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indicates that his data are also compatible with an interpretation that metamorphism reflects heating by the domes themselves and that peak metamorphic conditions occur in the Republic trough simply because that is the location where two hot basement domes were close together (Tinkham, 1997; Marshak et al., 1997). Further, our petrographic analysis demonstrates that the time of peak metamorphism overlaps with movement in dome-border shear zones. Of note, Holm et al. state that rocks in the Republic region never reached "substantial pressures." We disagree, for sillimanite has been observed in Paleoproterozoic supracrustal rocks near Republic (see references in Tinkham, 1997).

Holm et al. state that in the western part of the Penokean dome-and-keel province the geologic history differs from that of the Republic area. Specifically, they argue that amphibolite facies metamorphism is late Penokean in age, while "shearing and cooling associated with gneiss dome formation" occurred at approximately 1.755 Ga. We concede that if these data are correct, metamorphism in the western Watersmeet area predates final dome uplift. But we point out that our model states that dome emplacement can be late to post-orogenic. The possibility remains that dome emplacement began coevally with metamorphism, but that the last movement along shear zones occurred later.

Holm et al. suggest that domes are "shallow block uplifts," but do not provide a precise definition of this phrase. If it means that the structures are comparable to Laramide-style basement-cored uplifts (uplifts resulting from contraction-related displacement on basement-penetrating thrust faults which places basement over cover and generates a forced or drape fold in the cover), we respond that features of the Republic-area domes (e.g., mylonitic shear zones along dome borders yielding supracrustal-slide-down displacement radially outward from the dome center, and intense dome-border metamorphism and synmetamorphic transposition) look nothing like those of shallow basement-cored uplifts. Of note, Holm et al.'s use of the term *uplift* in their comment implies that basement was pushed up. We prefer to avoid use of the term *uplift* because juxtaposition of basement and supracrustal rocks may represent downdropping of supracrustal rocks into graben-like troughs between the basement blocks—the process of dome-and-keel formation may resemble crustal-scale boudinage.

Holm et al. question our suggestion that early extensional detachment faulting could have occurred along the basement-cover contact in the Republic area, based on Cannon's (1973) statement that at the nose of the Republic synformal keel, the basement-cover contact is a nonconformity. Examination of Cannon (1973), however, indicates that his suggestion is an inference, for the contact is not exposed. Cannon (1997, oral commun.) confirms that there is no direct evidence requiring that the contact be an unconformity; thus, the possibility remains that the contact could be a shear zone.

Finally, Holm et al. state that the McGrath gneiss dome is "a metamorphic core complex, not an extensional dome-and-keel structure." Holm et al. do not define what they specifically mean by a "metamorphic core complex." We emphasize again that whether the term *core complex* is appropriate for extensional dome-and-keel provinces is partly a semantic argument, for both features form as a consequence of regional extensional tectonics. As highlighted by Appendix 1 in our *Geology* article, the Republic domes do not have the geometric, kinematic, or metamorphic characteristics of Cordilleran-type core complexes. The McGrath Gneiss Dome may be similar to a Cordilleran-type metamorphic core complex, but structural studies in this area are lacking, so the concept cannot be tested. If the McGrath Gneiss Dome is structurally similar to a core complex, then it simply represents an earlier stage in our tectonic model than does the Republic area. We agree with Holm et al. that the development of the gneiss domes along the length of the Penokean orogenic belt differs in detail with respect to timing of metamorphism and dome development, but we maintain that all of this development could be due largely to postorogenic extension.

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The need for mass balance and feedback in the geochemical carbon cycle: Comment and Reply

COMMENT

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Berner and Caldeira (1997) addressed long-standing but unanswered questions about the long-term controls on atmospheric CO_2, levels including the necessity for, and nature of, the feedback mechanisms between climate and atmospheric CO_2. Their main point, that the stability of global climate over the long term requires a feedback between climate and silicate weathering rates, is well put. However, they miss an important subtlety in climate control by mountain uplift, which may have fundamental implications for long-term variations in global climate, in addition to providing compelling support for the significance of the feedback relationship. Collisional orogens may produce significant CO_2 fluxes, as well as significantly increase the weatherability of the crust; these have opposite consequences for climate.

