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Trimline trauma: the wider implications of a paradigm shift in recognizing and interpreting glacial limits.

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Abstract

Trimlines mark the boundary between glacially eroded landscapes on low ground and landscapes dominated by evidence of periglacial weathering on higher summits. For many years the trimlines of Scandinavia, Britain and Ireland have been interpreted as marking the surface of the ice sheets at the maximum of the last glaciation, but recent cosmogenic exposure dating of erratics far above the trimlines in NW Scotland show this to be false. The trimlines in that area must represent an englacial thermal boundary between warm (eroding) ice and cold (protecting) ice. It is now clear that even very experienced geomorphologists cannot necessarily tell the difference between terrain that has been recently glaciated and terrain that has not, because cold based ice can leave virtually no trace. This calls into question not only the interpretation of high level trimines elsewhere, but also the mapping of the lateral limits of past glaciations, which are often based on similar or even weaker geomorphological and sedimentological evidence.

Keywords: Glaciation, Devensian, Weichselian, Weathering, Blockfields,

Introduction

In mountainous terrain, ‘periglacial trimlines’ mark the altitudinal boundary at which landscapes dominated by clear evidence of glaciation are replaced, often abruptly, by those sculpted under prolonged periglacial conditions. Clear evidence of glaciation includes erosional landforms such as roches moutonnées, whalebacks and ice scoured bedrock, smaller scale erosional features such as striae, chattermarks and gouges and depositional evidence in the form of glacigenic sediments, constructional landforms, and smaller-scale features including perched boulders and scattered erratics. Although locating the precise altitude for a trimline on an individual mountain can be difficult, the distinction between sites above and below the trimline is often very clear. Below the trimline mountains are commonly dominated by smoothed bedrock surfaces, largely scoured clear of debris but with large boulders ‘perched’ in positions where they cannot have arrived under gravity. Mountain tops above the trimline, in contrast, are often dominated by thick and extensive ‘block fields’ comprising angular local bedrock riven along the joints (Hopkinson and Ballantyne 2014). Where the joints are not closely spaced, isolated tors with open joints and edges smoothed and rounded by granular disintegration are common (Ballantyne 1994b, McCarroll and Ballantyne 2005).

I began working on trimlines in the mountains of southern Norway in collaboration with Atle Nesje and Svein Olaf Dahl from Bergen (McCarroll and Nesje 1993; Nesje et al. 1994), but we soon joined forces with Colin Ballantyne, who had begun to do similar work in Scotland (Ballantyne 1997, 1998, 1999a, 1999b). For more than a decade we spent our summers in the mountains of Norway, Britain and Ireland mapping periglacial trimlines and measuring the degree of rock weathering. Our measures of weathering included rock surface hardness, measured with the Schmidt hammer (McCarroll 1989, 1991), joint depth (McCarroll et al. 1995) measured with a graduated probe (knitting needle with scratches on it), and rock surface roughness measured using a variety of instruments and indices (McCarroll 1992; McCarroll and Nesje 1996). We also dug holes to reach the base of the soil and took samples for X-ray diffraction analysis to check for the presence of the aluminium oxide gibbsite, regarded as an indicator of very prolonged weathering (Ballantyne 1994a; McCarroll and Ballantyne 2000).

In numerous publications we demonstrated a clear and measurable difference in degree of weathering of rocks and soil above and below the trimlines that we had mapped, supporting our contention that the landscapes above the trimline had been exposed to weathering for much more than a single interglacial, and that they therefore had not been glacially eroded during the last glaciation (Ballantyne et al. 1997, 1998a,b). When cosmogenic dating of rock surfaces became available, Colin secured the collaboration of one of the pioneers of the method, John Stone (Stone et al. 1998, Stone and Ballantyne 2006, Ballantyne and Stone 2015), who joined us on several field campaigns, ensuring that the sampling methods that we used were appropriate. The cosmogenic 'exposure ages', from sites in Norway (Nesje et al. 2007), Scotland (Ballantyne et al. 2013), Ireland (Ballantyne et al. 2006, 2007, 2008, 2011) and Wales (McCarroll et al. 2010) confirmed our contention that the surfaces above the trimlines had been exposed for much longer than the Holocene and that they must therefore have survived glacial erosion during the last glacial cycle.

