

Salinas-Matus et al. present an investigation into the effects of mesoscale eddies on the air-sea CO₂ flux within eddy rich regions of the Southern Ocean. They find geographical differences in the eddy effects on air-sea CO₂ flux between the Brazil-Malvinas confluence, Agulhas Retroflexion and the region South of Tasmania. They investigate the factors driving these differences in the air-sea CO₂ fluxes and find that the pCO₂ gradient ($\Delta p\text{CO}_2$) is the primary driver, which itself is mainly driven by differences in dissolved inorganic carbon (DIC). Using a temporal decomposition a further result shows that eddies act as persistent CO₂ sinks on decadal timescales but are more variable at shorter timescales. I find this study to provide a comprehensive analysis of the eddy rich regions of the Southern Ocean and the modulation of the air-sea CO₂ fluxes and can recommend for publication once my remaining comments below have been addressed (especially the analysis in Figure 5 which is unclear).

We would like to sincerely thank the reviewer for the very helpful and constructive comments and suggestions. We have carefully addressed each point below. The reviewer's comments are shown in bold font, and our responses are provided in normal font.

General comment: I'd suggest to aid readers, to change the cyclonic eddy results to be coloured blue, and the anticyclonic eddies to be coloured red. Although the legends are clear in defining the colours, the common conventions in previous work are for anticyclonic (generally 'warm') eddies are coloured red, and cyclonic (generally 'cold') eddies blue.

Yes, thanks for the suggestion. As you said, it's better to keep the convention that has already been used in other papers. In the new version, the colours have been reverted.

General comment: I'd suggest picking a different colour for the periphery to avoid having red and green to aid readers that are colour-blind.

Yes, completely makes sense. Done

Line 27: "Cyclone" should be "Cyclonic"

Done

Line 86: Can some details of the air-sea CO₂ flux parameterisation be mentioned? What was the parameterisation used for kw (and Schmidt number), and the solubility.

Thank you for pointing this out. We have now added details in Appendix A describing how the piston velocity (kw), Schmidt number, and solubility factor were computed for the analysis of the contribution of each term. These parameterisations are the same as those implemented in the model itself, which is why we reconstruct the CO₂ flux consistently with the model output. Specifically, we use Wanninkhof (2014) for both the Schmidt number and the gas transfer velocity, and Weiss (1974) for the solubility factor. The corresponding references have been added to the manuscript.

Appendix A: air-sea CO₂ flux terms effect

The air-sea CO₂ flux follows the empirical relationship from Wanninkhof (1992). This is determined by the solubility factor (S_{CO_2}), the piston velocity (k_w), and the air-sea pCO₂ difference ($\Delta pCO_2 = pCO_2^{ocean} - pCO_2^{atm}$).

$$CO_{2flux} = S_{CO_2} k_w \Delta pCO_2$$

350 where k_w scales with wind speed and decays with temperature (Wanninkhof, 2014). The Schmidt number (S_c) is fitted by the a 4th-order polynomial from Wanninkhof (2014) and normalized by the Schmidt number for CO₂ at 20°C of 660:

$$k_w = (1 - f) u^2 \left(\frac{S_c(SST)}{660} \right)^{-1/2}$$

$$S_c = a_1 - a_2 * SST + a_3 * SST^2 - a_4 * SST^3 + a_5 * SST^4$$

$$a_1 = 2116.8, a_2 = 136.25, a_3 = 4.7353, a_4 = 0.092307, a_5 = 0.0007555$$

355 and S_{CO_2} is calculated according to Weiss (1974)

$$\ln S_{CO_2} = A_1 + A_2(100 * SST) + A_3 \ln(SST/100) + SSS[B_1 + B_2(SST/100) + B_3(SST/100)^2]$$

$$A_1 = -60.2409, A_2 = 9345.17, A_3 = 23.3585, B_1 = 0.023517, B_2 = -0.00023656, B_3 = 0.0047036$$

where SST is sea surface temperature and SSS is sea surface salinity.