Raymo et al. (1988), following Chamberlin (1899), reemphasized the potential significance of enhanced erosion caused by the post-Eocene uplift of the Himalayas in increasing weathering rates, decreasing atmospheric CO_2, and causing the late Cenozoic global cooling. An important consequence of mountain uplift is that it changes the magnitude of the feedback function between atmospheric CO_2 levels and silicate weathering rates by increasing the weatherability of the silicate crust. Because the Himalayan-Tibetan region accounts for ~30% of global sediment production from only 5% of the land area (Richter et al., 1992; Veizer and Jansen, 1985), it would be surprising if this did not impact the global weathering rate. Confusion arose when Raymo (1991) cited the marked increase in seawater $^{87}Sr/^{86}Sr$ ratios since 40 Ma, coincident with the onset of Himalayan uplift, as prima facie evidence of the impact of Himalayan uplift on climate. If increased erosion were the only atmospheric consequence of Himalayan uplift, the rate of chemical weathering, monitored by the flux of Sr to the oceans, would stay constant on the >1 m.y. time scale in equilibrium with inputs of CO_2 to the atmosphere (Caldeira et al., 1993; Volk 1993). Global climate would cool because the weathering feedback function would change with the increasing sediment production, allowing the same rate of chemical weathering at lower levels of atmospheric CO_2 and cooler climates. An implication of the rise in seawater $^{87}Sr/^{86}Sr$ ratios is that the weathering rate of the silicate crust has increased over the past 40 m.y., which must have been supported by a corresponding increase in the rate of solid-earth CO_2 degassing. This interpretation of the seawater Sr-isotope record is complicated by the probability that some of the variation in seawater $^{87}Sr/^{86}Sr$ ratio reflects varying $^{87}Sr/^{86}Sr$ ratio of the riverine Sr rather than changes in the flux, and the Himalayas are noted for their high $^{87}Sr/^{86}Sr$ runoff (Edmond, 1992). However, even the most extreme changes in riverine $^{87}Sr/^{86}Sr$ ratios are unable to explain the range of seawater $^{87}Sr/^{86}Sr$ ratios without significant changes in the riverine Sr flux (e.g., Richter et al., 1992; Bickle, 1996). The seawater Sr-isotope curve thus presents a paradox. At a time when atmospheric CO_2 levels were decreasing and global climate cooling, solid-earth CO_2 degassing rates have increased by ~50%. That climate cooled during this increase in solid-earth CO_2 degassing attests to the sensitivity of the weathering feedback to the erosive sediment flux. This is implicit in Edmond et al.'s (1995) observation that sediment yield is a prime control on weathering rates.

Of considerable interest is the cause of the ~50% increase in solid-earth degassing over the past 40 m.y. There have been a number of suggestions for this (e.g., Beck et al., 1995; Caldeira, 1992; Raymo and Ruddiman, 1992), but many of these, based on interactions with the organic carbon cycle, could have only limited impact given the more limited variation in oceanic $\delta^{13}C$ since 40 Ma (Derry and France-Lanord, 1996). Bickle (1994, 1996) argued that one of the more plausible sources of CO_2 was metamorphic decarbonation reactions in the Himalayan and other orogens that, at the same time, were responsible for the increase in the weatherability of the crust. If this is the case, then the marked excursions in seawater Sr-isotope composition over the Phanerozoic may reflect the episodic nature of collisional orogens. Climate need not correlate with these since the orogenic activity moderates both CO_2 degassing and CO_2 drawdown. The interplay between the two factors will be hard to predict because metamorphic CO_2 degassing rates depend on the sedimentary rock types incorporated into the orogen as well as complexities of the metamorphic processes, whereas erosion and chemical weathering rates are a function of the climatic and geological setting of the orogenic belt (e.g., Beamont et al., 1992). The implication is that prediction of past climate from models of rates of orogenic activity will be difficult, if not impossible.

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REPLY

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It is possible that collisional orogens may produce both significant CO_2 fluxes (Kerrick and Caldeira, 1993, 1998) and significant increases in local weathering rates (Stallard, 1995), although both effects are far from certain. As pointed out by Bickle, these factors would have opposite consequences for climate.

