Thank you for your thoughtful comments. Including your suggested revisions has improved the quality of the manuscript. Our responses are indicated below in blue text.

This paper describes advancements in the modeling of global Hg cycling through up- dated redox chemistry as well as coupling to a more sophisticated ocean model. The approach is thorough and defensible and the paper well-written. I recommend publica- tion after minor revisions.
P5, l. 23-25: Can you add a summary of how accurate these OA concentrations are expected to be? If this isn't known, how does that impact your conclusions?

We respond to this and other reviewers' comments on a similar topic below.
We refer to a study evaluating modeled OA in detail, adding the following sentence (Page 6, lines 3-5): "An evaluation of modeled OA against aircraft observations globally is presented in Heald et al. (2011), which showed no systematic bias in remote environments but an underestimate of median concentrations in polluted regions."

P8, l. 1-20: This discussion of the mean and SD of TGM concentrations underplays the spatial deviations shown in Fig. 3. For example, the modeled and measured N-S gradients in Europe appear to be in opposite directions.
We reorder the discussion in this paragraph to highlight the spatial correlation coefficient (now page 8, lines 31-35).

P8, l. 28-30: I suggest you cite Soerenson et al., EST 2013, or other observation-based papers to show that this mechanism is consistent with observations.
We revise this section in response to this and others' comments (page 9, lines 22 to 24): "Modeled oceanic Hg^0 evasion from the North Atlantic Ocean peaks in winter and early spring, because higher windspeeds increase mixing and entrainment of reducible Hg^{II} from subsurface waters and enhance the rate of air-sea exchange, and Hg removal via particle settling during this time is low (Sorensen et al., 2010, 2013)."

P9, l. 5-7: It sounds like you are suggesting that a similar analysis for the southern ocean is needed, but both the sea ice and the relatively large riverine flux relative to the Hg pool in the Arctic Ocean are not likely to have any relevance to these southern non-polar sites.

We clarify this section and include additional discussion (page 9, lines 35-37 to page 10, lines 1-3):
"Long-range transport of atmospheric Hg from Antarctica could influence observed seasonality at these sites (Angot et al., 2016). Capturing the seasonality of atmospheric Hg in the Arctic using GEOS-Chem required parameterization of unique sea-ice, oceanic, and riverine dynamics (Fisher et al., 2012; 2013; Y. Zhang et al., 2015) and a similar analysis for the Southern Ocean region, with its distinct productivity dynamics impacting Hg cycling (e.g., Cossa et al., 2011; Gionfriddo et al., 2016), has not yet been performed."

P10, l. 1-5: The figure indicates a -28% mean bias in North America, but in the Gulf of Mexico it appears to still be closer to -50%. Can you quantify the improvement in the wet deposition relative to Holmes et al. (or other models)? Consider adding a figure showing scatterplots of model vs. observations for both TGM and wet deposition.

We rewrite this paragraph (now page page 10, lines 34-36 to page 11, lines 1-2) to address your and other reviewers' comments:

"Previous GEOS-Chem simulations with Br-initiated oxidation of Hg^0 failed to capture this maximum because Hg^{II} production favored higher latitudes (Holmes et al., 2010; Amos et al., 2012). The inclusion of NO_2 and HO_2 as second-stage oxidants in our simulation shifts Hg^{II} production to lower latitudes and matches the general location of the Gulf of Mexico maximum although the magnitude is still underestimated. Increasing horizontal resolution in the model could improve the definition of the maximum (Zhang et al., 2012)."