In response to this prior forcing, the EBM of Johansson *et al.* must simulate a large negative value of ΔT^{Kr}. In contrast, observations show only weak cooling after Krakatoa, probably due to sparse coverage of tropical GMST data in the 1880s. This mismatch in simulated and observed ΔT^{Kr} values may provide part of the reason for the downward scaling of the prior volcanic forcing. Because the scaling coefficient on the forcing is being estimated over the period 1880 to 2011, all temperature responses to major eruptions are downscaled — not just the Krakatoa GMST response.

A second issue relates to collinearity between the individual temperature components — volcanic, solar, El Niño Southern Oscillation (ENSO) and anthropogenic — used in the regression with observational GMST and ocean heat content (OHC) data. Collinearity between ENSO and volcanic forcing is relatively weak over the full 1880 to 2011 analysis period. In the 1980s and 1990s, however, El Niño events masked some of the cooling caused by El Chichón and Pinatubo[5,6,8]. This hampers reliable estimation of the true cooling signals caused by these eruptions — which may in turn impact scaling of the prior volcanic forcing.

Johansson *et al.* estimate equilibrium climate sensitivity (ECS) from long-term trends in GMST and OHC, and infer a "most likely" ECS value of ~2.5 °C. Empirical ECS estimates have also been obtained from a variety of other sources,

such as the maximum cooling after large volcanic eruptions[6]. In simulations performed with an EBM similar to that of Johansson *et al.*, a ΔT^{Pn} value of −0.2 °C implies an ECS < 1 °C (ref. 6). Johansson *et al.* argue that the ECS inferred from their ΔT^{Pn} value is consistent with their trend-based ECS estimate — but this apparent agreement only occurs because of their unrealistically low posterior value of F^{Pn}.

Finally, Johansson *et al.* report small error bars on estimated trends in $\Delta T_{\{t\}}^{Vol}$ over the recent 'warming hiatus' (see results for 'Volcanic aerosols' in their Table 1). These error bars do not accurately reflect known shortcomings and uncertainties in volcanic forcing[9,10]. The forcing data used by Johansson *et al.* omit a significant component of volcanic aerosol from the lowermost stratosphere. Inclusion of this component would enhance the surface cooling caused by post-2000 eruptions[9,10]. Short-term trends in $\Delta T_{\{t\}}^{Vol}$ over the hiatus are also influenced by GMST recovery from Pinatubo (and from the prior cooling caused by the eruptions of Agung and El Chichón). Thus the hiatus period trends in $\Delta T_{\{t\}}^{Vol}$ are not only sensitive to treatment of post-2000 volcanic effects, but also to volcanic forcing and EBM response uncertainties in the second half of the twentieth century. The true uncertainties in the volcanic component of GMST changes (and in trend-based estimates of ECS) are likely to be larger than those reported by Johansson *et al.*

In summary, it is concerning that Johansson *et al.* infer a posterior average estimate of F^{Pn} that is substantially smaller than that obtained from observations. We believe Johansson *et al.* underestimated the posterior values of both F^{Pn} and (in consequence) ΔT^{Pn}. Future Bayesian inference studies should incorporate physically based constraints on the estimated posterior forcings and temperature responses. ❏

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Reply to 'Volcanic effects on climate'

Johansson *et al.* reply – Our Letter[1] aimed to estimate how probability density functions of the equilibrium climate sensitivity (ECS) change as observations accumulate. To elucidate the causes of these changes we decomposed the observed global mean surface temperature (GMST) variability over the instrumental period into four contributing factors: anthropogenic, solar and volcanic forcing, and the El Niño Southern Oscillation (ENSO). Santer *et al.*[2] argue that we underestimate volcanic forcing in response to the Pinatubo eruption (in absolute terms), and its contribution to temperature variability. While our estimate of Pinatubo forcing is weaker than the observational estimates they cite, we believe that it is justifiable and not crucial for our conclusions regarding the estimate of ECS and how it has been affected by the hiatus period.

Santer *et al.* assert that our estimate of Pinatubo forcing is based on statistical arguments alone. However, our analysis combines empirical estimates, knowledge of physical processes, and statistical methods. The empirical estimates of Pinatubo forcing they refer to fall within the range of our prior distribution for the uncertainty in volcanic forcing, which has a 95% interval of −5.2 to −1.1 W m⁻². Observations of many other quantities (such as GMST[3], ocean heat content[4] and atmospheric concentrations) inform additional prior distributions. Knowledge of ECS and physical processes such as ocean heat uptake and ENSO variability, as represented in a simple climate model with prior uncertainty distributions for its parameters, is also incorporated. The Bayesian statistical method used formally combines observations and knowledge of physical processes to

produce revised estimates of uncertainty (posterior distributions).