When we published our results from the various regions we always discussed the different ways that trimlines can be interpreted. Perhaps the clearest explanation was in Ballantyne et al. 1998 where Colin produced Figure One to illustrate four competing hypotheses for weathering limit formation. Although we always considered and discussed the various options, we generally concluded that the most likely explanation was that the trimlines marked the upper surface of the ice at its maximum extent. Mountain summits above the trimline were thus interpreted as nunataks and by mapping the trimlines over large areas we argued that we were reconstructing the surface altitude of the last ice sheet. In NW Scotland, for example, we reconstructed an ice surface that descended north-westwards from about 900m close to the present watershed down to about 600m along the coast and down as far as 450m in NW Skye.

In retrospect I can see that we were working to a paradigm that had become comfortably familiar. The trimlines formed spatial patterns that seemed consistent with an ice surface, declining towards the coast for example, and there was always clear evidence of a substantial difference in exposure age, measured using either relative (weathering) or 'absolute' (cosmogenic isotope) methods. We were happy with our interpretation and so were the reviewers of our papers. We often discussed the other possible interpretations but always came to the same conclusion. We were convinced that we were mapping the surface of the last ice sheet.

We now know that we were wrong. Various small pieces of evidence started to chip away at our confidence in the 'nunatak paradigm'. The growing evidence for ice limits far offshore (Evans et al. 2005, Stoker and Bradwell 2005, Bradwell et al. 2008) did not sit comfortably with our thin ice model, though most of the evidence could be reconciled using reasonable values for shear stress (Bradwell et al. 2007). The ice limits and thick accumulations of sediment west of the Outer Hebrides, however, were problematic given our reconstruction of a thin independent ice sheet in that region (Ballanytne and McCarroll 1995, Ballantyne and McCarroll 1997, Ballantyne and Hallam 2001). At the same time I was aware of the work that was being done in Sweden, clearly showing that large tracts of the country that must have been covered by ice during the last glaciation nevertheless retained clear evidence of pre-glacial conditions (Kleman and Stroeven 1997; Fabel et al. 2002), the only reasonable explanation being that they were protected by cold based ice. The deciding piece of evidence, however, came when we worked in SW Ireland (Ballanyne et al. 2011). The trimlines there are as clear as in other areas we had worked, but when we tried to reconstruct the surface of the last ice sheet in that region it made no sense. It was simply too low to allow ice to reach the south coast without invoking implausibly low yield stresses. Sedimentological and dating evidence, however, clearly showed that it did reach at least that far (Ó Cofaigh et al. 2010). We had to conclude that in that part of Ireland the trimline could not reasonably be interpreted as the surface of the last ice sheet, and our paradigm was clearly in trouble.

By this time I had moved on to other things, and was busy running an EU project, but Colin grasped the nettle and devised a sampling strategy for cosmogenic isotope exposure dating that would critically test our hypothesis that the trimlines in NW Scotland represent the surface of the last ice sheet. Rather than dating quartz veins in glacially eroded bedrock surfaces, which was our usual target (Ballantyne 2010), he searched out the rare erratic boulders that occur above the trimline. We had always assumed that these were ancient, emplaced in some earlier glacial cycle. The work was done in collaboration with Derek Fabel who had used a similar logic in Sweden (Fabel et al. 2002). The results were clear and I think incontrovertible: the erratics were deposited during the last glaciation (Fabel et al. 2012).

I must confess that being proven wrong is not a new experience for me. My interpretation of the very thick glacial sediments of western Llyn, in North Wales, as meltout till, for example, fitted a paradigm that was popular at the time but has since been falsified (McCarroll and Harris 1992). I also produced several papers arguing that carbon isotopes in Arctic tree rings could be used to reconstruct summer temperature (McCarroll et al. 2010), which my younger colleagues demonstrated was not true (Young et al. 2010; Gagen et al. 2011). However, as a scientist there is no shame in being wrong because falsification of hypotheses is how we make progress (McCarroll 2015). If you are afraid of being wrong you will never have the confidence to publish anything of interest. We should, however, try to learn from our mistakes.

