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# PSJ AAS31290: Reviewers Reports

1 message

**Edgard.rivera-valentin@aasjournals.org** <Edgard.rivera-valentin@aasjournals.org> Thu, May 20, 2021 at 5:18 PM

Reply-To: Edgard.rivera-valentin@aasjournals.org, peer.review@aas.org
To: bjackson@boisestate.edu
Cc: Edgard.rivera-valentin@aasjournals.org

Dear Prof. Jackson,

I have now received each of the reviewers' reports on your submission to PSJ, "Inferring Vortex and Dust Devil Statistics from InSight", and have appended them here. As you will see, each of the reviewers think that your manuscript is interesting and that it will merit publication once you have addressed the issues raised in the reports.

In light of the reviewer reports, I encourage you to submit a revised manuscript that fully addresses the points raised by the reviewers.

When you resubmit, please outline the revisions you have made in response to each of the reviewers comments using plain text in the field provided when you upload the revised manuscript. Citing each of the reviewers comments immediately followed by your response would be particularly helpful. Reviewers find it helpful if the changes in the text of the manuscript are easily distinguishable from the rest of the text. We ask you to highlight the changes in bold. The highlighting can be removed easily after the review process.

Click the link below to upload your revised manuscript;
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The AAS Journals have adopted a policy that manuscript files become inactive, and are considered to have been withdrawn six months after the most recent reviewer reports were sent to the authors. I hope to receive your manuscript before then.

If you have any questions, feel free to contact me.

Best regards,
Dr. Edgard G. Rivera-Valentín
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AAS - Planetary Science Journal
Scientific Editor
Lunar and Planetary Institute (USRA)
Staff Planetary Scientist

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Reviewer #1:
The manuscript addresses the inferences of the characteristics and statistics of convective vortices and dust devils made on the basis of the analysis of meteorological data obtained during the InSight mission. Interesting and potentially useful results are obtained that are critically compared with the two previous analyses of the InSight vortices by Spiga et al. (2021) and Lorenz et al. (2021). The previous analyses are expanded in that sense that the wind speed data are used in the current analysis, in addition to the pressure series data, which allowed the authors to estimate all the main parameters of the encountered vortices, including the maximum wind speed in a vortex flow, the maximum pressure drop at its center, as well as the diameter of the vortex and the minimum distance between the

center of the vortex and the InSight lander during the encounter (the so-called miss distance). In the second part of the manuscript, inferences are made regarding the specification of the threshold values that define the ability of vortices to lift dust and produce tracks.
Despite my overall positive impression of the manuscript, I have several critical comments that are mainly related to the first part of the manuscript.
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 $.
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). Second, it is not explicitly stated in the text how exactly $ n $, determined from (7), is converted to $ f $. 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. 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).
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.
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. 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 $.
5. A minor point: equation (B6) follows from the three equations: (B2), (B4) and (B5).

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Reviewer #2:
This paper discusses vortices observed by the InSight lander and contrasts the vortex statistics with the lack of detection of dust devils. The early sections are extremely well-written and well document: concise and complete, with enough detail to follow the method plus two appendices for the curious. The transition to image analysis is quite jarring: it is perhaps 1/8 the length of the vortex section before considering appendices and omits much information that seems important. Given that the fundamental results of the paper depend on the information from images of lack

of dust lifting, one would expect a similar level of detail about the image analysis as about the pressure time series analysis.

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

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.

Some specific comments, by default minor, follow.

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'.

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

(Somewhat significant) Section 2: How do you know what fraction of vortices 9of 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.

(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).

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.

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. [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.]

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Statistical Review by AAS

In sec 2.2 and Appendix A, the authors describe a multi-stage procedure for detecting vortex encounters in the Martian atmospheric time series data (e.g. Fig 1). (1) The series is detrended by subtracting a boxcar smoother, (2) an arbitrary threshold is applied to the residuals, and (3) signal is characterized by regression with with chosen Laplacian functional shape (eqn 1). Fig 13 shows there is no obvious optimal choice for the boxcar width, and Fig 1b shows its deficiencies in the illustrated case.

This procedure is clearly adequate for strong vortices as in Fig 1a, but other methods of time series analysis may be more sensitive to smaller or more distant vortices. Fig 1b shows that the boxcar smoother does not remove many uninteresting temporal features. I can suggest two approaches:

a) Wavelet analysis. Here the Laplacian (or a standard Mexican Hat) can be used as the wavelet basis, and a wavelet transform applied. The wide-scale wavelets will map the uninteresting pressure trends while the narrow-scale wavelets will reveal the rapid symmetrical variation characteristic of vortices. I suspect that a specialized wavelet denoising step (setting all weak wavelet coefficients to zero and subtracting the strong wide-scale wavelets from the original time series) would give a clearer version of Fig 1b and reveal fainter vortex events. Well-developed wavelet code packages are available in the R, Matlab and Python software environments. See volumes like:
- Wavelet Methods for Time Series Analysis, Percival & Walden 2000
- Wavelet Methods in Statistics with R, Nason, 2008

b) ARIMA modeling. Maximum likelihood estimation of linear autoregressive models have been the dominant tool for time series analysis since the 1970s (books below). The AR and MA components treat short-memory responses of the system, while the I component is the differencing operator (the narrowest boxcar filter). ARIMA modeling has a powerful suite of methods for model complexity selection (Akaike Information Criterion) and model goodness-of-fit tests of residuals (Anderson-Darling normality test, Ljung-Box autocorrelation test, augmented Dickey-Fuller test for stationarity, etc). There are many extensions for nonlinear behaviors including ARFIMA (with 1/f^alpha long-memory behavior) and GARCH (with stochastic variations in variance; earned a Nobel Prize in Economics).

ARIMA-type models are not only effective in modeling an amazing variety of time series such as Fig 1a, but have the particular characteristic of giving a poor model for sudden jumps. (The data might be binned slightly so the vortex jumps are nearly instantaneous.) The vortex events should thus appear as sudden double-spikes in otherwise (near-)Gaussian white noise in the ARIMA residuals. This method has proved effective in detecting faint dips from transiting exoplanets in stellar photometry.

ARIMA & related codes are widely available, particularly in R and Matlab. I recommend the 'auto.arima' function in Hyndman's famous 'forecast' CRAN package in R. See:
- Chatfield & Xing 2019, The Analysis of Time Series: An Introduction with R (7th ed, elementary)
- Box, Jenkins et al., 2015, Time Series Analysis: Forecasting and Control (5th ed, authoritative)
- Hyndman & Athanasopoulos, Forecasting: Principles and Practice, 2018 (2nd ed, https://otexts.org/fpp2 with forecast package cookbook)
- 2018FrP.....6...80F and 2019AJ....158...57C (autoregressive modeling of astronomical time series)