

Interactive comment on “Bromine speciation and partitioning in slab-derived fluids and melts: Implications for halogens recycling in subduction zones” by Marion Louvel et al.

Anonymous Referee #1

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General comments This manuscript presents two experimental studies by combining diamond anvil cells experiments with (1) in situ synchrotron X-Ray fluorescence analysis and (2) in situ XANES and EXAFS spectroscopies. The general aim of the study is to investigate the partitioning of Bromine between silicate melts and aqueous fluids and the solution mechanisms of bromine in such fluids. The subject of the study is relevant because it reports very important questions about the recycling of halogen elements in subduction zones (bromine and chlorine because bromine is considered to be a good analogue for chlorine). For that reason I believe that this study should be published but not in its present form, for the following reasons: The results about partitioning are equivalent to those of a previous published study by another group (nothing new) and

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the discussion about one low pressure result (at 2 GPa) is not convincing (see specific comments). The results about bromine speciation in high pressure fluids are new and they deserve to be published, unfortunately the partitioning of Br is not measured for the same chemical system (haplogranite, HPG) than its speciation ($\text{Na}_2\text{Si}_2\text{O}_5$, NS2), which makes any comparison difficult. Therefore I would recommend to delate the part about partitioning, or at least to provide convincing explanation (see specific comments), and to focus on the speciation results. Specific comments Br partition coefficients (D) are measured in situ for HPG system within the range 0.2 – 1.7 GPa and 592 – 840°C: they are ranging from 4.1 to 15.3, they fall in the same range than those from Bureau et al., 2010, for similar conditions (0.66 –1.7 GPa, 590-890°C, D from 2.18 to 9.2). However, if one plots all results in a diagram D versus pressure, data exhibits a lot of dispersion and no real relationship, as it should be expected (i.e. an increase of D with decompression due to degassing). This is not discussed at all. About the same value of D is obtained at 0.9, 0.8, 0.65, 0.2 GPa, respectively 4.4, 4.2, 4.1, and 4.8. High values of D are obtained at high investigated pressure (15 at 0.9 GPa and 9.7 at 1.7 GPa) where unity would be expected due to imminent total miscibility. Why? Such a discrepancy may be due to the pressure determination. The authors use the diffraction of gold and its equation of state. However, it is well known that gold diffraction is not a good tool to for low pressure determination, as an example see Heinz and Jeanloz, JAP, 1983, where the first measurements are performed at 4.42GPa. Furthermore, in a recent intercomparison of the use of EOS for pressure determination (including Au), Ye et al., JGR, 2017, it is conclude that at high temperature, accuracy cannot be better than 1 GPa, from 2.5 GPa up to 140. For that reason, the discussion from line 278 to 294 is not relevant and should be suppressed.

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