To isolate the effects of each term, we used the long-term mean state of the system as a baseline. By keeping the base-
360 line constant for each driver, we could subtract its effect from the total CO₂ flux to determine the individual contributions (Chikamoto and DiNezio, 2021).

$$CO_{2flux} \approx F_{piston} + F_{solubility} + F_{\Delta pCO_2}$$

$$F_{piston} = k_w(t) S_{CO_2}(t) \Delta pCO_2(t) - \overline{k_w(t_b)} S_{CO_2}(t) \Delta pCO_2(t)$$

$$F_{solubility} = k_w(t) S_{CO_2}(t) \Delta pCO_2(t) - k_w(t) \overline{S_{CO_2}(t_b)} \Delta pCO_2(t)$$

$$365 F_{\Delta pCO_2} = k_w(t) S_{CO_2}(t) \Delta pCO_2(t) - k_w(t) S_{CO_2}(t) \overline{\Delta pCO_2(t_b)}$$

where $\overline{k_w(t_b)}$, $\overline{S_{CO_2}(t_b)}$, and $\overline{\Delta pCO_2(t_b)}$ are the respective baseline values, defined as their long-term means.

The combined effects of each driver should approximate the total CO₂ flux. The reconstruction is an approximation because it does not account for the effects of sea ice, although our analysis was restricted to sea-ice-free regions.

Line 92: What was the native time resolution of the model, that was then collated into the daily averages?

The time step of the model is 600 seconds, and as you mentioned, then a daily average is done.

Line 113: What is the sensitivity of the results to the Okubo-Weiss parameter values used to define the background, eddy and periphery. Is this a commonly applied value (I don't see a reference for this selection)? The selection appears to identify large regions far from negative OW values, that get identified as the "periphery" in Figure 1a, which may suggest that this value is too relaxed?

Thank you for this important comment. We have expanded in the Eddy detection method section a clarification of the procedure used to determine the Okubo-Weiss (OW) threshold and provided references to previous studies. The text added is:

“...The “background” regime is defined using a threshold equal to 0.3 of the temporal mean of the spatial standard deviation of the OW (σ_{OW}) corresponding to $\pm 0.5 \times 10^{-10} \text{s}^{-2}$, encompassing all OW values within this range. The most common approach in the literature is to define the threshold as $0.2\sigma_{OW}$ (Schütte et al., 2016a; Vu et al., 2018), with some studies adopting more relaxed values around $0.1\sigma_{OW}$ (Beech et al., 2025). However, in this study we chose to apply a slightly stricter criterion in order to isolate more robust and well-defined eddy and mesoscale structures, while still retaining the main mesoscale (see supplementary figure S1).”

We have added a supplementary figure showing the OW for different threshold values. This comparison demonstrates that the selected threshold provides the most adequate balance between capturing coherent eddy cores and avoiding overly extended “periphery” regions.

