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Feedbacks between climate change and landscape evolution

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Introduction

This 'debate' regarding whether or not rates of surface processes and vertical crustal motions have (Herman and Champagnac, 2016) or have not (Willenbring and Jerolmack, 2016) increased during the Late Cenozoic, in response to climate change, seems somewhat strange. It has been known for decades that such rates have increased following significant changes in climate. However, the present authors (Herman and Champagnac, 2016) keep trying to argue this without engaging with the strongest form of evidence – that from terrestrial sedimentary sequences – as was previously pointed out (Westaway and Bridgland, 2008) regarding an earlier publication (Champagnac et al., 2007). They thereby weaken their own arguments. Conversely, the Willenbring and Jerolmack (2016) argument that rates of surface processes have remained constant, essentially because a graph by Sadler and Jerolmack (2015), for some unspecified sites, appears to show no overall variation over timescales ranging between seconds and hundreds of millions of years, is contradicted by abundant evidence and should be disregarded. Nonetheless, their argument that silicate weathering has not intensified during the Late Cenozoic, despite long-standing claims by others (notably, Raymo and Ruddiman, 1992) that increasingly rapid silicate weathering has sequestered atmospheric CO_2 and thus cooled the Earth, seems timely and reasonable. The present comment will touch upon several points, relating to my own experience, regarding these issues.

Phases of vertical crustal motion

The clearest example of an increase in uplift rates at terrestrial sites, following the Mid Pleistocene Revolution (MPR), was first recognized in central Europe by Kukla (1975). This feature of the terrestrial record has since been confirmed in most regions worldwide in an extensive literature, including many investigations by me. It was indeed the subject of two international geoscience programmes, IGCPs 449 and 518, from 2000 to 2009 (e.g., Bridgland, 2016). This is not the place to list every publication on this topic by co-workers, myself, and others; some of these outputs will, however, be cited below.

The principal exception to the general pattern of phases of progressively faster vertical crustal motion arises in Archaean cratons (Westaway et al., 2003). Given that cratons lack a mobile lower-crustal layer, this observation underpins the notion that this layer has a role in inducing the vertical crustal motions elsewhere. This has in turn stimulated the development of modelling software, whose application to many observational datasets demonstrates how an increase in denudation rates, resulting from climate change and with no independent 'tectonic' cause, can increase uplift rates, via its effect on pressure at depth and thus on the lateral flow of lower-crust. As has been noted many times (e.g., by Westaway and Bridgland, 2008, in response to Champagnac et al., 2007), an important prediction of this general class of model is that under the non-steady-state conditions that develop following an increase in denudation rates, the resulting uplift rate is expected to exceed the spatially-averaged denudation rate driving it. Increases in denudation rates thus typically cause surface uplift (i.e., increase the spatially-averaged land-surface height) and thus (since some sites in any region will typically remain near sea-level as a result of fluvial/glacial downcutting) increased relief. The models, applied to such coupled systems, thus behave exactly as numerous datasets demonstrate that the Earth has behaved.

Earlier increases in uplift rates are also recognized, likewise at times when climate changed significantly, again indicating cause-and-effect connections between climate and vertical crustal motions (e.g., Bridgland and Westaway, 2008a, 2008b, 2014; Westaway et al., 2009a). One clear example follows the Mid-Pliocene climatic optimum (i.e., starts circa 3.1 Ma); another follows the climate deterioration at the 'conventional' start of the Pleistocene (~2 Ma). In the Mediterranean and Black Sea regions, an earlier phase is also recognized, associated with the Messinian Salinity Crisis and/or its 'Pontian' Black Sea counterpart. Some of the clearest examples are discussed in the aforementioned synthesis papers (e.g., Bridgland and Westaway, 2008a, 2008b, 2014; Westaway et al., 2009a), which also highlight the more detailed local literature where these effects are documented. These earlier phases are manifestly unrelated to glaciation; they evidently result instead from other climatic effects, such as dieback of vegetation (caused by falling temperatures or increased aridity) resulting in more vigorous slope processes. Herman and Champagnac (2016) discuss many localities where the cumulative effects of glaciation over many MIS climate cycles have resulted in spectacular denudation, but under-state the contribution of other aspects of climate variability to increases in uplift and denudation rates.

