The manuscript by Suni et al. (2023) utilizes Vlasiator hybrid-Vlasov simulations to study foreshock compressive structures (FCSs) as a cause of downstream dynamic pressure enhancements ("magnetosheath jets"). This work builds on previous results by Suni et al. (2021), where 75 % of jets were found to be associated with FCSs crossing the shock. Here the authors focus on the last 25 % of jets, which they show can be divided into anti-sunward and flankward jets. The authors show that the anti-sunward jets are related to weaker foreshock compressive structures. The manuscript is an important contribution towards understanding magnetosheath jet formation. The manuscript is well-written and the methods are clearly described.

I have only minor comments/questions regarding the interpretation of the results and a few suggestions for improving the text. I believe the manuscript will be suitable for publication after minor revisions.

Main comments:

1. The authors refer to Suni et al. (2021) as "the previous study" multiple times in the abstract, so a citation should be included. I think the authors should clearly mention in the beginning that this study concerns jets that were not associated to FCSs in Suni et al. (2021). Currently, this information is dispersed around the abstract: "We focus on jets whose origins have not been clearly determined in a previous study using the same simulations" and "from those of jets found in a previous study, which were associated with foreshock structures of enhanced dynamic pressure and magnetic field".

2. The authors should discuss the nature of these foreshock compressive structures more in the manuscript. How do these FCSs move with respect to the solar wind flow? Do these structures appear locally or do they travel from far upstream? Which FCSs produce jets and which do not? I understand that answering some of these questions may require further analysis, and I do not think that is required. However, as the nature of FCSs is an integral part of the jet formation mechanism being suggested, it should be discussed more.
3. In Figure 5 it seems like in all of the panels and for all shock bow shock criteria, the shock is initially closer to the Earth than after the jet. Could this be a signature of a local corrugation in the shock? Or is it simply due to the bow shock standoff distance increasing as a function of time in the simulation?
4. It is not clear to me how the results agree with the exact mechanism suggested by Raptis et al. (2022), more so than the one hypothesized by Karlsson et al. (2015). In the simulations, are the FCSs reforming the shock and jets forming as solar wind is compressed between the old and the new shock or are the FCSs themselves crossing the shock and emerging downstream as jets? In the Discussion and Conclusions sections, the authors should be more specific about how their results support the mechanism suggested by Raptis et al. (2022).

Minor suggestions:

Line 2: "dynamics pressure" to "dynamic pressure".

Lines 52-54: This sentence is unclear to me. What was the definition deemed most appropriate for? If you mean that this definition was found to be the most appropriate one for the goal of capturing transient enhancements in dynamic pressure, I suggest rewording the sentence.