Why were the trimlines misinterpreted?

It has been said that the best geologist is the one who has seen most rocks. If the same logic applies to glacial and periglacial geomorphologists then I would argue that our mapping team had strong credentials. Atle and Svein Olaf have spent their lives living and working in the mountains of Norway and Colin Ballantyne is probably the most gifted geomorphologist of his generation. I have always regarded the ability to stand in a landscape and imagine the processes and environmental changes that created it as one of the greatest gifts of a geographer's education. Skilled or not, we were clearly wrong.

Having reflected at some length, I suspect that the reason we were wrong was that we made the simple assumption that, in terms of landscape change, glaciation is a very traumatic event.

I now realise that is not necessarily true. Over most of the landscape of northwest Scotland and the Hebrides, for example, the evidence of glacial erosion is spectacular. The view from any of the isolated peaks of NW Scotland, for example, is dominated by vast tracts of ice moulded 'knoch and lochan' terrain. On the Torridonian sandstones of the mainland or the Lewisian gneiss of the Outer Hebrides you can lie down in some of the glacial grooves carved out at the base of the ice, and the striae, chatter-marks and gouges appear straight out of a textbook. When I mapped in that area I considered it inconceivable that the ice that was clearly having such a dramatic effect on the mountain flanks could leave no erosional traces at all on the summits. Clearly that was wrong. The same ice sheet did indeed cover those summits, but there is no evidence of erosion and there are no deposits that could reasonably be interpreted as glacigenic. The only evidence that ice has ever overtopped the high summits of NW Scotland is a very sparse scattering of erratic boulders.

Underlying our 'nunatak paradigm' was the mistaken assumption that a good geomorphologist can tell the difference between a landscape that has been recently glaciated and one that has not been glaciated for a very long time. I now think that this assumption is wrong, and that has important and far-reaching implications.

Interpreting periglacial trimlines

It is clear that the periglacial trimlines that we and other teams have mapped cannot necessarily be interpreted as representing the upper surface of the ice during the last glaciation and that in some areas, at least, they represent the transition from warm-based eroding ice to cold-based ice that does not erode but rather protects the pre-existing landscape. Terrain above the trimlines thus remains dominated by the effects of periglacial conditions.

The evidence from NW Scotland is clear; the cosmogenic exposure ages on the erratic boulders demonstrate that the trimline represents a thermal boundary, not the ice surface. The same must be true of the trimlines we mapped on the Inner (Dahl et al. 1996; Ballantyne and McCarroll 1997; Ballantyne 1999c) and Outer Hebrides (Ballantyne and McCarroll 1995; Ballantyne and Hallam 2001). The trimlines throughout Ireland are also most likely to represent a thermal boundary rather than the ice surface and very thick ice over Ireland helps to explain the lateral extent of the last ice sheet, particularly the proposed extension of the Irish Sea glacier as far south as the Isles of Scilly (Hiemstra et al. 2006). Given the location of ice limits on the shelf edge, and the very large amount of sediment that was delivered to them, I suspect that our reconstructions of the Norwegian ice sheet at the last glacial maximum (Nesje et al. 1994, 2007) are also wrong.

Elsewhere, however, I think that we need to be careful that we do not simply replace one untested assumption with another. We originally assumed that all of the trimlines represented the ice surface; we should not now automatically assume that they all represent thermal boundaries when we do not actually have clear evidence to test that hypothesis. I do not know the mountains of the English Lake District very well, and was not involved in mapping that area (Lamb and Ballantyne 1998), so I will not comment on that region. Snowdonia, however, is very close to my heart and I still have problems reconciling the new 'cold-based ice' paradigm with the evidence in that region (McCarroll and Ballantyne 2000; McCarroll 2005). I accept that the most likely scenario is now that proposed by Jansson and Glasser (2005) who suggest that the Welsh ice was centred to the south of Snowdonia and spread northward over all of the summits, cutting the north/south valleys of Nant Ffrancon and the Llanberis Pass and feeding ice towards the lowlands of Arfon and Anglesey, where it would have met the ice moving down the Irish Sea. However, whereas in NW Scotland the erratic boulders are the key to demonstrating that the cold-based ice is the most likely explanation, the distribution of erratics in North Wales actually argues against such an interpretation.

