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COMMENT ON "AN IGNEOUS ERRATIC AT LIMESLADE, GOWER, AND THE GLACIATION OF THE BRISTOL CHANNEL" BY BRIAN JOHN

Nick Pearce, Richard Bevins, Rob Ixer and James Scourse

In his article in *Quaternary Newsletter* 162 (pp. 4-14), *An Igneous erratic at Limeslade, Gower, and the glaciation of the Bristol Channel*, John (2024) reports the observation of a large unspotted dolerite erratic boulder on the foreshore at Limeslade, SE Gower. He provides petrographical observations and data, some attributed to others, to explore whether this boulder can be provenanced to igneous sources in the Mynydd Preseli region of northern Pembrokeshire. He concludes, "tentatively", that on the basis of this evidence the boulder cannot have derived from a Mynydd Preseli source and explores other possible igneous sources across Wales. Having been unable to establish any clear provenance for the boulder, he then discusses possible transport mechanisms, concluding that the boulder is most likely an erratic transported by the Irish Sea Glacier [*sic*; = Irish Sea Ice Stream; Scourse *et al*., 2021).

Given the ambiguity over the provenance of the boulder, he then, rather extraordinarily, states "*The discovery of this erratic in the SE corner of the Gower Peninsula has a profound bearing on the debate concerning the entrainment and transport of Preseli "bluestones" from West Wales towards Stonehenge*." (p. 10). Indeed, the motivation behind the article is clear in that John's exploration of transport mechanisms starts first by discounting human transport rather than the more parsimonious explanation of glaciation. This provides the starting point for a lengthy rehearsal of the Stonehenge bluestone transport debate involving a polemic against the advocates of human transport (e.g. Parker Pearson *et al*., 2021) and support for glacial transport to Salisbury Plain: "…*once the glacial transport hypothesis is taken seriously, Stonehenge loses much of its aura and marketing potential!*" (p.11).

We respond here to the following issues raised in the article: 1. hand specimen descriptions, thin section descriptions and provenance, 2. geochemistry and petrogenesis and 3. glaciation and glacial transport in the Bristol Channel region.

Hand specimen descriptions, thin section descriptions and provenance

John makes reference to potential provenances for the Limeslade boulder, but the commentary provided leads to more questions than answers. He is certainly correct in stating that the boulder cannot have been derived from anywhere in the local vicinity, given the geology of the Gower

Peninsula. John reports that Dr Katie Preece, from Swansea University, had examined a detached flake from the bottom surface of the boulder, noting that it is "*made of greenish unspotted dolerite or micro-gabbro with crystals of feldspar and pyroxene"* (p.5). John reports that *"The rock's colour is very similar to that of fresh unspotted dolerite surfaces in the western part of Mynydd Preseli in Pembrokeshire."* (p.5). Whilst that is true, it similarly applies to almost all dolerite/gabbros exposed across the whole of the north Pembrokeshire area. He then goes on to say that the rock is similar to *"another rock sample from Foel Eryr on Mynydd Preseli"* (p.5) and then that *"When the surface texture is compared visually with that of unspotted dolerites in Cilgwyn, near Newport, there is remarkable similarity"* (p.5), here, rather misleadingly suggesting two specific locations while comparisons which could be made with almost any dolerite in north Pembrokeshire. Matching of dolerites/microgabbros at the outcrop scale across north Pembrokeshire is just not possible on visual similarities alone and to suggest so is misleading.

John then goes on to quote Professor Peter Kokelaar as saying that *"the banding perpendicular to the 'columnar' length is fairly typical of some coarsely jointed sills I have mapped between Fishguard and St David's Head"* (p.5), which is a fair observation. However, John then states that *"One such sill occurs at Ogof Golchfa, near Porthclais (SM 740236), and the similarities in hand specimen are again striking"* (p.5-6). However, not only does Ogof Golchfa not lie between Fishguard and St David's Head, it is not a dolerite but rather a porphyritic microtonalite (British Geological Survey, 1992, Sheet 209, 1:50 000). Having identified at least three outcrops that show 'similarities' John then admits - despite stating earlier that *"The boulder surface is remarkably fresh..."* (p.5) - that *"However, visual comparisons (especially for weathered surfaces) can cause confusion.."*. This is indeed true.

A further section provides information on the boulder in the form of a summary of the petrography of thin sections from the boulder. Why this is separate from the earlier descriptions is unclear, but is confusing. The information is basic, reporting that it contains clinopyroxene and plagioclase in ophitic intergrowth, lacking any detail of alteration of the rock, other than that the pyroxene is incipiently uralitized, the plagioclase is partially argillized, and a vague comment – quoting Dr Steve Parry - about *"overprinting by opaque material (presumably oxides of Fe and/or Ti"* (p.7). There is no mention of the presence of particular secondary minerals, which in altered basic igneous rocks in north Pembrokeshire comprise variable amounts of chlorite, titanite, epidote, clinozoisite, prehnite, pumpellyite and actinolite. The distribution of the last three phases is linked to subtle variations in whole rock Mg/(Mg+Fe) contents in the sub-greenschist to greenschist facies boundary region, as present in the Mynydd Preseli area and reported by Bevins and Robinson (1993), and these offer significant potential for provenancing.