The mismatch between the aforementioned terrestrial datasets and the dataset used by Herman and Champagnac (2016) to quantify changing erosion rates, associated with changes in uplift rates (illustrated in their Fig. 2), is striking. Virtually all their dataset (taken from Champagnac et al., 2014) is derived from thermochronology or terrestrial cosmogenic nuclide (TCN) dating. Indeed, in their worldwide site inventory, Champagnac et al. (2014) only recognized three localities constrained by terrestrial datasets. The first was the generalized ~100±50 m of vertical crustal motion since the MPR in northwest Europe, from Gibbard and Lewin (2009). This is a particularly weak study of landscape development from fluvial evidence; much better (more detailed and more authoritative) reference sources are indeed available, including those cited above. This reference is also problematic as part of any analysis involving estimation of uplift, since it argues that you *cannot* determine uplift from fluvial incision; it indeed qualifies as an instance of 'uplift denier' literature (e.g., Westaway et al., 2015; Westaway, 2016). The second cited reference, by Bonnet et al. (2000), which discusses a staircase of Middle Pleistocene fluvial terraces in Brittany, northwestern France, is another relatively weak example; the terraces are not documented in detail, and no dating evidence is presented. There are many better references on fluvial terrace staircases in northern France, not least for the River Somme, one of the best-documented examples in the world (e.g., Antoine et al., 2007). The third example is a TCN dating study, by Haeuselmann et al. (2007), of speleothem deposits in a cave in the Alps; I return to this below. My own investigations indicate that such speleothem data are typically reliable (see below); in contrast, TCN dating of river terrace deposits is often unreliable due to post-depositional modification (e.g., by cryoturbation, terrace bluff degradation, or even modest amounts of deflation of terrace surfaces) and has yet to make any substantive contribution to dating these deposits; hence the need to concentrate on other dating techniques (e.g., Bridgland and Westaway, 2008a).

A particular difficulty with the Herman and Champagnac (2016) approach concerns time-averaging. Their dataset (in their Fig. 1(b); taken from Herman et al., 2013) is grouped over two-million-year intervals, the most recent spanning 4-2 Ma and 2-0 Ma. My own investigations (e.g., Westaway et al., 2009a; Bridgland and Westaway, 2014) indicate best estimates of the start times for the three most recent phases of vertical crustal motion of 3.1, 2.0 and 0.9 Ma, the first of these representing the end of the Mid-Pliocene climatic optimum and the last the MPR. Thus, only the 2.0 Ma phase is aligned with the Herman and Champagnac (2016) dataset. From my own experience, it is to be expected that their 4-2 Ma phase is lumping together relatively low erosion rates before ~3.1 Ma with somewhat higher rates between ~3.1 and ~2.0 Ma, and their 2-0 Ma phase is likewise lumping together fairly substantial erosion rates between ~2.0 and ~0.9 Ma with much higher rates thereafter. Evidence from Alpine caves (see below) supports this suggestion. It would therefore be most useful for these authors to rerun their 2013 analysis, aligning it to these different time-steps; it is to be expected (assuming the underlying thermochronologic data have sufficient resolving power) that a pattern similar to which has already been established from terrestrial datasets throughout most of the world should emerge. By analogy with these terrestrial records, this reanalysis should confirm the significance of increased erosion rates following the MPR, an aspect that is almost completely downplayed by Herman et al. (2013) and Herman and Champagnac (2016). I also note in passing that the 'News & Views' piece on the Herman et al. (2013) paper, by Egholm (2013), starts by saying that these authors show that 'global cooling in the past 6 million years has accelerated the destruction of mountains'. They never claimed that and, indeed, showed nothing of the kind: rather than 'destroying mountains', through the combination of feedbacks summarized above, the increase in denudation rates that they documented has added significantly to global relief and thus increased the height and relief of mountain ranges.

The role of 'tectonics'