The Arenig Mountains, south of Snowdonia, comprise a very distinctive white microgranite that is easy to identify and boulders of this lithology are conspicuous in the landscape. If ice from that region had crossed Snowdonia then we might expect to find Arenig microgranite erratics crossing those mountains and extending into the Arvon lowlands, north of Snowdonia, and then Anglesey. However, it was demonstrated almost a century ago by Edward Greenly (1919) that the erratics do not follow that pattern. They are absent from Snowdonia and from Arvon but occur in a band that crosses southern Anglesey. The simplest interpretation, in my view, is that Snowdonia was an independent ice centre, with ice radiating in all directions, and that ice from the south was forced to pass to the east, flowing down the Conwy valley to meet the Irish Sea ice and be pushed to the SW, parallel with the Menai Straights, leaving the erratic train in southern Anglesey.

If this interpretation is true, and Snowdonia maintained an independent ice sheet rather than being over-ridden by ice from the south, then the very clear trimline in that region (McCarroll and Ballantyne 2005) can still be interpreted in at least two ways: it could represent a thermal boundary or it could be the surface of the ice sheet. Without a supply of erratic boulders to date I am not sure how these competing hypotheses can be critically tested. Measuring both ^{26}Al and ^{10}Be on exposed bedrock surfaces might provide an answer because a period of prolonged ice cover would produce a difference in apparent age, but care would have to be taken to exclude the influence of prolonged winter snow cover, which might have a very similar effect. I am still of the opinion that the spectacular cantilevered slabs on the blockfield summit of Glyder Fawr, and the great tors of the Carneddau may well have stood proud of the ice during the last glacial maximum. I am happy to be proven wrong again, but I would like to see some hard evidence.

Lateral ice limits at the last glacial maximum

Perhaps the most important lesson to learn from the paradigm shift in interpretation of high level trimlines is the implications for recognizing and interpreting the lateral limits of glaciers and ice sheets. It is now clear to me that even a very experienced geomorphologist cannot necessarily recognize whether a landscape has been recently glaciated or not, because cold-based ice can leave virtually no trace of its presence. That is why we misinterpreted the origin of the trimlines. However, in most cases the lateral limits of glaciers and ice sheets are also based on geomorphological evidence. The main difference is that whereas the trimlines represent an abrupt and rather obvious change in the landscape, the lateral limits are often very subtle indeed. This has led me to question the extent to which we can trust the established ice margins for the last and previous glaciations.

I know of only one place in the British Isles where it is possible to stand in the landscape and see clearly where the limit of glaciation lies, and that is the Isles of Scilly. The ice limit is clear because the islands are formed entirely of granite and have only been glaciated once, so that south of the ice limit erratics are completely absent. The Island of St Marys, for example, has no erratics and on the southern coast of St Martin’s, less than 3km to the north they are also completely absent. Walking the few hundred metres northward over the moorland of St Martin's, however, reveals a very gradual transition marked only by the appearance of a few scattered pebbles of mixed lithology. The same transition occurs on the adjacent island of Tresco. On the northern shores of both islands erratic pebbles are abundant and on St Martin's, at Bread and Cheese Cove, there is even a small remnant of glacitectonized sediment (Hiemstra et al. 2006). The available dating evidence suggests that the ice limit on Scilly was produced during the last glacial cycle (Scourse 1991, McCarroll et al 2010, Chiverrell et al. 2013). The important point here is that the position of the ice margin on Scilly, irrespective of age, is revealed only by a light dusting of erratics; there is no clear geomorphological evidence. If Scilly had been glaciated more than once, and erratic pebbles were widespread, I doubt that the present limits would even be visible.