Bickle writes that it would be surprising if the impressive sediment production in the Himalayan-Tibetan region did not impact the global weathering rate. The main point of our comment (Berner and Caldeira, 1997) is that on the million year time scale the global chemical weathering rate is limited by CO_2 supply; therefore, enhanced sediment production could affect the long-term weathering rate only if it affected the amount of CO_2 available to weather silicate rocks. (Some extra CO_2 could come from net oxidation of organic carbon during enhanced weathering [France-Lanord and Derry, 1997].) There is no reason to believe that the amount of extra CO_2 for enhanced erosion-accelerated weathering would necessarily be supplied in the exact amount and simultaneously by greater metamorphic degassing. On a global basis, increased Himalayan sediment production instead would be expected to affect the ease with which silicate rocks may be chemically weathered, without affecting the global weathering rate. In other words, for a constant global supply of CO_2 from degassing, an increased local rate of weathering in the Himalayan region would be compensated by lower weathering rates elsewhere, due to lowered atmospheric CO_2 and global cooling, so that the total global weathering rate would remain equal to the degassing rate. This way there would be no need for additional CO_2 from accelerated metamorphic degassing to accompany Himalayan increased erosion and weathering (Kump and Arthur, 1997).

Nevertheless, if, as suggested by Bickle, the Cenozoic marine ^{87}Sr/^{86}Sr record primarily reflects changes in chemical weathering rates, and thus changes in degassing, then CO_2 degassing rates generally must have been increasing over the past 40 m.y. (This is because the ^{87}Sr/^{86}Sr ratio has been much greater over the past few million years than earlier in the Cenozoic.) Furthermore, if the inferred ~50% increase in CO_2 degassing over this time period was dominated by metamorphic decarbonation in collisional orogens, one would need to argue that metamorphic CO_2 releases from collisional orogens have been at a Cenozoic maximum over the past few million years and that such CO_2 releases were much lower earlier in the Cenozoic. However, there is little evidence that metamorphic CO_2 releases from collisional orogenic belts have been at a Cenozoic maximum over the past few million years (Nesbitt et al., 1995; Kerrick and Caldeira, 1998).

Bickle (1996, p. 274) wrote that "an excess of CO_2 production is more probable early in the orogenic cycle when previously metamorphosed continental margin sediments are processed and because uplift and erosion tend to lag behind thermal re-equilibration (England et al., 1992). As the collision belt evolves uplift, erosion and crustal extension must equal crustal thickening and CO_2 uptake by weathering would be maximized." By this scenario, and because of the necessity of carbon mass balance as pointed out above and in our paper (Berner and Caldeira, 1997), one would expect to see more global chemical weathering during the early period to balance enhanced CO_2 production, and less global weathering later during the period with less CO_2 production. Thus, according to the model of Bickle (1996), if Cenozoic weathering had been dominated by the effects of metamorphism, uplift, and erosion associated with the India-Asia collision, then we would expect more chemical weathering globally in the Oligocene or Miocene than during the past few million years (Kerrick and Caldeira, 1998). This is exactly the opposite of what has been inferred from the Sr-isotope record.

We concur that prediction of past climatic variation is made difficult both by uncertainties in past CO_2 supply and uncertainty in the temporal evolution of the effect of atmospheric CO_2 content on the rate of silicate-rock weathering. Many factors affect CO_2 supply and the ease with which silicate-rock is weathered (Berner, 1994). The importance of collisional orogenesis relative to other factors, such as land plant evolution, variation in sea-floor generation, stochastic volcanic processes, and the relative exposure of basaltic versus granitic terranes to name a few, is yet to be determined, and these represent fruitful areas for future research.

We should be able to agree that (1) global chemical weathering of silicate rock on the million year time scale is controlled by the supply of CO_2 available to weather that rock; (2) changes in the ease with which silicate rock is weathered cannot directly affect long-term global silicate-rock weathering rates; (3) changes in the CO_2 supply and changes in the ease with which silicate rock is weathered would affect both atmospheric CO_2 content and climate.

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