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Reply to Comment on "Supra-subduction zone ophiolites retain hydrogen generation potential after 300 Myr" by Donzé and Truche (2026)

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Reply to Comment

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We thank Vidavskiy *et al.* (2026) for their close reading of our Letter and for the opportunity to clarify several points. We share their view that the origin of natural hydrogen in ophiolitic settings is far from settled, and we presented our results and conclusions to contribute to the discussion rather than to present an unambiguous explanation. In the same spirit, we welcome a debate organised around evidence and testable predictions.

Following the editors' guidance, we focus our reply on the points that bear directly on the data and reasoning of our original Letter. Some of the recommendations in the comment, including an expanded literature review and the adoption of an alternative model in place of the one we proposed, fall outside the scope of our original Letter, and we address these only where they intersect our arguments. We group our response thematically rather than following the numbering of the comment, because several of its points share a common root.

Timing of Hydrogen and Methane. The most substantive point in the comment concerns our statement that the young apparent radiocarbon age of methane at Bulqizë implies that the associated hydrogen must also have been recently produced. We accept that, phrased as a strict co-genesis, this conflates a temporal correlation with mechanistic simultaneity, and we are glad to sharpen it.

The ^{14}C datum presented in our original Letter constrains the bulk carbon residence time of the methane, but not directly that of the hydrogen. A low ^{14}C age requires the incorporation of young carbon. This can occur in many ways including addition of modern carbon during sampling and preparation; mixing between radiocarbon dead, abiotic/thermogenic/mantle sourced methane and modern microbial methane; or the recent abiotic methanation of young inorganic carbon. Discriminating among these different possibilities requires paired measurements of the $^{14}C(CH_4)$ and ^{14}C in dissolved inorganic carbon, together with clumped isotope and biomarker constraints. Together, these measurements would be far more decisive than the radiocarbon age alone.

When the methane carbon is demonstrably recent and coupled to the dissolved inorganic carbon, its abiotic synthesis

$$CO_2 + 4H_2 \rightarrow CH_4 + 2H_2O$$

requires molecular hydrogen to be available during methanation. Recent methanation then does imply a contemporaneously active hydrogen supply. We therefore retain our original conclusion that these systems are presently active, while restating the radiocarbon result as an upper bound on the age of the youngest carbon component rather than the date of hydrogen generation. We thank the commenters for prompting this distinction, which we believe strengthens, rather than weakens, the case for ongoing hydrogen generation.

The Redox "Contradiction" is the Mechanism. Vidavskiy *et al.* (2026) identify what they regard as a fundamental inconsistency: the oxidation of Fe(II) that drives hydrogen generation is, in their reading, incompatible with the strongly reducing assemblages (awaruite, native iron, Fe–Ni alloys), observed in these rocks. They also note the absence of explicit reaction equations. The two observations are best answered together, because the equations resolve the supposed contradiction.

Serpentinisation is an internally coupled redox (disproportionation) process. The oxidation of ferrous iron and the reduction of water are two half-reactions of a single reaction. In the simplest case for the fayalite end member this reaction can be written,

$$3Fe_2SiO_4 + 2H_2O \rightarrow 2Fe_3O_4 + 3SiO_2 + 2H_2$$

where the ferrous iron in the fayalite is oxidised to ferric iron, here hosted in magnetite, while water is reduced to molecular hydrogen. The production of H_2 is the reduction term of the same reaction that oxidises the iron, so that there is no requirement for a separate "oxidising medium."

Awaruite and native iron are not evidence of an environment inconsistent with serpentinisation. Their presence is compatible with our model in one of two ways. These minerals may be products of advanced serpentinisation itself: where high water/rock ratios and high hydrogen fugacity drive oxygen fugacity below the magnetite field and into the stability region of these minerals (Mayhew *et al.*, 2013). Alternatively, where the paragenetic sequence places them early, these minerals may be relicts of a magmatic stage that predates serpentinisation. The two origins are texturally distinguishable: magmatic awaruite and native iron are primary, high temperature phases, whereas serpentinisation related awaruite forms at lower temperature as a secondary assemblage. Thus, paragenetic relationships can distinguish among these. Either way these phases are not incompatible with serpentinisation.

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For the same reason, the FMQ buffer is not the appropriate frame of reference for the formation of hydrogen. The oxidation potential of serpentinisation fluids is characteristically one to several log units below FMQ; the system is governed by reaction progress and disequilibrium, not by equilibrium with a fixed mineral oxygen buffer. The coexistence within one rock of oxidised products (magnetite, Fe(III) in serpentine) and reduced products (H_2, and, where serpentinisation derived, awaruite) is thermodynamically expected for a disproportionation reaction, and is observed wherever serpentinisation is documented.