Another location that casts doubt on our ability to locate ice limits on the basis of geomorphological or erosional evidence is the Isle of Lewis, off NW Scotland. I had the great pleasure of assisting Siwan Davies and Paul Albert to sample organic sediments at Tolsta Head on the NW coast of the Island in 2014. The organic sediments, attributed to OI Stage 3 (Whittington and Hall 2002), are underlain and overlain by glacial sediments, and it is clear from the shape of the exposure that they are not a raft that has been transported. If the organic sediments were not there, it would be very difficult to tell that there were two glacial deposits. The most recent glaciation clearly did not cause much erosion, since even the soft organic sediments have not been removed. On the NW coast of Lewis, at Toa Galson, the evidence for glaciation that leaves virtually no mark on the landscape is even clearer, because here there are thick accumulations of un-cemented beach gravel, that must have accumulated before the last glacial maximum, but which have been overridden by ice that extended well offshore. I could see no evidence of erosion and if it were not for the clear offshore evidence for the extent of the ice I, as a geomorphologist, would have assumed that northwest Lewis was not covered by ice during the last (Devensian, OI Stage 2) glaciation.

The ice limits that I know best are those in South Wales, and I now have very serious doubts about their veracity. In north Pembrokeshire, for example the proposed ice limit runs along the northern flanks of the Preseli Hills, leaving southern Pembrokeshire ice free. The evidence used to define that limit is exactly the same as the evidence we used to define the ice surface at trimlines. It is marked by a transition from bedrock that has been glacially scoured, and where glacigenic sediments are widespread, to a landscape dominated by blockfields and tors where glacigenic sediments are absent (Walker and McCarroll 2001). However, there are plenty of erratic boulders well to the south of the proposed ice limit, just as there are erratic boulders above the trimlines in Scotland. My best guess at the moment is that the north Pembrokeshire ice limit probably is the southern limit of the last ice sheet in that area and that the headwaters of the Cleddau remained ice free. That explains why that is the only river system in wales that remains graded to well below present sea level, producing the deep water port of Milford Haven. However, on a recent visit to the Castlemartin Penninsula, the limestone area south of Milford Haven, I noticed that the Carboniferous limestone is littered with erratic pebbles. There are also old records of large erratic boulders perched on the limestone, though most (perhaps all) have since been moved. Of course the erratics may have been deposited during an earlier glaciation, but I am not aware of any clear evidence to that effect. The concept of the 'ice free enclave' of South Pembrokeshire should really be critically tested.

The ice limits that are closest to home for me, and that I know best of all, are those on Gower Peninsula, to the west of Swansea. Generations of university students have visited the area to examine the Ipswichian raised beaches, spectacular cave deposits and of course the ice limits. The established wisdom is that there are two ice limits on Gower: one at Rotherslade in the east, representing the Devensian ice limit (disputed by Hiemstra et al. 2009) and one further west, marked by the 'Paviland Moraine', that represents some earlier glaciation (Bowen et al. 2002). It has always seemed unlikely to me that a narrow peninsula like Gower would, just by chance, retain clear evidence of the limits of two glaciations. I now suspect that both ice limits on Gower are wrong and that the whole of the peninsula was covered by ice during the last glaciation (McCarroll and McCarroll 2015).

Conclusion

I have no regrets for the many summers I spent mapping trimlines. Time spent in good company is never wasted, and the company was excellent. I do not even regret that we misinterpreted the evidence for so long, because I think that we used the best methods that were available to us and we made our interpretations within the established paradigms of the day. Progress in science often involves the falsification of bold and testable hypotheses, and our hypothesis was bold and testable and wrong. The trimlines still exist, of course; they are a real part of the landscape. No doubt some of the limits that we mapped are wrong (e.g. Glasser et al. 2012), not least because we performed the mapping in the field, in mixed weather, without the aid of modern tools like Nextmap or Google Earth. The most likely interpretation of the trimlines now seems to be the transition from warm based eroding ice to cold-based ice that protects the landscape. I hope that the maps will still be of value to ice sheet modellers, though I doubt the interpretation will be simple because I see no reason to assume that the thermal boundary represents any fixed point in time, such as the glacial maximum.