Both Herman and Champagnac (2016) and Willenbring and Jerolmack (2016) repeatedly refer to 'tectonics' or 'tectonic uplift'. The latter paper indeed begins with a statement '… the pace of erosion and chemical weathering is determined by tectonic uplift rates'. On the contrary, I would suggest that uplift rates are typically determined by climate through a variety of feedbacks including those summarized above. I further recommend that usage of 'tectonic' in this context be restricted to only apply to vertical crustal motions caused by plate motions; for the more general case of motions caused by surface processes and mediated by lower-crustal flow, the term 'atectonic' (after Kaufman and Royden, 1994) is preferable. Since the vertical crustal motions occurring in most regions worldwide manifestly have nothing to do with plate motions (e.g., Westaway et al., 2009a), the argument by Willenbring and Jerolmack (2016) that constancy of plate motions implies constancy of denudation rates or uplift rates is irrelevant. Herman and Champagnac (2016) state that 'some of the high [exhumation/uplift] rates are observed in tectonically active places' implying (but never actually stating) that they are aware of instances of atectonic uplift. However, they then add 'Nevertheless, the important observation is that the best documented changes [in exhumation/uplift rates] are in relatively quiescent places like the Patagonian Andes, western Alps, Coast Mountains of British Columbia and Fiordland, New Zealand …'. These examples are all, of course, within convergent plate boundary zones, where one might expect the vertical crustal motions to result mainly from plate motions. On the contrary, in other plate-boundary-zone localities, it is now evident that the observed vertical crustal motions are being caused mainly by surface processes, not by the plate motions themselves; this can be inferred because the vertical crustal motions show features indicative of a climatic cause, such as increased rates following the MPR (e.g., Westaway, 1993; Westaway and Bridgland, 2007; Demir et al., 2009; Westaway et al., 2009b). The examples discussed by Herman and Champagnac (2016) are likely to follow the same pattern; Westaway and Bridgland (2008) indeed already pointed this out with regard to the discussion of the western Alps by Champagnac et al. (2007). I would also dispute the notion that the Patagonian Andes, western Alps, Coast Mountains of British Columbia or Fiordland, New Zealand, provide 'the best documented changes [in exhumation/uplift rates]'. Currently, the best-documented datasets demonstrating climate-driven changes in uplift rates during the Quaternary or wider Late Cenozoic are those provided by the best-documented fluvial sequences in Europe, such as for the River Thames in southeast England (e.g., Bridgland, 1994; Bridgland and Schreve, 2009), the Somme in northern France (e.g., Antoine et al., 2007), and the Dniester in the Ukraine/Moldova border region (e.g., Matoshko et al., 2004). The latter sequence extends back to the Late Miocene and provides evidence of increases in uplift rates circa the Pontian salinity crisis of the Black Sea, following the Mid-Pliocene climatic optimum, around the 'conventional' start of the Pleistocene, and following the MPR. Work documenting the comparably superb long-timescale records of vertical crustal motion provided by the fluvial datasets from the catchments of the rivers Euphrates and Tigris in Syria and southeastern Turkey, which supports the patterns evident elsewhere (e.g., Abou Romieh et al., 2009; Westaway et al., 2009b; Demir et al., 2012), is now on hold due to the tragic political situation in these regions.

Thermochronology

Much of the discussion by both Herman and Champagnac (2016) and Willenbring and Jerolmack (2016) concerns thermochronologic datasets; the former study states that such data are key to identifying Late Cenozoic increases in denudation rates whereas the latter study argues that this is an effect of selection bias in relation to histories of intermittent denudation, given that 'the only rates that are recordable during the last few millions of years are those with higher values'. Willenbring and Jerolmack (2016) attribute this deduction to an unpublished manuscript, whose contents are I know not what, and to Naylor et al. (2015). However, Naylor et al. (2015) were addressing the uncertainties in thermochronologic analysis of detrital samples, which is inherently more difficult than for in-situ surface or subsurface samples. Most analyses on in-situ samples indeed determine a common denudation history that can account for measurements throughout the vertical extent of the dataset. Terrestrial Cenozoic case studies illustrating this technique include Green (2002) and Green et al. (2012).

As regards the application of thermochronology to Quaternary denudation, the essential point made by Westaway and Bridgland (2008) (and re-stated by Herman and Champagnac, 2016) remains valid: thermochronology can only constrain Quaternary denudation rates (and, thus, can only bear upon discussion of increases in denudation rates) in regions of relatively rapid denudation. The core evidence for increases in uplift rates, associated with increases in denudation rates, comes from terrestrial sedimentary sequences, primarily staircases of river terraces, as already noted. In regions of rapid denudation, such evidence will be lost to erosion. In the absence of other evidence, one must resort to thermochronology (cf. Herman and Champagnac, 2016); however, the two approaches lead to essentially the same conclusions.

