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.

RESPONSE: The referee’s point is well-taken, but the wind direction data have important limitations that diminish their usefulness to our study. In any case, determining the direction of rotation is not necessary for our study. We have made the following modification at the point in the manuscript indicated by the referee to address this issue:

“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.”

replaced with

“In fitting the vortex wind profiles, we do not consider the TWINS wind direction data. Although these data could help us to reconstruct the encounter geometries and determine cyclonicity (clockwise or counter-clockwise rotation), the precision of $22.5^\circ$ is insufficient to provide robust constraints (though the directional data is useful for studying other boundary layer processes). Moreover, our analysis requires only the magnitude of the vortex wind speeds, not the direction.”

COMMENT: 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.

RESPONSE: We have changed the sentence to “Thus, the total wind speed observed $W(t)$ \replaced{is}{involves} the vector sum of the ambient wind and vortex wind\replaced{,}{and is} given by”

COMMENT: 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 $.

RESPONSE: We recognize that we were not sufficiently clear about our wind profile-fitting procedure in our original manuscript. In fact, the $U$-values before and after the encounters are estimated separately from the $V$-values. We have modified the paragraph after Equation 6 as follows:

“We fit the pressure and wind speed profiles for each encounter in two separate steps -- first, the pressure, then the wind speed. In so doing, we hold the \replaced{$\Gamma\_{\rm obs}$-value}{$\Gamma\_{\rm obs}$- and $t\_0$-values} fixed from the pressure profile fit. \added{To fit the wind profiles, we estimate $U\_1$ and $U\_2$ by finding the median wind speed $W(t)$ between $3$ and $5\times\Gamma\_{\rm obs}$ before and after the encounter and then hold these values fixed as we fit $V$.}”

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 choices. 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. (Kurgansky (2019) discusses the expected relationship between $D\_{\rm act}$ and $\Delta P\_{\rm act}.$)”

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

RESPONSE: Fixed

-- Reviewer #2 –

COMMENT: Section 3 omits more than it contains despite seeming to be material to the conclusions. No dust devils were seen, like Banfield et al. [2020]. This is quite plausible, but to use the information one would need more information: Seen based on casual inspection? Seen based on some detection threshold? If so, what?

RESPONSE: We agree that the discussion of the image survey was not sufficiently detailed. We have substantially revised that section to include more details.

COMMENT: How much dust, how much contrast can be ruled out?

RESPONSE: We have included a new assessment of the upper limit on the allowed optical depth for any dust devils not detected in the images ($\tau < 0.1$) based on the distribution of pixel values.

COMMENT: This level of detail is given for vortices, and no information is presented for images. For example: At 3.9 km, one pixel is 7.8 m using the pixel size given. One cannot simply estimate a diameter of one pixel in size, or claim that the diameter is resolved at one pixel. The pixels have vertical extent, not just horizontal-how far away is 2 pixels below the horizon? The images have compression artifacts-how much does this limit the resolution? How much does it limit he detectable contrast? In short: when no dust devil is seen, what is ruled out? The observational and analysis detail seems to be the only reason for the section, and the section does not even approach an answer. Perhaps (as a null hypothesis) all vortices could have been dusty; because they appeared against a dusty surface/atmosphere at a distance, then maybe nothing could still have been seen in the lossy-compressed images. Without the detail that allows a reader to understand how much of that can be ruled out, what does the conclusion about dust-free vortices mean?

RESPONSE: We have clarified the dust devils ruled out by our survey in the added discussion: “To be clear, our null detection rules out dust devils with $\tau > 0.1$ and subtending angles smaller than $2\times10^{-3}\,{\rm rad}$ as seen by ICC. Dust devils occurring within the available images with both a greater $\tau$ and a significantly larger angular diameter likely would have been spotted.”

With regard to image compression effects, our newly described approach accounts for them since we used the distribution of observed pixel values in the ICC images. This point is included in the newly added discussion.