In my view the most important lesson of the 'trimline trauma' is that even very experienced geomorphologists cannot necessarily recognize the difference between a landscape that has been recently glaciated and one that has not. Cold-based ice can leave virtually no trace. This gives cause to question not only the veracity of maps of the upper limit of the last ice sheet, but also of the lateral limits which are often based on very similar or even weaker geomorphological and sedimentological evidence.

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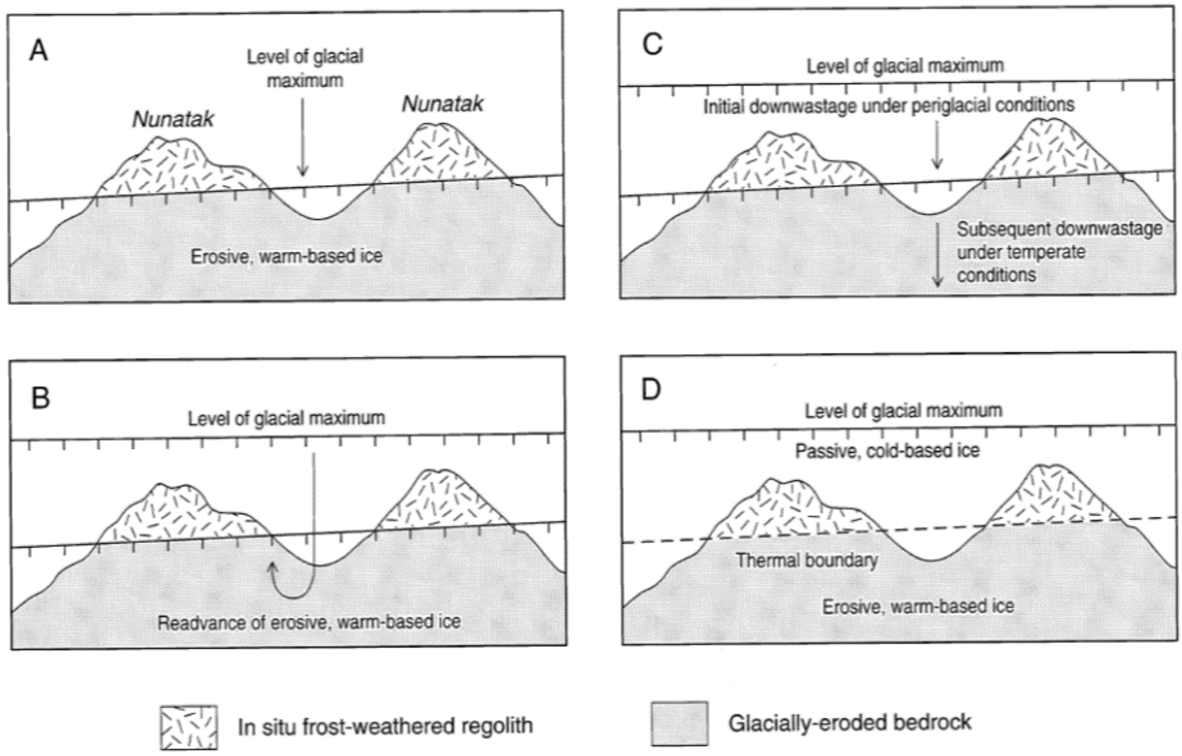

Figure 1. Four hypotheses of weathering limit formation. (A) A periglacial trimline representing the upper surface of an ice sheet at its maximum thickness. (B) A periglacial trimline cut by a glacial readvance during overall ice-sheet downwastage. (C) A weathering limit representing formation of frost debris during an initial period of ice-sheet downwastage under periglacial conditions. (D) A weathering limit representing a thermal boundary between cold based (passive) and warm-based (erosive) ice within a former ice sheet. Reproduced with permission from Ballantyne CK, McCarroll D, Nesje A, et al. (1998) The last ice sheet in north-west Scotland: Reconstruction and implications. *Quaternary Science Reviews* 17: 1149-1184.