John states *"Dr Parry suggests that there is nothing about the petrology or geochemistry which is exceptional or particularly noteworthy. Micro-gabbro intrusions of this general type are not unusual in the Lower Palaeozoic successions of Wales...but at present it would be unwise to suggest a specific age or provenance."* (p.8). This is fine, given the cursory examination of the boulder by John and colleagues but then John, on the basis of no evidence, states that *"it can be tentatively suggested that the Limeslade boulder has not come from a Mynydd Preseli outcrop"* (p.8). However, he then admits that *"there are scores of outcrops in NW Pembrokeshire (including the St Davids Peninsula, Pen Caer and the Eastern Cleddau catchment) where narrow strips of microgabbro or coarse dolerite with a greenish tinge are exposed at the surface"* (p.8-9), again mentioning erroneously the microtonalite outcrop at Ogof Golchfa.

There is reference to alteration of Preseli dolerite; *"Preseli unspotted dolerite is also found in hand axes, and the Implement Petrology Group has classified it as "group XIIIb"* (p.8), followed by "*The group comprises altered sub-ophitic dolerite, originally with clinopyroxene-plagioclase-titanomagnetite-ilmenite-apatite intergrowths. Alteration is widespread and secondary minerals include muscovite, chlorite, epidote, clinozoisite, actinolite, quartz, pyrite, titanite, pumpellyite and prehnite (Ixer and Bevins, 2018)."* (p.8). However, Ixer and Bevins (2018) were describing Group XIII spotted dolerite, known as 'preselite'. The non-spotted and spotted dolerites are petrographically significantly different.

To add to the confusion, John then goes on to state that *"more distant sources of dolerite, for example in the Cader Idris region, in Llyn, and in Snowdonia have in the past been considered as possible sources for the Stonehenge unspotted dolerite assemblage but dismissed on the grounds that they are physically not well matched"* (p.9) quoting Kokelaar *et al*. (1984), Williams-Thorpe *et al*., (2006) and Bevins *et al*. (2014). Quite why these potential areas of Wales are brought into the equation is puzzling, as none of them would likely be a potential source for the Limeslade boulder. What is meant by *"physically not well matched"* (p.9) is not clear, whilst the choice of reference citations here is odd. Kokelaar *et al*. (1984) make no reference to the origin of the Stonehenge bluestones, whilst Bevins *et al*. (2014) discuss the source of the spotted and non-spotted dolerite bluestones, focussing on specific outcrops in the Mynydd Preseli. Reference to Wiliams-Thorpe *et al*. (2006) is also confusing and no reason for its citation is offered.

Recent petrographical descriptions of igneous rocks in southwest Wales have commonly employed total petrography using polished thin sections, especially if petrography is to be allied to geochemistry, as has been advocated by Ixer *et al*. (2004). Total petrography allows for the identification of opaque and semi-opaque minerals, in addition to making minor phases more apparent and accessible to technical (e.g. SEM, microprobe) detailed characterisation. The descriptions in John are based on transmitted light microscopy only, and hence miss the opportunities provided by total petrography.

As reported by John, the petrography of the Limeslade boulder thin sections offers no evidence for a provenance for the boulder.

Geochemistry and petrogenesis

John attempts to compare the composition of the Limeslade erratic with XRF and pXRF analytical data from other sources. This analysis is deeply problematic.

First, no details whatsoever are given of the analytical methodology used by Darvill and Parry. It is not known what Parry's affiliation is, but presumably they have access to a pXRF. Different models of pXRF instrument operate differently, and it is essential that operating conditions and some reference material data are presented so that analytical quality can be assessed. In lacking any traceability data, the results presented by John have no meaning and cannot be compared externally. Using an 8 mm diameter analytical spot, Pearce *et al*. (2022) show that between 15-20 pXRF analyses are required to give a meaningful analysis of Preseli dolerites. If the pXRF used by Darvill and Parry has a smaller (e.g. 1 mm or 3 mm diameter) analytical spot as used on some other brands of instrument, many tens of analyses will need to be performed across a medium to coarse grained doleritic rock to obtain a representative analysis (this being related to grain size vs. spot size considerations). Bevins *et al*. (2022) presented a wide range of reference material analyses for the pXRF used in their studies, with analysis of an in-house standard during each analytical run to check day-to-day calibration. Here, John presents no reference material data for the Darvill and Parry analyses.