Cave evidence

A key form of sedimentary evidence, which can reveal denudation histories of mountain areas (e.g., the Alps), independent of thermochronology, concerns speleothem deposits in caves (e.g., Meyer et al., 2011). This was not mentioned by Willenbring and Jerolmack (2016) and only given the briefest mention by Herman and Champagnac (2016). A typical cave level in an uplifting region will initially form below the water table under phreatic conditions. As denudation and uplift proceed, fluvial/glacial entrenchment will lower the water table, creating vadose environments where speleothems can accumulate. Continuing, denudation, uplift and entrenchment can subsequently raise the cave within the landscape, where it will ultimately be destroyed by the continuing denudation. Alpine cave evidence includes the following:

-Haeuselmann et al. (2008) applied TCN dating of speleothems to the Siebenhengste-Hohgant cave system flanking the Aare valley, in the central Alps near Interlaken in Switzerland. They inferred ~500 m of entrenchment, time-averaged at 0.12 mm a^{-1}, between ~5.0 Ma and the MPR, followed by ~900 m of entrenchment post-MPR, time-averaged at 1.2 mm a^{-1}. More detailed inspection of their results (their Fig. 3) indicates ~250 m of entrenchment, time-averaged at ~0.08 mm a^{-1}, during ~5.0-2.0 Ma, with the next ~250 m of entrenchment, time-averaged at ~0.21 mm a^{-1}, between ~2.0 Ma and the MPR.

-Bosák et al. (2002) and Haeuselmann et al. (2015) have documented the Snežna Jama cave system, in the Raduha Massif near the southeastern extremity of the Alps, in northern Slovenia just south of the Austrian border. This cave is at ~1530 m altitude, ~950 m above the adjacent River Savinja. It contains speleothem assigned to the timespan between the Thera normal-polarity subchron (~5.0 Ma) and the Kaena reverse-polarity subchron (~3.0 Ma), indicating relative landscape stability during this span of time. Elsewhere in the cave, siliciclastic deposits span the early Gauss chron (~3.5 Ma) to the Olduvai normal-polarity subchron (~1.9 Ma) and yielded a TCN burial date of ~3.6±1.2 Ma (±1σ). These deposits demonstrate faster denudation in the vicinity during ~3-2 Ma, since when fluvial/glacial entrenchment isolated the cave from sediment inputs. If the ratio of pre-MPR and post-MPR rates of valley entrenchment/uplift is the same as for Siebenhengste-Hohgant, the typical post-MPR rate has been ~1.0 mm a^{-1}.

-Meyer et al. (2009, 2011) documented speleothem from the Wilder Mann Cave in the northern Alps, near the Germany-Austria border. Flowstone from this cave, at ~2400 m, yielded a uranium-lead date of 2.02+0.04/-0.07 Ma, suggesting deposition during MIS 77 or 75. The local water table is controlled by valley floors at ~800 m; assuming that was already so during the speleothem deposition, ~1600 m of entrenchment is indicated, time-averaged at ~0.8 mm a^{-1}. If the actual rate of uplift/entrenchment increased at the MPR in the proportion indicated at the other sites, ~1250 m of it might post-date the MPR, time-averaged at ~1.6 mm a^{-1}.

The above evidence clearly indicates very clearly, independently of any vagaries of thermochronology, that uplift rates in the Alps have increased during the Quaternary. This evidence is also entirely consistent with the widespread observations of phases of accelerated uplift in surrounding regions (e.g., Bridgland and Westaway, 2008b, 2014; Westaway et al., 2009a), especially regarding the effect of the MPR. One remaining uncertainty concerns integrating the records of uplift of and denudation within the Alps with the corresponding records in their immediate surroundings, bridging between the Alpine records (mainly from thermochronology and cave evidence) and those from the adjoining lower-relief uplands, mainly river terrace records. Given recent experience in Britain (e.g., Westaway, 2009, 2016; Bridgland et al., 2014; Westaway et al., 2015), such integration should be feasible in future.

Constancy of sediment fluxes

As noted above, the notion of constancy of sediment flux, on timescales of less than a year to ~100 million years, strikes me as a particularly problematic aspect of the Willenbring and Jerolmack (2016) analysis. The underlying reference source, Sadler and Jerolmack (2015), begins by citing a pioneering work on fluvial process studies, Wolman and Miller (1960), and proceeds to argue that the sediment flux transported by a given river is independent of the timespan over which one measures, or averages, the observations. However, this deduction contradicts the principal conclusion of Wolman and Miller (1960), that the bulk of the sediment flux in most river systems is associated with flows on timescales of the order of a year, with a range between catchments between circa six months and circa five years. Something has evidently gone fundamentally wrong with the Sadler and Jerolmack (2015) analysis, but since they do not indicate what data informed their conclusions, one has no way of establishing precisely where their error has occurred.