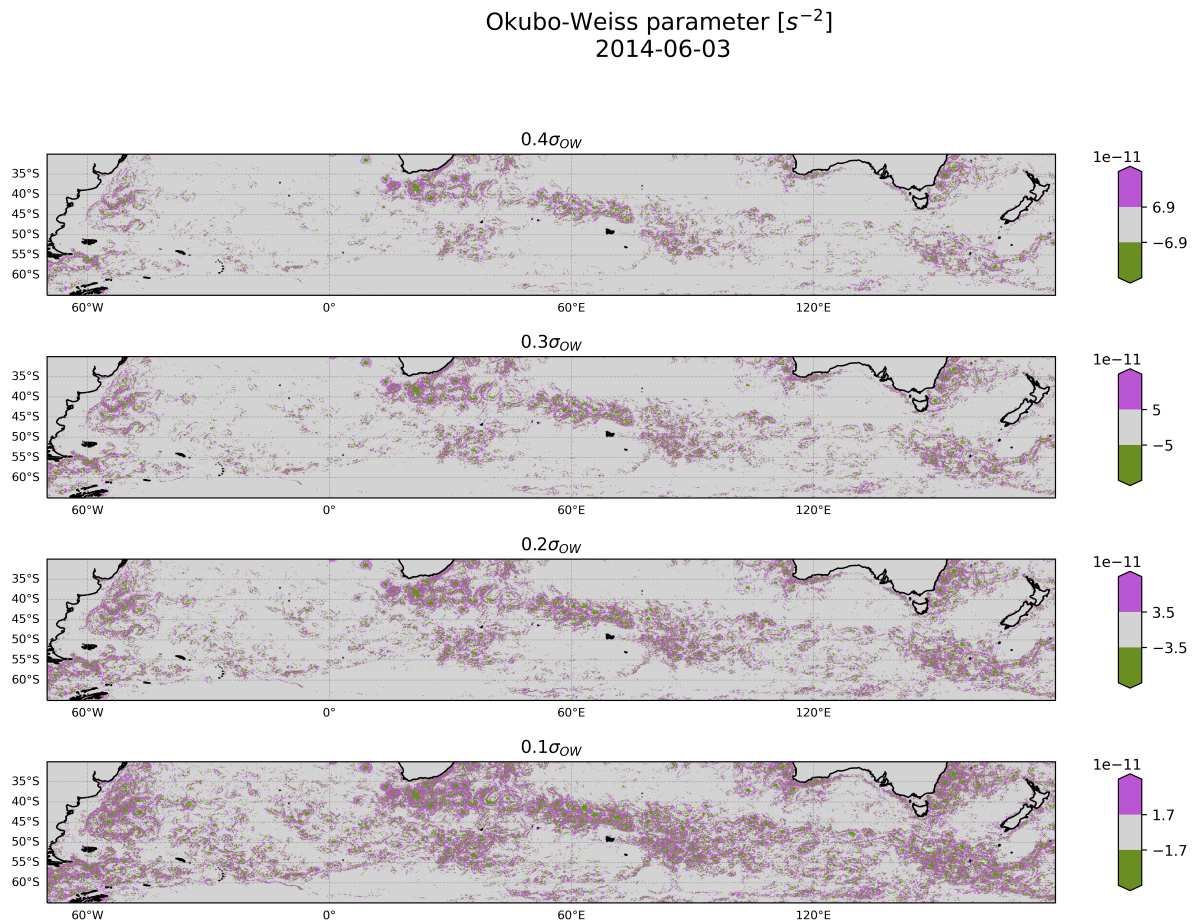


Figure S1. Sensitivity of mesoscale structure detection to different Okubo–Weiss (OW) threshold values. The panels show four threshold selections used to define mesoscale regimes. The threshold of $0.3\sigma_{OW}$ was used in this study, as it provides a balance between capturing well-defined eddy structures and preserving the main mesoscale variability.

Additionally, a sensitivity test was performed for the periphery and background, evaluating threshold limits of $0.2\sigma_{OW}$, $0.3\sigma_{OW}$, and $0.4\sigma_{OW}$. The results show same variability, and the differences in magnitude among these thresholds are minimal, confirming that the results are not sensitive to the specific choice of threshold.