The statistical component of the analysis is critical because it allows each posterior distribution to be based on a wider range of information than its corresponding prior. In the case of volcanic forcing, the posterior is based not only on observations of volcanic forcing, but also on their consistency with observations of other quantities and with process knowledge. Considering all of these factors combined, our analysis suggests volcanic forcing (including Pinatubo) is more likely to be towards the weaker end of the prior range. This result is consistent with other studies[5–7] and produces a temperature response to Pinatubo that is in line with detection and attribution studies assessed in IPCC AR5[8]. Santer *et al.* argue that the value is more likely to be towards the stronger end, based only on observations

of Pinatubo forcing. In addition, each observation they cite is a single best estimate of forcing, and therefore their range only provides an estimate of the mode of the uncertainty distribution, underestimating the full distribution.

We are not convinced by Santer and colleagues' proposed explanations for our weak posterior estimate of volcanic forcing. The estimate is unlikely to be strongly affected by the lack of a strong cooling response in the observations to the 1883 eruption of Krakatoa. The observational errors at the time of this eruption are large[3], and by including this fact in our analysis these observations do not strongly constrain our estimated parameter values. A related study[9] finds that the exclusion of observations before 1900 only has a small impact on the ECS estimate.

In our view, it is too early to draw any conclusions on how the existence of collinearity between volcanic radiative forcing and ENSO affects results. The correlation coefficient between our estimated average GMST signals from volcanoes and from ENSO is only −0.19, suggesting that it is not a critical issue on the timescale of our study. It is unclear how higher correlation over shorter periods could be taken into account without introducing subjective constraints in the statistical model.

In our view, there are two more likely explanations for our results: (1) our estimate should be seen as an estimate of the effective radiative forcing of volcanoes, which is believed to be weaker than the radiative forcing[10,11]; (2) the class of climate model we use is structurally simple. One important simplification is that the ECS is constant. Results based on models with a richer description of the climate system have suggested that the ECS may be state-dependent and increasing over time[12–14]. This state and time dependency of climate sensitivity is likely to have a larger impact on the forcing–temperature relationship over a yearly timescale relevant to volcanoes than over a decadal-to-century timescale. Our estimate of ECS primarily reflects the decadal-to-century timescale response, and therefore may indicate a weaker volcanic forcing than we would find with a more flexible climate system response.

Accounting for this type of structural uncertainty is an important future research direction. Improvements in estimates of uncertainty in observations, including (but not limited to) volcanic forcing would also be important[15]. For these reasons, we do not exclude the possibility that the true uncertainty for ECS is larger than what we presented. As we stated in our Letter, all empirical estimates of ECS should be interpreted with some care. However, we do not see the views of Santer *et al.* as significantly challenging our estimate of ECS. ❑

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CORRESPONDENCE:

Bistability and the future of barrier islands

To the Editor — Writing in *Nature Climate Change*, Dúran Vinent and Moore[1] suggest that barrier islands are intrinsically bistable, existing in one of two equilibrium states, and that the state can be used to predict island response to climate change. Bistability exists when any system can exist in two distinct states of equilibrium[2]. Applied to barrier islands, Dúran Vinent and Moore claim that extrinsic variable changes (that is, reduced sediment supply) alter system parameters (dune-building) and can push entire islands across a threshold from one equilibrium state to an alternate equilibrium state, from high to low, for example. This results in

fundamental changes to system properties and affects island resilience. However, barrier islands do not function within a dichotomous framework of bistability where 'low' islands are simply an alternate equilibrium state.

Empirical evidence of bistability at the island level supplied in figure 1 of Dúran Vinent and Moore[1] ignores that many of the Virginia islands exist in both high and low states[3] (Fig. 1). Parramore Island, characterized as a low island[1] displays areas of high elevation near the northern inlet where sediment supply is greater, allowing accumulation and dune-building processes to dominate. Dune

ridges >2 m in height exist nearshore, with inland relict dunes >9 m in height and extensive woody cover, including maritime forest. However, some areas of woody cover are now being lost to shoreline retreat[3,4]. Extensive erosion along the transverse shoreline has led to dune scarping, despite high-elevation dunes. Although the presence of high dunes and woody vegetation may slow the overall erosion of the shoreline, external changes in longshore currents and island position are eroding these high-elevation sections of the island.

Hog Island, described as a high island[1], exhibits both high and low elevation