CORRESPONDENCE

Reply to "Comments on 'Rethinking the Lower Bound on Aerosol Radiative Forcing'"

BJORN STEVENS

Max Planck Institute for Meteorology, Hamburg, Germany

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ABSTRACT

This reply addresses a comment questioning one of the lines of evidence I used in a 2015 study (S15) to argue for a less negative aerosol radiative forcing. The comment raises four points of criticism. Two of these have been raised and addressed elsewhere; here I additionally show that even if they have merit the S15 lower bound remains substantially ($0.5\,\mathrm{W\,m^{-2}}$) less negative than that given in the AR5. Regarding the two other points of criticism, one appears to be based on a poor understanding of the nature of S15's argument; the other rests on speculation as to the nature of the uncertainty in historical SO_2 estimates. In the spirit of finding possible flaws with the top-down constraints from S15, I instead hypothesize that an interesting—albeit unlikely—way S15 could be wrong is by inappropriately discounting the contribution of biomass burning to radiative forcing through aerosol–cloud interactions. This hypothesis is interesting as it opens the door for a role for the anthropogenic (biomass) aerosol in causing the Little Ice Age and again raises the specter of greater warming from ongoing reductions in SO_2 emissions.

Following the notation introduced by Stevens (2015, hereafter S15) I denote the anthropogenic aerosol forcing by F_{aer}, and globally averaged SO_2 emissions by $\overline{Q}_a(t)$, where t denotes time, measured in years. For the sake of argument, assume that both aerosol–cloud and aerosol–radiation interactions contribute to forcings that scale linearly with $\overline{Q}_a$, as advocated by Kretzschmar et al. (2017), and also Booth et al. (2018). In this case,

$$F_{\mathrm{aer}}(\overline{Q}_a) = -\alpha\overline{Q}_a. \qquad (1)$$

By requiring the forced (globally averaged) temperature response to have the same sign as its forcing over a given time interval, S15 hypothesized that the warming over the first hundred years of industrial society (1850–1950 CE; hereafter all dates are CE) usefully constrains α such that

$$\alpha < \frac{F_{\mathrm{ghg}}(1950) - F_{\mathrm{ghg}}(1850)}{\overline{Q}_a(1950) - \overline{Q}_a(1850)}. \qquad (2)$$

Here F_{ghg} is the forcing from long-lived greenhouse gases, including CFCs. Through Eq. (1), α determines the value of F_{aer} for the "present day," which in S15 was taken to mean the year 2005. Estimates of the different components of Eq. (2) are provided in Table 1, and yield $\alpha < 12\,\mathrm{W\,yr\,(m^2\,Tg\,SO_2)^{-1}}$. This then sets $\lfloor F_{\mathrm{aer}}$—a symbol I introduce to denote the central estimate for the lower bound, or floor, for the present-day aerosol forcing—such that

$$F_{\mathrm{aer}}(2005) \geq \lfloor F_{\mathrm{aer}} = -1.4\,\mathrm{W\,m^{-2}}. \qquad (3)$$

By giving confidence intervals for each of the terms in Eq. (2) one could in turn derive confidence intervals for α and $\lfloor F_{\mathrm{aer}}$. As elaborated on below, because Eq. (3) subsumes several of the points raised by Booth et al. (2018), it serves as a useful starting point for thinking about the effects of different assumptions on estimates of $\lfloor F_{\mathrm{aer}}$.

The term $\lfloor F_{\mathrm{aer}}$ as given by Eq. (3) could be positively biased (insufficiently negative) 1) if estimates of $F_{\mathrm{ghg}}(1950) - F_{\mathrm{ghg}}(1850)$ are too small, 2) if estimates of $\overline{Q}_a(2005)/[\overline{Q}_a(1950) - \overline{Q}_a(1850)]$ are too small, or 3) if

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Corresponding author: Bjorn Stevens, bjorn.stevens@mpimet.mpg.de

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TABLE 1. Estimates of greenhouse gas forcing F_{ghg} and anthropogenic SO_2 emissions $\overline{Q}_a$ for different years. Estimates of $\overline{Q}_a$ are taken from S15, and from the Stevens et al. (2017) analysis of data published by Hoesly et al. (2018), denoted here as S17.

Year	F_{ghg}(W m^{-2})	$\overline{Q}_a$ (S15) (Pg yr^{-1})	$\overline{Q}_a$ (S17) (Pg yr^{-1})
1850	0.185	4.3	2.1
1950	0.891	63.3	58.2
1960	1.1052	88.4	90.5
2005	2.649	118.5	115.5

