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Interactive comment on "Shelf erosion and submarine river canyons: implications for deep-sea oxygenation and ocean productivity during glaciation" *by* I. Tsandev et al.

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After languishing for several decades, Broecker's Shelf-Nutrient Hypothesis (1982) is making a comeback as a plausible scenario, supported by a number of physical records of ocean history and modeling efforts to explain the mechanisms. The physical records include, among others those relating to phosphorus (P), the master controlling variable for net export production in the ocean (e.g., Filippelli et al., 2007; Tamburini and Föllmi, 2009) and carbon (C), the climate modulator (e.g., Hodell et al., 2001). This modeling study by Tsandev et al. (2010) follows on several insightful biogeochemical modeling efforts by this group, and proves an excellent way to examine the major impacts of sea-

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level-induced changes in oceanic biogeochemical cycles, and to provide hypotheses to be tested based on the geologic record of biogeochemical cycling in the ocean. The timing of variations in various biogeochemical records is problematic in supporting the original hypothesis of nutrient-driven forcing of atmospheric carbon dioxide variations, but certainly the loss and gain of substantial shelf areas is an active area of modern inquiry, given how much of a filter continental margins are in global nutrient cycles.

This study focuses on marine and marginal sinks and sources of nutrients and carbon. Many open questions remain about the terrestrial side of the glacial world, as well as processes on the newly exposed glacial shelves. These are critical issues to understand how to grapple with the global impact of glaciation, including weathering rates variations on glaciated landscapes and the role of soil development on newly exposed shelf sediments and the nutrient and carbon geochemical implications of plant colonization. But as another interesting application of this biogeochemical model, this contribution is valuable and will add to ongoing discussions of glacial cycles as a strong modulator of global nutrient cycles.

The most significant puzzle that arises from these model results is why the P cycle shows so little variation, at least in terms of P burial, on glacial timescales. The model predicts substantial changes during glacials in deep ocean oxygen, as well as organic carbon burial and soluble reactive P and productivity. This latter increases by 21% in the scenario with shelf erosion and substantial bypass, supporting higher P-driven productivity and organic carbon export during glacial in line with the glacial shelf-nutrient hypothesis (Broecker, 1982). In the face of these significant changes, however, the net reactive P burial in deep sea sediments changes hardly at all (only 4%) during glacials. The authors argue that net P burial is largely dependent on redox state of the deep ocean, and thus the lower deep ocean oxygen drives enhanced redox-induced P regeneration from marine sediments. They further note that this indicates that P burial is not sensitive to changes in surface ocean productivity.

Two significant concerns arise from this result. The first is that deep marine sedi-

ments contain very little redox-sensitive P, roughly about 20% of total reactive P is in a redox-sensitive form in deep sea sediments (Delaney, 1998), and this fraction is likely comprised largely of oxides conducive to dissolution only under truly anoxia conditions given the aggressive reducing agent typically used to perform the extraction (e.g., Filippelli and Delaney, 1995). Given this small fraction of P sensitive to the slightly reduced oxygen levels modeled here, it is difficult to reconcile the reactive P burial record modeled here with the soluble reactive P and productivity record, also modeled here.

The second concern is perhaps the most intriguing. Simple mass balance calculations predict a transfer of a significant portion of the shelf P sink to the deep sea during glacial. The shelf sink currently comprises about 50% of the total oceanic reactive P sink, and loss of that sink, as discussed in this paper and in earlier ones (Broecker, 1982; Latimer and Filippelli, 2001; Filippelli et al., 2007; Tamburini and Föllmi, 2009), means, that, all other factors being equal, that a significant shift in deposition of P to the deep sea sink must occur. Further, given the residence time of P ($\sim$15 kyr), this shift to deep sea sinks must eventually reflect a shift in reactive P burial as well. That is a finding of Filippelli et al. (2007), who found that deep sea burial increases by up to 100% in high productivity areas, whether interpreted from P accumulation rate records or from geochemical proxies for "excess" or biogenic P. These high productivity areas represent at least 80% of total new production in the ocean (Filippelli et al., 2007), and thus the physical record seems to indicate that burial does correspond to production.

So, why is the glacial reactive P burial rate predicted by the model of Tsandev et al. (2010) at odds with the actual reactive P burial record? Several possibilities exist to answer this question, including (1) the model over-predicts the impact of P loss from lower oxygen deep waters and thus under-predicts actual reactive P burial, (2) the model correctly predicts reactive P burial during glacial, but the geologic record is somehow flawed by overestimation (due to improper normalization to determine excess P, exaggeration of the accumulation rate record from sediment focusing), or (3) a little of each effect. Initially, I had hoped that converting the model results into a temporal record

(as done for deep oxygen) as opposed to the simple glacial versus interglacial scenario would indicate in what way the geologic record differs from the model prediction, but given how small the model glacial effect is, this approach probably is not fruitful. I suggest that some aspect of the recycling efficiency assumed for P is not correct in the model, as the geologic record is the standard to which a model should compare, and not the other way around. But proxies records can indeed be flawed for multiple reasons, and the authors should consider more fully what the model predicts and why it is not observed in extant recordsâĂŤthis might provide hypotheses that can then be tested using the physical record.

Typo: p. 888 line 1 should be "sea level"

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