We thank the referees for their thorough and very helpful reviews. We have responded to each of the referee comments below. The original comment is preceded by “COMMENT:”, and our response by “RESPONSE:”.

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-- Reviewer #1 --

COMMENT: 1. My first comment, and probably the most serious, refers to the disregard of the wind direction data, as expressed in lines 102-104: "Since the wind direction data give constraints that are, in principle, redundant and less robust than the wind speed data, we do not include the direction data in our analysis." From my perspective, it may be not good, because only these wind direction data allow, in principle, to restore the precise encounter geometry, that is, the relationship between the direction of rotation (cyclonic or anticyclonic) of a vortex and the sense of its approaching the lander (from the left side or from the right side) if to look at the approaching vortex from the location of InSight. Also, I think there could be a possible confusion related to an authors' sentence "Thus, the total wind speed observed $ W(t) $ is the vector sum of the ambient wind and vortex wind, given by...", on one of the unnumbered lines between lines 144 and 145 on page 5. From my perspective, one should not speak of the wind speed (magnitude) but of the wind velocity vector in this context. In principle, since there is a given cliché (4) for the vortex wind, it might be better to say that the total observed wind velocity is the vector sum of the vortex advection velocity and the vortex wind velocity. It is not just a question of terminology, but is directly related to methodology of the authors, because if, for example, we consider a particular case $ \theta=0 $, then, in general terms, there will be three possible solutions of the equation (6), $ W=U+V, W=U-V, W=V-U $ (for simplicity, I use $ U\_1=U\_2=U $) depending on the geometry of the encounter (see above) and the relationship between the vortex wind speed $ V $ and the vortex advection speed $ U $, and it should be more clearly explained in the manuscript how to find a proper solution for $ W $.

COMMENT: 2. I have some concerns regarding formula (7). First, there is a misprint in it. The areal density of vortices $ n $ must be in (7) but not $ f $. This can be confirmed by dimensional analysis, since $ n $ has the dimension of km^(-2).

RESPONSE: The referee is correct: instead of $f$, the term on the far right-hand side should have involved $n$. We have corrected the equation in the manuscript.

COMMENT: Second, it is not explicitly stated in the text how exactly $ n $, determined from (7), is converted to $ f $.

RESPONSE: We have added another equation (Equation 8) and accompanying text to clarify how $f$ is calculated.

COMMENT: Third, looking at the expression for $ b\_max $ on a line below Eq. (7) I noticed the quantity $ \Delta P\_min $ under the radical sign. This quantity is not explained in the text and not used elsewhere in the manuscript.

RESPONSE: We have defined $\Delta P\_min$ where it first appears (beneath Equation 7 in the current manuscript).

COMMENT: A very similar formula with $ \Delta P\_min $ in it appears in Eq. (6) of the recent article (Kurgansky MV An estimate of convective vortex activity at the InSight landing site on Mars. Icarus 358 (2021)), where an equation analogous to (7) was used to estimate the areal density of InSight vortices based on data presented in Spiga et al. (2020), in eprint arXiv:2005.01134 publication before journal publication in Spiga et al. (2021). [This eprint publication was cited in Jackson et al. (2020); see, AAS Division of Planetary Science meeting #52, id. 308.03. Bulletin of the American Astronomical Society, Vol. 52, No. 6 e-id 2020n6i308p03.] I suppose this could be recognized in this manuscript, and a reference could be made to Kurgansky (2021) regarding Eq. (7).

RESPONSE: We apologize for having neglected this important previous study. We have added a reference to it both where we define P\_min and in our discussion of previous work (Section 4.2).

COMMENT: 3. It is better not to name the vortex model (4) as the Rankine vortex because this designation sticks to the Rankine combined vortex with the discontinuity of the radial gradient of the tangential velocity on the vortex core wall, but to name it the Vatistas vortex as Ralph Lorenz suggests in his publications.

RESPONSE: We have made this replacement.

COMMENT: 4. I agree with the authors that the most likely reason for the discrepancy between their inference about the relationship between the observed and actual pressure frequency distributions and the previous published results is due to filtering out distant encounters and low-signal vortices. I wonder how sensitive the authors' deduction is to a partial removing ('softening') of these rather rigid constraints.

RESPONSE: We explored several approaches to filtering out vortex encounters that gave spurious or questionable values for P\_act and D\_act. As described in Section 2.3, experimentation with synthetic datasets meant to replicate the observational data corroborated our chooses. More lax constraints resulted in unphysical inferred parameters for some vortices (unphysical negative P\_act values, for example).

COMMENT: I am also concerned about the $ D\_act\propto(\DeltaP\_act)^(-1/3) $ dependency, although the authors state that the results are also statistically consistent with no correlation as well. This dependency does not seem to be very physically justified, but if in fact there exists a dependency $ D\_act\propto(\DeltaP\_act)^(-x) $, with $ x>0 $, then it follows from the theory by Kurgansky (2019) that the actual differential distribution will be shallower than the observed distribution and the difference of the exponents will equal $ x $, that is 0.34 in this case. However, the difference obtained is 3.39-2.28=1.11>>0.34, which is worth some explanation. Taken together, I attribute these inconsistences not to the authors' determination of the differential pressure frequency distribution with the exponent 3.39, which is quite reasonable, but to their procedure for determining $ D\_act $ and $ \DeltaP\_act $ based essentially on equation (B6), which I have not seen before and which is brilliant per se, but its practical application may suffer from some intrinsic flaws, possibly related to difficulties in determining the values of $ V\_obs $.

RESPONSE: The referee’s point is well-taken. We have made the following replacement in the relevant text:

“A power-law fit to the distribution of $D\_{\rm act}$ vs.~$\Delta P\_{\rm act}$ gives $D\_{\rm act} \propto \Delta P\_{\rm act}^{-0.34}$, although the distribution is also statistically consistent with no correlation as well.”

was replaced with

“The distribution of $D\_{\rm act}$ vs.~$\Delta P\_{\rm act}$ is statistically consistent with no correlation; however, a strict power-law fit gives $D\_{\rm act} \propto \Delta P\_{\rm act}^{-0.34}$. This unexpected (and possibly unrealistic) power-law index may arise from small-number statistics or our admittedly conservative choice to filter out distant and low windspeed encounters, rather than a true anti-correlation between the two parameters.”

COMMENT: 5. A minor point: equation (B6) follows from the three equations: (B2), (B4) and (B5).

RESPONSE: Fixed