COMMENT: If it is true for that most vortices are dust free at InSight, what does it mean for other locations? There is comparative discussion at the end, but it was not obvious there was closure-is there less dust to lift around InSight? Or does the lack of dust tell us about Mars as a whole? The immediate environment must have some role, but the reader cannot discern how much.

I note that it was somewhat difficult to judge the discussion given that it depended on accept results that were not well demonstrated.

RESPONSE: We attempted to clarify this point by adding the following text to the first paragraph of Section 4.1:

Whether this result is representative of all martian vortices or reflects a dearth (or even a glut) of dust in the region surrounding InSight is unclear, but it appears roughly consistent with studies of terrestrial studies: deploying pressure loggers alongside solar sensors, (Jackson & Lorenz 2015) found that 40% of vortex events produced no solar attenuation, and only 20% of events caused dimming greater than about 2%. Studies on Mars have suggested martian vortices are very often dustless, especially when the boundary layer is shallow, which correlates with less vigorous vortices (Moores et al. 2015; Steakley & Murphy 2016).

We also added a summary sentence at the end of the second paragraph of the Conclusions section:

This result agrees with terrestrial field studies about how often vortices may loft visible dust (Jackson & Lorenz 2015).

COMMENT: 43-47: More complete references should be presented to avoid the impression that dust devil studies spun up in 2016- vortex time series were analyzed for the Pathfinder, Phoenix, and Curiosity at the time, not 'going back'.

RESPONSE: We thank the referee for pointing out this deficiency. We have added a more complete list of meteorological studies of martian dust devils to the Introduction.

COMMENT: 99-109: what is the distinction between data\_calibrated, modelevent, and model files? What makes one more suited?

RESPONSE: For the pressure time-series description (first paragraph, Section 2.1), we added the following sentence:

“These data files are different from the raw data files because they include a temperature-dependent calibration -- see \href{https://atmos.nmsu.edu/PDS/data/PDS4/InSight/ps\_bundle/document/pressure\_processing.pdf} for details.”

For the wind speed data, we modified the last sentence of the second paragraph to

‘A higher resolution dataset (20 Hz) is labeled as ``modelevent'' on PDS, but it is more limited in extent. So we opted to use the lower-resolution data. ``Derived'' data involve modeling out instrumental effects to achieve a (presumably) more accurate representation of the wind field; ``Calibrated'' data involve converting the raw instrument measurements to physical quantities. See \href{https://atmos.nmsu.edu/PDS/data/PDS4/InSight/twins\_bundle/document/twinsps\_dp\_sis\_issue10.pdf} for more details.’

COMMENT: (Somewhat significant) Section 2: How do you know what fraction of vortices of relevant pressure drop you detect? There is mention prior to this that sources of systematic error will be discussed. The method of searching is presented, but no discussion of how many vortices might be missed-until a brief note contrasting the result with the different Spiga et al. result, suggesting that non-Lorentz shapes might be a factor.

RESPONSE: We attempted to address this question in Appendix A by adding the following text:

“By design, our detection scheme will filter out some vortex signals. In particular, vortices with pressure signals very different from a Lorentzian will be missed. As an example, a Vatistas vortex that passes over the sensor in a non-linear trajectory would not generate a Lorentzian; however, such encounters seem to be unusual (Lorenz & Jackson 2015), so we do not consider them. So what about simple Lorentzians -- how many vortices of a given pressure drop is our approach likely to have missed? A simple way to address this question is to consider how often the pressure time-series were too noisy to have detected a vortex of a given $\Delta P\_{\rm obs}$. Figure 13 suggests that, for most of the vortices we consider, a threshold $F \* P >= 5$ requires $\log\_10 (\Delta P\_obs/\sigma\_P) >= -0.5$. For the vortex with the smallest $\Delta P\_ obs = 0.1 Pa$, this requirement translates to $\sigma\_P <= 0.3 Pa$. On sols with scatter larger than that threshold, we could not (in principle) have detected such vortices. Of the sols we analyzed, only about 18\% had such large scatter, meaning our approach likely missed few such vortices. For more typical vortices (the median $\Delta P\_obs = 1.1 Pa$), none of our roughly 400 sols had sufficiently high scatter that we would have failed to detect the vortex, suggesting a miss rate of less than 1 in 400 for vortex signals matching our detection criteria.”

