, 641–683 (1983).
- Garrels, R. M. & Lerman, A. *Am. J. Sci.* **284**, 989–1007 (1984).
- Lasaga, A. C., Berner, R. A. & Garrels, R. M. in *The Carbon Cycle and Atmospheric CO₂: Natural Variation Archaean to Present* (eds Sundquist, E. T. & Broecker, W. S.) 397–410 (Am. Geophys. Un., Washington DC, 1985).
- Kump, L. R. & Garrels, R. M. *Am. J. Sci.* **286**, 337–350 (1988).
- Berner, R. A. *Am. J. Sci.* **291**, 339–376 (1993).
- Shackleton, N. K. in *Marine Petroleum Source Rocks* (eds Brooks, J. & Fleet, A. J.) 423–434 (Geol. Soc. London, 1987).
- Burdett, J. W., Arthur, M. A. & Richardson, M. *Earth planet. Sci. Lett.* **84**, 189–198 (1989).
- Kump, L. R., Hoyer, W. T., Kaplan, I. R., Sakai, H. & Zak, I. *Chem. Geol.* **28**, 199–260 (1980).
- Arthur, M. A., Dean, W. A. & Clappold, G. F. *Nature* **315**, 216–218 (1985).
- Yump, L. R. *Am. J. Sci.* **289**, 390–410 (1989).
- Palmer, M. R. & Edmond, J. M. *Geochim. cosmochim. Acta* **56**, 2099–2111 (1992).
- Broecker, W. S. & Peng, T. H. *Tracers in the Sea* (Geopub, Palisades, New York, 1982).
- Kastner, M. in *The Sea* 7 (ed. Emiliani, C.) 915–960 (Wiley, New York, 1981).
- Caldeira, K. *Am. J. Sci.* (in the press).
- Volk, T. *Geology* **17**, 107–110 (1989).
- Tardy, Y., Rogier, N. K., Probst, J. D. *Am. J. Sci.* **289**, 455–483 (1989).
- Kump, L. R. *Nature* **335**, 152–154 (1988).
- Caldeira, K. *Nature* **337**, 578–581 (1992).
- Raymo, M. E., Ruddiman, W. F. & Froelich, P. N. *Geology* **16**, 649–653 (1988).
- François, L. M. & Walker, J. C. G. *Am. J. Sci.* **292**, 81–135 (1992).
- Gibbs, R. J. *Science* **186**, 1734–1737 (1967).
- Stallard, R. F. in *Physical and Chemical Weathering in the Geosphere* (eds Lettenmaier, A. & Meadeck, M. J.) 225–246 (Wiley, Dordrecht, 1988).
- Shmetsen, A., Montrock, R. A. & Froelich, P. N. *Paleoceanography* **4**, 221–234 (1989).
- Richter, F. M., Rowley, D. B. & DePaolo, D. J. *Earth planet. Sci. Lett.* **109**, 111–123 (1992).
- Modeli, D. A., Mead, G. A. & Mueller, P. A. *Chem. Geol. (isotop. Geosci. Sec.)* **80**, 291–307 (1990).
- Kristiansen, S., Truesdell, J. R., Sarin, M. M., Ramesh, R. & Sharma, V. K. *Earth planet. Sci. Lett.* **109**, 243–253 (1992).
- François, L. M., Walker, J. C. G. & Oopyke, B. *AGU-UGS Spec. Vol.* (in the press).
- Berner, R. & Ganfield, D. *Am. J. Sci.* **289**, 333–361 (1989).
- Lasaga, A. C. *Am. J. Sci.* **289**, 411–435 (1989).
- Arthur, M. A., Dean, W. A. & Clappold, G. E. *Nature* **315**, 216–218 (1985).
- Popo, B. N., Takuoku, R., Hayes, J. M., Louden, J. W. & Baker, E. W. *Am. J. Sci.* **289**, 436–454 (1989).
- Linth, T. B. *thesis* (univ. Miami, 1983).