John describes "*The geochemical compositions of **<u>three</u>*** [our emphasis] *"control samples" of spotted dolerite from Carn Meini*" (Table 1, p.6) and then uses the average of these to compare the data with other published analyses. We do not know if this is three single analyses or whether it is the average of many analyses from three samples. If it is the former, this is a totally inadequate number of analyses for such comparisons; the latter is only valid if each analysis is an average of many repeat determinations (several tens) from each sample (see above). The comparison can then only be made if reference material data are presented to show the accuracy of the data presented by John. John goes on to state that because the tabulated data are *"ppm measurements...means that **direct comparisons are not possible** with...past geochemistry...which presented oxide percentage weights for the major elements and ppm measurements for the trace elements."* (p. 6). Conversion between wt % oxide and ppm is a simple calculation, described in many basic geochemistry texts and online, routinely undertaken by undergraduates, thus comparison is entirely possible.

John compares Ni concentrations for three Carn Meini analyses from Darvill and Parry (average 99 ppm, no standard deviation given) and compares these with data from Pearce *et al*. (2022) (average 42.5 ± 11.6 ppm). John quotes no standard deviation for the Pearce data, nor the fact that only 88 of the 165 analyses performed by Pearce *et al*. (2022) were above the Limit of Detection - LoD - thus the true concentration will be notably lower than the reported average of 42.5 ppm were all the analyses <LoD to be known/included (this is discussed in detail in Pearce *et al*., 2022). John states that "*Not one of the 165 readings was over 90 ppm, raising the possibility of calibration or instrumental error*" (p.6-7), presumably meaning errors in the Pearce data, subsequently adding "*environmental, procedural and human errors"* (p.7) were also likely factors causing the difference. John, notably, fails to compare the bulk sample XRF analyses from Carn Meini of Thorpe *et al*. (1991) (29 ppm, included in John's Table 1, p.6) and Bevins *et al*. (2014) (35 ± 7.2 ppm, excluded from Table 1), both of which are entirely consistent with the pXRF Ni data from Pearce *et al*. (2022), and far lower than the Darvill and Parry reported concentration. For Rb, Darvill and Parry determined Rb at 5.7 ppm, Thorpe *et al*. (1991) recorded 19 ppm, Bevins *et al*. (2014) 14 ± 5.6 ppm and Pearce *et al*. (2022) 9.7 ± 3.2 ppm. Once again, the Darvill and Parry data looks out of place (here, too low) when compared with bulk data. The Bevins *et al*. XRF and the Pearce *et al*. pXRF data overlap at ± 1 standard deviation, and whilst the pXRF data look slightly low compared to the bulk-sample laboratory XRF, this could be related to surface leaching of Rb (Potts *et al*., 2006), which may have had a more extreme impact on the Darvill and Parry data. John fails to make these comparisons. What John's argument in fact shows here - ignoring the uncertainties of the analytical methods used - is that the Darvill and Parry pXRF data are too high for Ni and possibly too low for Rb. Preconceptions drive the preferred narrative here - that the Darvill and Parry data are correct, and the Pearce *et al*. data are *de facto* wrong - despite clear and contradictory information which have been ignored.

John displays his ignorance of geological and geochemical processes related to sill formation. The various crags of Carn Meini and Carn Gyfrwy are all mapped as part of one sill (see Burt *et al*. (2012) and the map in Bevins *et al*., 2014). Sills inflate by the injection of magma between layers of rock (typically sediments), extending laterally (for long distances, often kilometres) and vertically (typically tens/several tens of metres) as more magma is injected. They may show vertical compositional variation related to flow injection processes, and as they cool from the outer surfaces, further vertical compositional variations may form related to differential cooling rates, the order of crystallisation and crystal settling (Bhattacharji, 1967; Merriman *et al*., 1986; Marsh, 1989; Gibson and Jones, 1991; Gibb and Henderson, 1992; Latypov and Chistyakova, 2009; Marsh, 2013; Holness *et al*., 2017). Thus, to capture the range of compositional variation displayed in a sill, vertical profiles are required and this approach was followed by Pearce *et al*. (2022) in their pXRF assays of the Preseli sills. John, contrary to this extensive body of literature, contends that the variations between the Darvill and Parry, and Pearce *et al*. data, are because