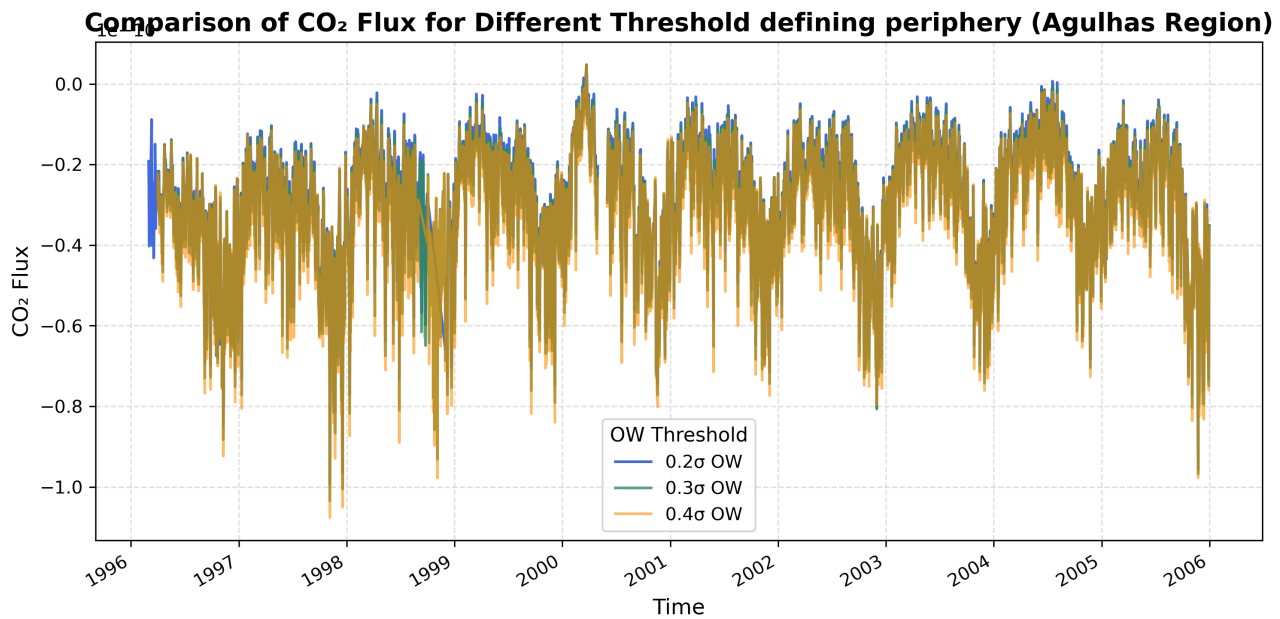


Figure: Results of the sensitivity analysis evaluating the impact of different threshold selections used to define the periphery regime in the Agulhas Retroflection region. The figure illustrates how variations in the threshold affect the composites of air–sea CO₂ fluxes.

Line 130: I'd suggest the final sentence isn't required - it could be moved to the conclusion or introduction if the authors would like to keep it.

We agree with you that the final sentence was not necessary in that section. Accordingly, we have removed it from the manuscript.

Figure 1: Suggest changing colour map in Figure 2b (bottom panel) for colour-blindness.

Done, we changed it to another color palette that is friendly for people with color blindness.

Line 170: Id suggest more is said surrounding the “background” conditions in the Agulhas retroflection region being cooler than the cyclonic eddies. The cyclonic eddies would be forming from the cool side of the retroflection and therefore should be cooler than the background as this water wouldn't be originating in the Indian Ocean. Figure 3a shows the cooler water on Southern Ocean side of the retroflection, and Figure 2b shows SST anomalies for cyclonic eddies being negative. The background conditions also appear based on Figure 1a to be originating from the Indian South Subtropical gyre with warmer temperatures.

We thank the reviewer for this careful comment, which we had initially overlooked. We identified labeling errors in Figure 3b, during the creation of the Python dictionary used to assign the data to each flow regime, the cyclonic composites were incorrectly mapped to the periphery keyword, the periphery to background, and the background to cyclonic. We have now corrected Figure 3b accordingly. In addition, we reviewed all other figures and confirmed that no similar labeling errors are present.

We modified the text as follows:

“In our simulations, flow regimes exhibit distinct characteristics that influence the CO₂ flux

(Fig. 2b-d), with contrasting vertical structures of cyclonic and anticyclonic eddies emerging (Fig. 3b), consistent with previous findings (Keppler et al., 2024). Anticyclones, which tend to induce water downwelling, exhibit higher temperatures and lower DIC concentrations. In contrast, cyclones exhibit lower temperatures and higher DIC concentrations, mostly driven by water upwelling. The periphery regime shows intermediate properties, representing the transition zones surrounding both anticyclonic and cyclonic eddies (Fig. 3b). While this general pattern is evident across all three regions, the magnitude of the differences varies with latitude and local dynamics, reflecting the influence of regional circulation.”