the changing composition or spatial pattern of the anthropogenic aerosol caused α to become larger with time. As to the first point, one reason that the S15 estimate of F_{ghg} (1950) − F_{ghg} (1850) may be too small is that new calculations (Etminan et al. 2016) suggest that methane is more effective as a greenhouse gas than was previously estimated. Then again, estimates of midcentury methane emissions have been revised substantially downward (Hoesly et al. 2018) and this would have the opposite effect. Additionally, the S15 assumption that the residual of a number of minor forcings, ranging from land-use change (negative) to changes in ozone and centennial variability in volcanic and solar forcing, was negligible might miss a small positive contribution to the forcing. Based on estimates in the literature [cf. Lewis and Curry (2018) and Annex II of the Working Group 1 contribution to the Fifth Assessment Report] this could inflate the S15 estimates of F_{ghg} (1950) − F_{ghg} (1850), and hence the magnitude of the lower bound given by Eq. (3) by 20%. To arrive at a substantially lower (more negative) $\lfloor F_{aer}$ Booth et al. (2018) follow the second tack, and argue (their third point) that unpublished and outdated (ca. 2004) SO_2 emissions allow, following Eq. (3), for a more negative $\lfloor F_{aer}$. This approach conflates error with uncertainty, but nonetheless highlights the value of better quantifying historical emissions inventories; indeed, doing this could conceivably raise the floor on F_{aer}. Emission inventories recently updated by Hoesly et al. (2018) have, however, a near-negligible (2.5%, or 0.03 W m^{-2}) impact on estimates of $\lfloor F_{aer}$. The third point was extensively discussed by S15, but not directly raised by Booth et al. (2018). Nonetheless, their Fig. 1 suggests that to the extent that this is an important effect in their model, it is acting to mask an otherwise concave relationship between $\overline{Q}_a$ and F_{aer} so that the net effect of a secular increase in α on $\lfloor F_{aer}$, as estimated by Eq. (3), is negligible.

In addition to the converse of the Booth et al. (2018) arguments, there are structural reasons as to why Eq. (3) may give a too negative (pessimistic) estimate of $\lfloor F_{aer}$. Three of these, discussed in varying detail by Booth et al. (2018), are outlined below:

1) Any concavity ($dF_{aer}/d\overline{Q}_a$ decreasing in magnitude with $\overline{Q}_a$) would lead to a less negative $\lfloor F_{aer}$. Following Kretzschmar et al. (2017), Booth et al. (2018) take CMIP models at face value—something often excused due to a purported lack of better alternatives, but which, given the well-documented deficiencies of the CMIP model's representation of aerosol forcing (Boucher et al. 2013; Stevens 2015; Stevens and Fiedler 2017; Malavelle et al. 2017; Toll et al. 2017), conflates error with uncertainty—and use them as a basis to argue against the idea that F_{aer} ($\overline{Q}_a$) is concave (their first point). The idea that F_{aer} ($\overline{Q}_a$) is concave is not an idea introduced by S15; it has long been a staple of aerosol modeling (Boucher and Pham 2002; Carslaw et al. 2013), which as shown by S15 is consistent with the best estimates of F_{aer} as given in the AR5 (see Fig. 3 in S15) and studies by Carslaw et al. (2013) and Myhre et al. (2013). Indeed, the analysis in S15 was substantially motivated by the Carslaw et al. (2013) invocation of concavity (their Fig. 3) to argue for the importance of knowledge of the preindustrial aerosol to estimate F_{aer}.[1] In addition to assessing the implications of the concavity argument for global forcing, S15's novel contribution was actually to outline reasons why concavity might not be as important as emphasized in the earlier literature, reasoning that motivated the development (Stevens et al. 2017) and application (Stevens and Fiedler 2017) of the multiplume model to account for the possibility of such effects. The upshot is that concavity in the relationship between F_{aer} and $\overline{Q}_a$ plays a relatively minor role in S15—a value of −1.4 W m^{-2} as estimated here without concavity [e.g., Eq. (3)] as opposed to −1.3 W m^{-2} as estimated by S15 with concavity—but *any* concavity implies that $\lfloor F_{aer}$ as estimated by Eq. (3) would be too negative.
2) S15 conjectures that it is implausible that the region of Earth—the North Atlantic and adjacent continents—that had the greatest (many times the global mean) aerosol loading through the twentieth century should be among the regions that simultaneously warm the most. Kretzschmar et al. (2017) uses CMIP5 simulations to argue that substantial warming in the hemisphere where the forcing is most negative is less implausible than one might think, an argument that Booth et al. (2018) reiterate (their second point). As already discussed by Stevens and Fiedler (2017), the contra-indicative result from the analysis of a small subset of CMIP5 models would be

[1] K. Carslaw (2018, personal communication) indicated that he was thinking of this as a local argument, but this was not stated in the manuscript, which only talked about global effects, nor is it consistent with the evocation of the preindustrial rather than the pristine airmass aerosol in that manuscript.

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more compelling if the pattern and magnitude of the temporally evolving clear-sky aerosol forcing in those models were more plausible. Inverse modeling studies, with more strongly constrained aerosol forcing patterns, provide further reason to be skeptical of the Kretzschmar et al. (2017) argument. In these studies, models that latitudinally resolve the forcing and response yield a substantially less negative $\lfloor F_{\mathrm{aer}}$ as compared to studies based only on global means (Forest 2018). An attempt by S15 to incorporate such hemispheric constraints reduced the magnitude of the lower bound by 25%, yielding $\lfloor F_{\mathrm{aer}} = -1.0\,\mathrm{W\,m^{-2}}$, not too different from the $-1.15\,\mathrm{W\,m^{-2}}$ derived from the inverse modeling studies. Hence the additional constraints are potentially large (25% to 30%). I continue to think that it remains reasonable to suppose that a consideration of the spatial pattern of the forcing, along with the associated response in regional (and seasonal) surface temperatures, should more strongly constrain $\lfloor F_{\mathrm{aer}}$, but agree that S15's quantification of this effect is rather speculative.