- *"these discrepancies (and the wide scatter of plotted points on bivariate graphs) is that Carn Meini is not a single tor but an association of tors"* (p. 7). *T*he source of the geochemical data providing the scatter to which John is referring to here is not clear, but if it is their data it merely relates to the fact there are so few analyses.
- "*Samples have been taken by researchers from many different locations in this assemblage of tors, revealing great heterogeneity within the parent igneous mass"* (p. 7) although it is not stated who performed these analyses. If it is Bevins *et al*. (2104) and Thorpe *et al*. (1991), then their data are consistent with the pXRF data of Pearce *et al*. (2022), and their analyses do not show the wide variation John claims.
- John claims these other sources of data "*contradicts the claim made by Pearce et al (2022) that there is homogeneity in the Carn Meini intrusion, based on the fact that there are near constant concentrations of Ni, Zr and Ba"* (p. 7) but we have shown above the similarity in the analytical data (e.g. Ni, above), so John's claimed contradiction is false.
- John also claims incorrectly "*That* [the claimed homogeneity] *takes no account of substantial lateral variations across the intrusion which are visible to the naked eye". (p. 7).* The analyses of Carn Meini by Thorpe, Bevins and Pearce (described above) show comparable concentrations from diverse sample locations, so what this variation is that John claims is not clear. It is clear, however, that some analytes do not compare with the pXRF data from Darvill and Parry. How geochemical variation can be visible to the naked eye is a mystery.

John does however admit *“There is inadequate data for the creation of scatter diagrams or bivariate graphs involving the Limeslade boulder ppm readings”* (p. 7): he is correct in stating the Limeslade data are inadequate: were it plotted alongside the Carn Meini data from Thorpe *et al*. (1991), Bevins *et al*. (2014) and Pearce *et al*. (2022) with the Darvill and Parry Carn Meini “*control sample*” data, the shortcomings would be self-evident.

Glaciation and glacial transport in the Bristol Channel region

That this boulder was transported by glacial ice, at some point during the Pleistocene, is highly likely, as suggested by John. Whether this was by ice rafting at a time of high relative sea level, or by grounded ice – either by the Irish Sea Ice Stream (Scourse *et al*., 2021) or reworked into a local Welsh ice cap, is impossible to determine given it is not stratified. The Limeslade erratic is not a singular occurrence. It is but one of many hundreds of erratic boulders found in the modern intertidal zone across southern Britain and Ireland that, as pointed out by John, have been difficult to explain by ice rafting because of the necessity of generating high relative sea-level at a time of low eustatic sea level during glacial stages. Scourse (2024) recently provided a mechanism for their emplacement by ice rafting arising from the asynchrony between Early and Middle Devensian regional ice sheet development and global sea level. John argues that this is “*not supported by the field evidence*” (p.10) on the basis that in Cardigan Bay only one glacial episode – the Last Glacial Maximum during the Late Devensian at 26 ka BP - is present following the Ipswichian interglacial raised beach. However, as pointed out by Scourse (2024), 1. recent geochronological evidence from the raised beaches of the region is challenging the notion that these are all of interglacial status and 2. that the bounding periglacial sequences can be of very different age depending on the extent of postglacial coastal erosion (see Figure 3 in Scourse, 2024).

John questions whether high level erratics can be explained by ice-rafting. Some may be, as is probably the case of the uplifted sequences along the Sussex coastal plain (Scourse, 2024). It is true that “*Large erratics on the coasts of Devon and Cornwall are not restricted to the intertidal zone*” (p.10) but the vast majority are. Some may well be related to grounded ice, or glacilacustrine conditions caused by impounding by glacial ice, as in the case of North Devon. As to why “*it is a mystery why glacial transport should be questioned, given that glacial deposits are on the Isles of Scilly, on Lundy Island…and south of Barnstaple*” (p.10), there is actually no mystery. The evidence for glaciation from these three localities is unequivocal, whereas further afield in SW England, apart from the evidence for cold-based plateau icefields on Dartmoor (Evans *et al*., 2012), the evidence is equivocal.

John's conclusion that "*On balance, the Limeslade is most likely a glacially transported erratic, carried southeastwards by the ice of the Irish Sea Glacier (sic) during one of the Quaternary glacial episodes*" (p. 10) is as likely as it is ice-rafted. The occurrence of boulders from Pembrokeshire transported by glacier southeastwards across South Wales towards the Severn Estuary has been known for well over a century (reviewed in Scourse, 1997). There is, however, no geochronological or other evidence to support John's contention that this ice advance occurred during the Anglian glaciation, nor is there any evidence to extrapolate this transport route eastwards from the western Mendips towards Stonehenge, as shown in Fig. 7 (p.9).

The Limeslade erratic is in no way exceptional. It is simply another giant erratic on the foreshore of southern Britain. There is no evidence presented by John to shed light on its provenance; rather, the narrative represents a curious journey of local sources to a broad, Wales-wide journey of potential sources of the Stonehenge bluestones, which has no relevance to the identification of the boulder on the foreshore, at Limeslade on Gower, and which logically, on the basis of previously published works, was derived from north Pembrokeshire. This article merely represents a disingenuous cover to justify a rehearsal of the now well-worn and increasingly tedious debate concerning transport of the Stonehenge bluestones.

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