Wolman and Miller (1960) anyway quantified sediment transport regimes under present-day climates. If one considers longer timescales, including the Pleistocene, much greater variability is apparent; for example, at certain times in each MIS cycle rivers transport much larger sediment fluxes, in many cases by orders-of-magnitude, than at present. Such conditions, when (as already noted) reduced vegetation cover enhances slope processes and, thus, much greater fluxes of sediment enter rivers, determine times of emplacement of fluvial terrace deposits; at other times during a climate cycle river systems might have relatively low energy, like in many temperate regions at present (e.g., Bridgland, 2000; Bridgland and Westaway, 2008a). Conversely, sometimes extremes of discharge transport large fluxes of sediment throughout river systems to the continental shelf or ocean basin. An example, recently studied in detail, concerns the Channel River, which flowed along the now-submerged English Channel during late Middle Pleistocene and Late Pleistocene sea-level low-stands, transporting very large volumes of sediment to the Western Approaches continental shelf and Bay of Biscay during a series of large-magnitude flow pulses, each probably of short (? less than a century) duration (e.g., Busschers et al., 2005; Toucanne et al., 2008, 2009; Westaway and Bridgland, 2010; Mellett et al., 2013). It seems impossible to reconcile the views of Sadler and Jerolmack (2015) and Willenbring and Jerolmack (2016) regarding constancy of sediment flux, with such evidence. In contrast, Herman and Champagnac (2016) recognise, correctly in my view, that sediment fluxes are typically greatest during climate transitions. However, as with much else, they envisage this essentially in terms of transitions between glaciated and unglaciated states, rather than appreciating the extent of climate-induced variations in sediment flux in the more widespread regions that have not been glaciated but have nonetheless experienced the consequences of climate cyclicity. They would do well to engage with the extensive modern literature on this topic, an appropriate starting point being Bridgland (2000).

I note, in addition, that the logarithmic vertical scale used for the Willenbring and Jerolmack (2016) graph has the effect of masking substantial variations in sediment flux, which (if one takes antilogarithms) are in fact depicted as ranging by a factor of ~10, from values of ~0.3 to ~3 in the units stated, across a range of timescales relevant to the Pleistocene. From my own experience (e.g., Westaway and Bridgland, 2010) I would suggest that the variation is rather higher, probably by two orders-of-magnitude at least, rather than a single order-of-magnitude. However, this greater effect might possibly arise because the Westaway and Bridgland (2010) study relates to Britain, a region of extreme variations in Pleistocene climate (having alternated between temperate and arctic regimes) that might be expected to have a particularly strong effect on sediment fluxes. It would thus be useful to explore the magnitude of sediment-flux variations in a series of case studies for different regions worldwide. It is, therefore, particularly disappointing that neither Sadler and Jerolmack (2015) nor Willenbring and Jerolmack (2016) have said where their data came from, so readers are none the wiser; as it stands this work is thus of no help as a starting point for such a project.

Constancy of atmospheric CO_2

Willenbring and Jerolmack (2016) state that 'recent proxies suggest stabilization of atmospheric CO_2 at near-modern levels since the late Oligocene, despite continued global cooling (Beerling and Royer, 2011 …).' However, Beerling and Royer (2011) in fact indicate that there has been no overall trend in atmospheric CO_2 since the Late Oligocene, they never assert that values have remained constant. They also note that different proxy measurements often differ by a factor-of-two and that variations in atmospheric CO_2 are evident, with apparent peaks during the Mid-Miocene and Mid-Pliocene climatic optima (~18-15 Ma and ~3 Ma, respectively). Furthermore, they summarize the work of Seki et al. (2010); apart from other variations, these authors inferred that during the Mid-Pliocene climatic optimum the atmospheric CO_2 concentration was ~330-400 ppm, this upper bound resembling the present-day value, but it decreased to ~280 ppm (i.e., similar to pre-industrial values) in the latest Pliocene, by ~2.8 Ma. Finally, Beerling and Royer (2011) note that Antarctic ice cores provide direct (not proxy) measurements of atmospheric CO_2 back to ~0.8 Ma (e.g., Lüthi et al., 2008), including decreases to ~180 ppm during glacial maxima, and recommend that proxy methods for CO_2 be calibrated against this observational record, a record that Willenbring and Jerolmack (2016) do not even mention.