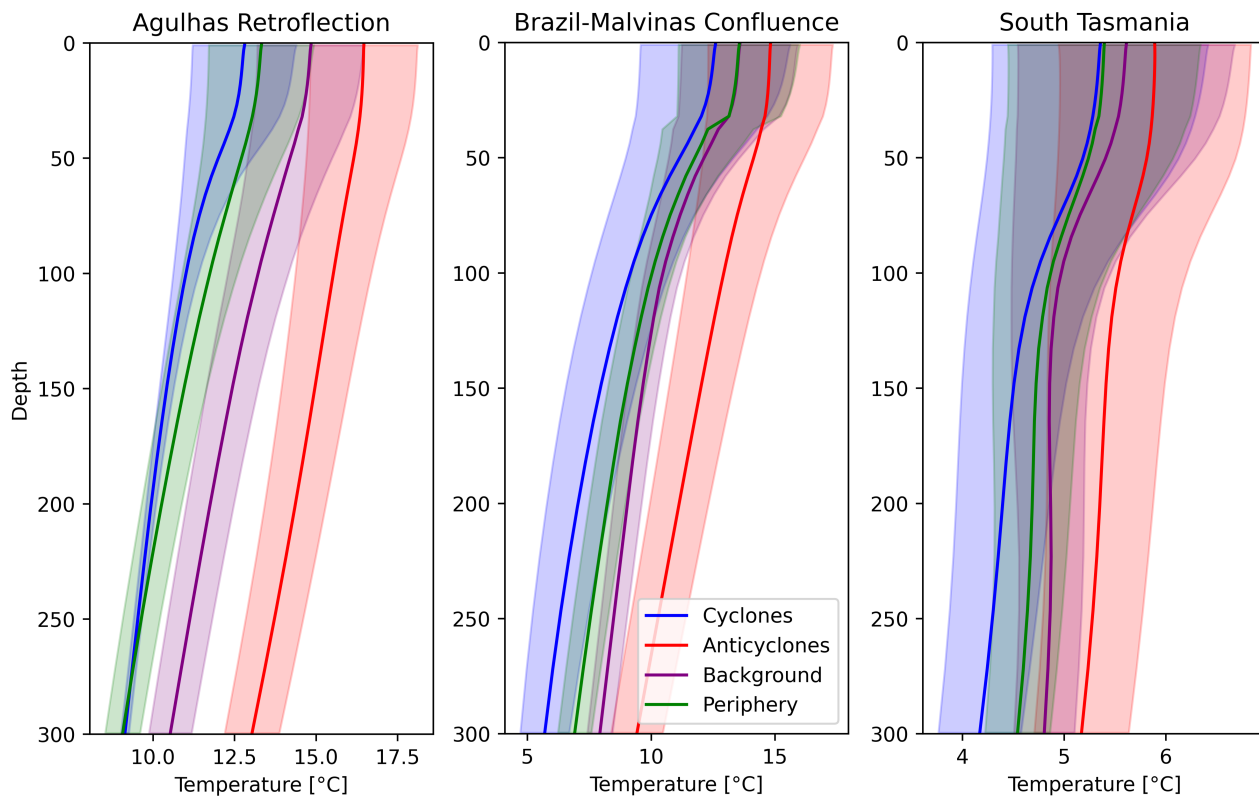


Figure 3. ... (b) Vertical temperature and DIC profile composites for the flow regimes within the defined regions, ...

Line 201: I'd suggest more is said on the Tasmania region eddies and the sporadic events. These sporadic high uptake events seem to be more prevalent in the Tasmania region, compared to the other regions, and have a large effect on the Figure 4 uptake results. As pCO₂ is the dominant driver in the integrated fluxes, could you elaborate on a mechanism?

The paragraph as previously written could be misleading. What we meant is that strong carbon uptake events are present across all flow regimes, including the background, and that when integrating over all regimes, the net result for the region is CO₂ uptake. We have also added a reference discussing periods of enhanced upwelling that transport DIC from the deeper ocean, thereby reducing carbon uptake in the region. In addition, we expanded the discussion on the anomalous contribution of mesoscale regimes, following the suggestion from Reviewer 2. The revised paragraph now reads:

“Tasmania region exhibits a contrasting pattern, as the “background” has the highest CO₂ uptake (Fig. 4a). Here, both anticyclonic and cyclonic eddies take up less or outgas more carbon (Fig. 4c). This pattern is influenced by the weaker eddy intensity (Fig. 1c) and the characteristics of the region. Most eddies in the Tasmania region are located in the high-DIC band, which limits the ocean’s capacity for CO₂ uptake (Fig. 3b). Nevertheless, anticyclonic and cyclonic eddies take up carbon in the entire analyzed period (Fig. 4a), a pattern influenced by sporadic but intense carbon uptake events occurring across all flow regimes in the region (Fig. 3). These events may be associated with periods of reduced upwelling activity, which limit the vertical transport of DIC-rich subsurface waters to the surface, thereby allowing enhanced CO₂ uptake (Pardo et al., 2017). However, since the efficiency of CO₂ uptake in mesoscale flow regimes is lower than in the background, the anomalous contribution is negative. Mesoscale regimes take up approximately 17% less carbon compared to what would be expected under background conditions. (Table 3).”

