The contribution of the very short lived brominated compounds to the stratospheric inorganic bromine load is an interesting and actively debated topic in atmospheric chemistry. The manuscript by Hossaini et al. titled "Evaluating global emission inventories of biogenic bromocarbons" uses currently available emission inventories of $CHBr_3$ and CH_2Br_2 (two most important brominated VSLS's) in a CTM (TOMCAT) and compare simulations with surface measurements from the NOAA global flask network and aircraft data from the recent HIPPO and SHIVA campaigns. This is a well-written paper that fits the scientific scope of ACP. The work presented in this paper leads to better constraint on the stratospheric Br_y load from VSLS emissions. The results suggest the number is in the lower end of previous range of estimates at about 4 ppt. My overall opinion about the manuscript is positive. There is one area where the manuscript falls short of my expectations, which I explain in the following paragraph. I would like to see some improvement on that front before it gets published in ACP, although the changes will likely be relatively minor.

Of the four available inventories evaluated in the manuscript, three of them are top-down. While the paper does a very good job pointing out the shortcomings of these top down inventories in matching the new data, it does not offer enough discussion on why the prior top downs all seem to overestimate $CHBr_3$ or CH_2Br_3 emissions or both. What went wrong with the previous attempts? It is stated in the manuscript that long-term ground based observations, which were lacking in the work that led to the existing top-down estimates, provide better constraints. Surely everyone would agree that more data with better temporal and spatial coverage would provide better budgetary constraints but agreement with aircraft data simulations are not very good either. Some justification is needed on why we should ignore differences on the modeling side, for example. On this note, some brief info on the modeling that went into creating the evaluated inventories should be provided in the manuscript. From what I read, TOMCAT was used in only one of three top-down inventories. Would the current TOMCAT simulations be better at reproducing the data sets used in developing the inventories? Equally relevant, or may be more important, would be a comparison on the data side. Is it possible to make direct comparisons between the measurements used in previous works and the data presented here. Could there be issues regarding temporal variability or may be systematic biases between data sets?

Some minor comments:

Pg. 12489, lns. 14-16: I don't fully understand what is meant by "surface" as it relates to lifetime and the phrase "a global/seasonal mean photolysis rate"? There is another slash on line 16. I think they can use "and" instead.

Pg. 12491, ln. 3: May be a reference needed after "TransCom-CH_4".

Pg. 12496, lns. 5-13: Seasonal cycle in the SH is also driven by photochemistry?

Pg. 12507, ln. 9: The end of the sentence, starting with "…, which…" is unnecessary (stated before). There is an "also" at the very end, probably leftover text from editing.