COMMENT: (Very significant) 191-193: Figure 5 is said to show that gamma increases from 2 to 20. It does not. It shows two orders of magnitude of scatter with an arbitrary seeming trend line drawn through it. Is there a statistically significant trend? The comment about 5d is more plausible from the figure, but still of undemonstrated significance.

206-207: this decline in advection speed is said to correlate very closely with the increase in gamma. It does not. Gamma is purported to increase through the sol; the wind speed increases, then decreases (one could question the statistical significance, but it seems at least plausible). The decline in advection speed happens at the same time as some of the increase in gamma; 'correlates very closely' is a dramatic overstatement given that one is a linear trend and the other has a maximum. It seems that the 'gamma' curve is steeper at 9-11 than 11-14, so if I believed that curve, I could not believe this correlation.

(Very perplexing) On through 211: the physical explanation is unsatisfying. If gamma is increasing because winds are decreasing and duration scales inversely with speed: why does a 30% change produce an order of magnitude effect (from 2 to 20)? I believe the physics the paper is trying to describe-I am unconvinced the data illustrate the physics. If I believed the red lines in Figs 5-6, I would have to conclude that the vortex diameter increased by a factor of several through the day, which is the opposite of the stated conclusion. (As before: the stated conclusion is more reasonable, but the data do not obviously illustrate it).

RESPONSE: We thank the referee for a careful reading of the manuscript. Indeed, the increase in Gamma described in the original manuscript was incorrect, a holdover from a previous version of the analysis. The referee’s comment prompted us to re-analyze our results. We have replaced the original Figure 6 (which showed the advection velocities as a function of sol and time-of-day) with a new Figure 6 that shows advection velocity vs. vortex duration ($\Gamma\_obs$) and which shows a statistically significant (though weak) anti-correlation between duration and advection. We have also updated all the text associated with Figures 5 and 6.

COMMENT: Lines 278-279 assert vortices are frequently dustless: this is absolutely not demonstrated. It seems likely that it could be demonstrated, but the analysis that is presented fails to do the job.

RESPONSE: We hope that the updated discussion in Section 3 about our image survey helps to address this concern.

COMMENT: 280-281: Seeing no dust devils in 1000 images leads to an upper limit of 35% of vortices containing dust at the most vortex rich site. How are dust devils ever seen? This is a surprising conclusion that should be discussed more, that 1/3 of vortices might be dusty even given the proposed results.

RESPONSE: As we now note in Section 4.1, the result that no more than 35% of vortices are actually dusty seems consistent with the terrestrial field study reported in Lorenz & Jackson (2015), which found only about 40% of vortices detected via pressure excursion seemed to carry significant dust. So, although 35% might appear to be surprisingly large, it’s consistent with previous work and suggests that the region surrounding InSight is not particularly unusual with regard to dust devils.

COMMENT: [Maybe there should just be a note that the paper later disputes InSight as being more vortex rich than other sites; I believed its PR.]

RESPONSE: In the second to last paragraph before Section 5, we note explicitly that our results suggest vortex occurrence at InSight is similar to occurrence at other mission sites.

-- Statistical Review by AAS –

The suggestions made by the statistics editor are intriguing and worth exploring. However, given the rather large amount of effort required to use the suggested approaches and the uncertain return on that effort, we leave exploring those approaches for future work.

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In addition to the changes above, we made some small changes to the manuscript:

-We discovered that Figure 13 (b) was calculated using a different time-series filter than we meant to use. We have corrected that figure. The correction has no consequences for our results since we only used the results quantitatively, and the quantitative results were unchanged.