Table 2: It is unclear what the $\pm$ values denote. Is it mean $\pm$ the standard deviation?
We added a clarification in the table description:

“Table 2. Area-integrated carbon uptake contributions by region and mesoscale flow regime. The value beneath each region name indicates the total carbon uptake (including background, eddies, and periphery). The values in parentheses represent the contribution of each flow regime as a percentage, first relative to the total uptake (in bold), and second relative to the combined uptake from all mesoscale flow regimes (eddies + periphery). All values are reported as mean $\pm$ standard deviation.”

Line 253: Could the greater influence of kw be due to the increase in wind speeds generally observed over anticyclonic eddies? (Frenger et al., 2013) and the opposite for cyclonic eddies?

Indeed, some observational and modelling studies have shown that mesoscale SST anomalies associated with eddies can imprint on the overlying atmosphere, producing local wind-speed anomalies that tend to be positive over warm (anticyclonic) anomalies and negative over cold (cyclonic) anomalies (e.g. Frenger et al., 2013; He et al., 2020; Ji et al., 2020). However, our study uses an ocean-only model forced by prescribed atmospheric fields (two-way atmosphere–ocean is absent). Therefore, eddy-scale modifications of wind speed are only present in our simulations if they already exist in the prescribed atmospheric forcing; they are not generated dynamically by the model. In our case, the atmospheric forcing is ERA5 (as stated in the Experiment Design section). ERA5 does contain some representation of eddy-scale wind anomalies, although the signal is generally attenuated compared to satellite-based products or fully coupled simulations. This constitutes a limitation of our study, We have added a sentence in the Experiment Design section to clarify this:

“...As this is an ocean-only configuration without coupling to the atmosphere, eddy-induced feedbacks on surface winds are not represented.”

Line 264: How was the pCO₂ (atm) prescribed? Can details of this be added in the methods for model setup?

The atmospheric pCO₂ used in the model was prescribed from the Global CO₂ concentration dataset (in ppm) prepared for the Global Carbon budget (Friedlingstein et

al., 2022). We have now added the citation in the Experiment design section:
“...The atmospheric pCO₂ used in the model was prescribed from the global CO₂ concentration dataset prepared by the Global Carbon Project (Friedlingstein et al., 2022).”

Line 271: Figure 5 captions indicate these regressions are for $\Delta p\text{CO}_2$ regressed against the thermal and non-thermal components separately, where as the text indicates this is the total flux. Based on the regressions I think this is each component regressed against the total flux. The analysis in this form may not answer the aim as the contributions of the thermal and non-thermal components to the changes in $\Delta p\text{CO}_2$ would be combined with variability in k_w (and other inputs to the fluxes) when regressing against fluxes. I am unsure of the aim of this portion of the analysis and suggest the authors should clarify this analysis (Lines 264-275).

Thank you very much for pointing this out. The text is incorrectly phrased. What we actually did in this analysis was to regress oceanic $\Delta p\text{CO}_2$ against its thermal and non-thermal components separately, as correctly shown in the description of Figure 5. The mention of the “total flux” in the text was a writing error that escaped our revision. We are not sure why this may have given the impression that the regression was against the total flux, but we will clarify the wording to avoid any possible confusion. What we had was the time series of $\Delta p\text{CO}_2$ decomposed into its thermal and non-thermal components, which add up to the total $\Delta p\text{CO}_2$. Each of these components was regressed against $\Delta p\text{CO}_2$ in order to explain the variability.

We corrected the text accordingly to clarify that the regressions refer to $\Delta p\text{CO}_2$ and its components, not to the total flux:

“The regression of the pCO₂ocean on the non-thermal component, primarily driven by DIC, exhibits high R² values (Fig. 5), indicating that a larger proportion of pCO₂ocean variability is explained by this component.”

Code availability: Code is available to do sections of the analysis but was unable to find plotting scripts to complete the analysis.

The scripts used to generate the figures will be included in the same repository as the analysis code when the revised version of the manuscript is resubmitted.

Data availability: I note no data availability statement. Are these fields available or can they be requested? I expect not due to data volume, but this should be stated. These fields appear very useful for studying mesoscale eddies over a long time period, so could be useful for the community.

A data availability statement will be included in the revised submission. While the full dataset is very large, we will provide clear information on how the fields can be accessed or requested.

References

Frenger, I., Gruber, N., Knutti, R., & Münnich, M. (2013). Imprint of Southern Ocean eddies on winds, clouds and rainfall. *Nature Geoscience*, 6(8), 608–612. <https://doi.org/10.1038/ngeo1863>