3) S15's energy budget analysis does not apply equally to all time intervals, as it rests on two ideas: one being that—to separate forcing from feedback—the forced temperature response should share the same sign as its radiative forcing; the other being that the time period that gives the strongest constraint is the most useful. Thus, and in marked contrast to my understanding of Booth et al.'s fourth point, the choice of time interval is essential. For time intervals that are too short, or chosen in a way that gives too much weight to changing natural forcings (e.g., from volcanoes[2]) then it is not possible to separate the forced temperature response from natural variability. Consideration of time intervals that imply an unambiguously positive net forcing risks conflating feedbacks with forcing, something S15 expressly attempts to avoid. My interpretation of Booth et al.'s Fig. 2 is that the climate sensitivity of their model is too large. This, not an insufficiently negative F_{aer}, is then what causes the late-century warming to be overestimated in those runs whose F_{aer} is more in line with S15's arguments and whose temperature better matches the midcentury warming. S15 identified the mid-twentieth century as being a critical period precisely because it had a secular temperature trend that lay outside of natural variability (even including for the rebound from early-century volcanism) as estimated from a 100-member historical simulation (S15), and because it constrained $\lfloor F_{\mathrm{aer}}$ to a degree that implied a substantial reduction in aerosol forcing uncertainty. Even so, and in retrospect, it was a somewhat conservative estimate; the argument applies equally to the period between 1850 and 1960, sill prior to the 1963 eruption of Agung, and applying it over this period (e.g., Table 1) leads to a substantially less negative $\lfloor F_{\mathrm{aer}}$ $(-0.9\,\mathrm{W\,m^{-2}})$.

Taking the above arguments into consideration, I see no reason to question the central point of S15, which is that a consideration of the midcentury temperature record, and best estimates of anthropogenic aerosol and aerosol precursor emissions, supports the other lines of evidence presented in S15 in indicating that the more negative range of estimates of aerosol forcing as given by Boucher et al. (2013) is implausible. One can argue as to whether the ideas outlined above limit $\lfloor F_{\mathrm{aer}}$ to $-1.0\,\mathrm{W\,m^{-2}}$ or perhaps only to $-1.6\,\mathrm{W\,m^{-2}}$. To argue for a more negative lower bound requires somewhat more creativity.

Even if I do not find the combination of arguments that Booth et al. (2018) advance for the plausibility of $F_{\mathrm{aer}} < -1.6\,\mathrm{W\,m^{-2}}$ to be particularly compelling, the exercise of attempting to reconcile S15's "top-down" constraints on $\lfloor F_{\mathrm{aer}}$ with a more negative forcing is a constructive one, which I take up from a different perspective in closing. For the sake of argument, suppose that forcing from aerosol–cloud interactions was somehow large in magnitude, large enough for F_{aer} to be consistent with the lowest (most negative) quartile of its range as assessed by Boucher et al. (2013). This would imply a substantially more negative forcing from aerosol–cloud interactions than allowed for by S15, near the upper limit (in magnitude) of what is inferred by recent observational studies (McCoy et al. 2017) for cloud intrinsic responses to aerosol perturbations, and allowing for a substantial cloud extrinsic effects. Here, however, there is growing weight from a diversity of evidence against a substantial extrinsic effect (Seifert et al. 2015; Toll et al. 2017; Malavelle et al. 2017; Stevens 2017; see also references therein), despite the difficulty of ruling it out based on satellite observations alone (Christensen et al. 2017; Gryspeerdt et al. 2016). One way to reconcile such a negative forcing with Eq. (3) is to assume that estimates of emissions from biomass burning[3] prior to 1850—whose contribution to aerosol

[2] The idea that the residual noise from subtracting a volcanic signal whose magnitude is only roughly known outweighs the additional signal one might obtain by extending the analysis into periods with a substantial volcanic forcing seems at least as adventurous as my idea that the hemispheric response to hemispheric forcing adds additional constraints on the forcing as compared to a global analysis.

[3] Because of its lower single scattering albedo the biomass burning aerosol is thought to have a more dominant contribution to aerosol–cloud as compared to aerosol–radiation interactions.

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forcing are not directly considered by S15—are underestimated in present emission inventories. This would imply a substantially more negative aerosol forcing concentrated over the developing population centers of North America and western Europe in the centuries prior to 1850, which would have then been supplanted by aerosol forcing from SO_2 emissions as industrialization gained steam. In this case, F_{aer} could be quite large in magnitude, but $F_{aer}(1950) - F_{aer}(1850)$ could remain rather modest and thus not come into conflict with the arguments of S15. Such a scenario, admittedly a little far-fetched, has broad implications, both for understanding the Little Ice Age in the past and for the consequences of desulfurization of emissions from fossil fuel combustion in the future.

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