Willenbring and Jerolmack (2016) also argue using proxy measurements for CO_2 and other data that there has been no significant change in the global rate of silicate weathering since ~10 Ma, and conclude that their work '…undermines the hypothesis that increased weathering due to mountain building or climate change was the primary agent for a decrease in global temperatures.' However, I am not aware that anyone has ever suggested such a variation, since ~10 Ma, anyway. If this conclusion is instead taken as applying throughout the Cenozoic, then it bears upon the long-standing claims by others (notably, Raymo and Ruddiman, 1992) that denudation associated with the India-Eurasia collision caused higher rates of silicate weathering, which sequestered atmospheric CO_2 and thus cooled the Earth. This 'silicate sequestration' hypothesis is indeed now highly engrained; for example, it has been presented as established fact in popular science broadcasting (e.g., Stewart, 2010). However, it has never been supported by direct evidence, nor has it ever been integrated with direct evidence of rates of silicate weathering, such as the work of Washbourne et al. (2012, 2015), or with mainstream research on feedbacks pertaining to Late Cenozoic climate change, for which better working hypotheses involving carbon dioxide are now available (see below). It is therefore timely that Willenbring and Jerolmack (2016) have challenged this engrained argument, notwithstanding the not-exactly-crystal-clear manner in which they did this.

In support of their argument, Raymo and Ruddiman (1992) listed data indicating high dissolved loads in rivers that drain the Himalaya. However, they did not provide any data on the chemistry of this dissolved material to substantiate their claim that it is derived largely from chemical weathering of silicates. Such data are nonetheless available, for example, from Sarin and Krishnaswami (1984). This latter study reports that the Ganges' dissolved load is indeed high, typically ~2000-3000 micromoles per litre. However, most of it is hydrogen carbonate, as might be expected from dissolution of limestone, only a small proportion being dissolved silica. For example, at Hardwar, where the Ganges leaves the Himalaya, the total dissolved load is 1750 micromoles per litre of which 894 are hydrogen carbonate and only 109 are dissolved silica. Conversely, at Kanpur, ~500 km farther downstream, the dissolved load is much greater, 3410 micromoles per litre, of which 1829 are hydrogen carbonate and 157 are dissolved silica. Furthermore, in the River Chambal, a right-bank Ganges tributary, which drains part of the ancient shield region of peninsular India, the dissolved load is much higher, 4999 micromoles per litre, of which 2531 are hydrogen carbonate and 296 are dissolved silica. These observations, especially the low dissolved silica concentration where the Ganges leaves the Himalaya, fundamentally undermine the notion that chemical weathering of silicate rocks is significant in the Himalayan Mountains and has had any significant role in the post-Eocene cooling of the Earth.

Many researchers have argued, on the contrary, that the principal cause of the Oligocene global cooling was the formation of the Antarctic Circumpolar Current as a result of the opening of the Drake Passage (between South America and West Antarctica) and the Tasmanian Gateway (between Australia and East Antarctica (e.g., Livermore et al., 2005; Scher and Martin, 2006; Scher et al., 2015), and the resulting thermal isolation of Antarctica. Changing current patterns in the Southern Ocean at this time, resulting in upwelling and turnover, might have facilitated atmospheric CO_2 sequestration via growth of phytoplankton (e.g., Livermore et al., 2005). A similar effect, growth of phytoplankton in the Southern Ocean caused by influxes of iron-rich dust from South America, likewise sequestering atmospheric CO_2, accounts for maybe a third of the reduction in atmospheric CO_2 between Pleistocene interglacials and glacial maxima (e.g., Martínez-García et al., 2014).

Conclusions

Herman and Champagnac (2016) have reached some valid conclusions regarding the role of feedbacks between climate, denudation rates, and vertical crustal motions during the Late Cenozoic. However, they have over-stated the role of glaciation in this succession of processes and have not engaged with the modern literature that considers such feedbacks in unglaciated regions that have nonetheless experienced significate climate fluctuations. By not engaging with the best evidence demonstrating the feedbacks that they envisage, they weaken their own arguments. Conversely, Willenbring and Jerolmack (2016) argue for constancy of sediment flux, which is contrary to abundant evidence. They also argue for a constant atmospheric CO_2 concentration during the Late Cenozoic, which seems to be based on mis-citation of the relevant literature. They are nonetheless correct, in my view, to note that the idea that silicate weathering has resulted in atmospheric CO_2

sequestration is mistaken, notwithstanding the highly engrained status of this hypothesis (cf. Raymo and Ruddiman, 1992).

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