The Hydrogen Isotopic Signature Argues for Low Temperature Water Reduction. The comment invokes the isotopically light hydrogen reported at Kempirsay (Ukhanov *et al.*, 1987) in support of its deep seated origin. We, however, would like to draw attention to what that signature actually implies. Strongly D-depleted molecular hydrogen, with $\delta D\text{-}H_2$ between −744 and −766 ‰ at Kempirsay and −743 ‰ at Bulqizë (our Table 1), is the expected product of the low temperature reduction of water, where the large equilibrium D/H fractionation between H_2 and H_2O drives $\delta D\text{-}H_2$ to strongly negative values. We recognise that a single $\delta D\text{-}H_2$ value, considered in isolation, is not uniquely diagnostic of a source, because this composition can be modified by secondary processes such as diffusion, mixing and re-equilibration with water. Our argument therefore rests not on the absolute values alone, but on their convergence between the two massifs.

Taken together with the geological context, such values are difficult to reconcile with hydrogen inherited from a deep or mantle reservoir, as deep hydrogen would not be strongly depleted in deuterium. The close similarity of the signatures across the two studied massifs, differing by more than 200 Myr in age and by a factor of two to three in geothermal gradient, (12 vs. 25–30 °C/km, Table 1 of the original article) is itself most simply explained by a common low temperature hydrogen generation mechanism. The very observation that the comment used to argue for a deep hydrogen origin is, upon closer reading, among the clearest isotopic arguments for water derived, low temperature H_2.

Reservoir Exhaustion Motivates an Open System; it does not Refute One. Vidavskiy *et al.* (2026) cite Murray *et al.* (2020), in which a closed, finite granitic reservoir is calculated to exhaust within about 150 years, and contrast this with the 250–400 Ma age of the host rocks. We agree with the premise, but come to the opposite conclusion. A closed, finite reservoir cannot sustain a geologically long lived hydrogen production. This is why we did not propose it in our original study.

Our model is not of a static reservoir slowly bleeding out hydrogen, but of an open system which involves inherited fault permeability, its periodic tectonic reactivation, and the progressive exposure of fresh Fe(II)-bearing peridotite to meteoric water. Within this model, the apparent paradox of long lived hydrogen flux from old rocks is resolved: although the rock is old, the reaction front and the water supply are continually renewed. We would add that the exhaustion argument applies to any model that attributes present day emissions to a finite, ancient gas charge, since such a reservoir is the configuration least able to sustain fluxes that are still active today.

The Role of Chromitite. The comment proposes regional, fault controlled degassing along steeply dipping channels as an alternative to chromitite proximity for hydrogen generation. We do not regard massif scale structure and chromitite control as alternatives. In our model these two factors are complementary, operating respectively at the kilometre and the 10^2 m scale. It should be noted, however, that Lidin *et al.* (1982) reported that at the Kempirsay massif hydrogen occurs essentially within ~300 m of chromitite bodies and was not reported from barren ultramafic units. Chromite and platinum group phases could have a catalytic role in the process as discussed by Mayhew *et al.* (2013), Foustoukos and Seyfried (2004) and Pappalardo *et al.* (2025). To date however this process is underexplored. Its quantification will require the dedicated experimental work we flag as beyond the scope of this discussion.

Ascent of an externally or deeply sourced gas along regional faults would lead to emissions distributed along those faults. This would not explain their confinement to chromitite halos, whereas *in situ* hydrogen generation at chromitites does. A structural control invoked without reference to the role of chromitite leaves the central spatial pattern of both our studied massifs unexplained. We note in passing that the report of occluded methane in unserpentinised Kempirsay ultramafics with no detectable hydrogen (Chashchukhin *et al.*, 2020), raised by the comment, is consistent with this framework: where serpentinisation has not occurred, trapped methane may persist while active hydrogen is absent. Active H_2 generation tracks serpentinisation rather than a pervasive, rock hosted charge.

Finally, on the preservation of reactive mantle domains over geological time, the mechanism is set out in our original Letter and illustrated in its Figure 2: mechanical partitioning of strain concentrates deformation and fluid flow within serpentinite corridors, while competent, chromitite bearing panels are translated as comparatively coherent blocks, so that fresh Fe(II) bearing peridotite survives beneath the hydration front and remains available for later reaction.

Concluding Remarks. We are grateful to the commenters for prompting these clarifications, two of which (the proper reading of the radiocarbon datum and the explicit statement of the redox coupling) are genuine improvements to our original presentation. Nevertheless, the evidence assembled in the comment (the hydrogen isotopic signature, the absence of hydrogen in unserpentinised rock, and the rapid exhaustion of finite reservoirs) is readily accommodated by the serpentinisation framework described in our original Letter.

Identifying the sources, whether active or fossil, of natural hydrogen is an evolving problem, and our original Letter is one step towards solving this problem. A productive path forward, in our view, is not the substitution of one inferred source for another, but the use of targeted, multi-tracer measurements, including paired $^{14}C(CH_4)$ and $^{14}C(DIC)$, clumped isotopes, noble gases, and biomarkers, that can discriminate among co-occurring hydrogen and methane sources at a single site. We would welcome the application of such a programme to the Kempirsay and Bulqizë systems, and we expect it to refine, and possibly lead to the revision of our original model.

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Additional Information

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