Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1206-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessing the contribution of ENSO and MJO to Australian dust activity based on satellite and ground-based observations" *by* Yan Yu and Paul Ginoux

Anonymous Referee #1

Received and published: 30 December 2020

Overall comments

This paper first compares several satellite dust products in Australia against in-situ observations, and then looks at MJO and ENSO - driven variations in dust optical depth. It seems that detailed comparisons of this type have not previously been done. The ENSO-driven dust signatures confirm expectations from previous work, but the MJO ones are novel and look a bit hard to explain (at least to me).

I think this is a useful study but with a number of problems that I think will require major revisions to address.

1. Many aspects of the data and analysis are confusingly, incompletely or misleadingly described (see examples under Detailed Comments). These problems need to be corrected.

2. The authors have not considered serial autocorrelation in their data and therefore may have overestimated the statistical significance of relationships. How likely this is to be a problem depends on what time averaging interval they used for doing the regression tests (which they don't say). They need to check this.

3. I find the authors too ready to declare success in their satellite data evaluation. In some respects, particularly the climatology and to some extent the seasonal cycles, the satellite and in-situ datasets agree very well. But in terms of day to day (I think) variations they don't look very good. Moreover some comparisons show MISR very different from MODIS (in particular, they look totally different in Fig. 7). And I'm not sure about the MJO or ENSO signatures, which are a better test (day to day variations will be noisy even if the instruments are working well, and climatology is perhaps too easy). The ENSO comparisons (Fig. 8) don't seem to agree well in DJF, with high DSI signatures at several sites where the satellite shows little, and a swath of near-zero DSI signatures in the southeast where the satellite shows dusty conditions. They look better in SON but there is less signal to work with. And the MJO patterns (Fig. 10) seem to be significantly different in quite a few areas between in-situ and satellite, though it is very hard to see because the in-situ points are so small. I think this needs to be better explored—we would expect all of these platforms to deliver similar results on this, wouldn't we? By the time the authors got to the ENSO and MJO signatures they consolidated to just one satellite dataset since up to then they'd agreed well enough, but I think given the seeming disagreement with in-situ on these signals, the authors need to back up and include the separate satellite products in this analysis.They also need to see if they can explain the satellite-in situ discrepancies on the basis of poor sampling in the in-situ data—they seem to perhaps have been too lenient in

including short time series that will give unstable results.

4. In addition the MJO patterns, to my eye, really don't support the claims by the authors that wind speed and rain explain the dust variations. The ENSO patterns could be explained by either wind or rain, but the MJO ones seem to be explained by neither. This is surprising and a bit discouraging but needs to be conveyed clearly by the authors. I wonder if data quality could be an issue?

5. Even if we ignore the serial correlation issue and accept the significance results shown, the authors have not demonstrated that the interactions reported between ENSO and MJO are statistically significant and I doubt that they are. In Table 2 (last four columns) the authors consider only the null hypothesis of $r = 0$ for each phase of MJO. This simply establishes whether a relationship exists. But to establish that this relationship is modulated by MJO (or vice versa), the relevant null hypothesis is that r is *invariant with the phase* of MJO, not that it is zero. Wherever a significant r is reported in the table during at least one MJO phase, there are generally also strong r for the other phases as well. The fact that r exceeds an arbitrary significance threshold during one phase of MJO and not another is NOT a legitimate demonstration of any real change. Likewise, the maps in Fig. 12 don't look that different to me, and the variations among the MJO phases are probably well within statistical sampling uncertainty. Finally the authors don't offer any convincing hypothesis to explain the interactions claimed. They should either do the correct tests to confirm this interaction is real, or remove it from the paper.

Detailed comments

Entire manuscript: there are numerous minor grammatical errors that should be fixed by having the manuscript copy-edited. If the journal doesn't do this then the authors should find a way to have it done.

38: this statement is an exaggeration—I doubt dust from one continent is the only thing controlling biological productivity in the Southern Ocean, and it is surely not the only thing (or even a dominant thing) controlling carbon uptake (compared to, say, the ocean circulation).

52: It would be helpful to mention what the difference was (is there some region that the weather-station studies identified as a dust source that was not identified by the others?)

95: this raises a point not yet mentioned, which is what aspects of precipitation do we expect to influence dust? Do extremes matter (I would think not), or is the most important aspect the time lag between rain events (what I would expect)? Are there studies linking these aspects to MJO or ENSO?

Section 2.1: Please say what the time resolution is of the datasets (monthly? Daily?) It is stated under Aeronet that you average monthly data to get annual means but that's the first we've heard of any time resolution and surely your analysis is not all based on annual means? I didn't find any statement of this until the caption of Fig. 11.

103-4: please write out DOD (I assume it is dust optical depth but you never say). I don't think column-integrated extinction is correct (the extinction will be 1–exp(–DOD) right?)

116: can you explain a bit more about how this estimate works? It seems to depend on dust being a different size from non-dust aerosol. From later text I gather this is actually a coarse-mode AOD—tell us!

132: I don't understand this, it does not seem consistent. The AOD in Section 2.1.1 is the MODIS total AOD, no? And the MISR nonspherical AOD is meant to be an estimate of the DOD (this is what you say in the previous sentence)? I am guessing that (1) is designed to give a coarse-mode AOD (which you call there the DOD)? Please explain

this better. And I think it would be clearer to define a (total) AOD; a coarse-mode AOD (perhaps “cmAOD”), which you get from MODIS via (1) and from MISR as a product; and a non-spherical AOD (nsAOD) which you get from MISR. The latter two can be taken as approximations to the true DOD and tested as such.

141: Here again it would be better, rather than confusingly renaming products, to “call a spade a spade” and refer to this as the Aeronet cmAOD. Especially since later (line 210) you acknowledge that it is measuring sea salt in coastal areas (not just dust).

164-174: This random sample re-ordering test will not account for serial autocorrelation in the data, because any that is present will be destroyed in the scrambled synthetic data series. Please check the autocorrelation time scale of the two time series—if at least one of them decorrelates within a couple of time steps you are OK. Otherwise one way to deal with this is to randomly shift, rather than scramble, one of the time series relative to the other, which will preserve any serial autocorrelation. It will limit the number of distinct synthetic samples you can generate but you should have enough. Also, as noted before please indicate what the time resolution is at which you are doing the resampling.

Figure 1: I found it confusing to have different regions shown in panels (a) and (b) of Fig. 1, suggesting some difference in what Terra and Aqua MODIS are seeing. In fact the two appear essentially identical, but this is obscured by the different labels. I suggest maybe generating a separate figure that is just a map showing and identifying all the regions, and then maybe reproducing some of the ellipses from that map (with no labels) in each of the other panels for reference to help match dust blobs to geographic regions. Apart from that, what do unfilled circles mean in the Aeronet data? Do these mean values of less than 2?

Table 2: You don’t say what the numbers in the third column mean (I assume p-value), nor what the units are for the “Reg” quantities. The Reg values are very hard to interpret

since they depend on the amplitude of the Nino 3.4 index; it might be more useful to show correlation coefficients.

212-222: Please explain how you get the seasonal cycle peak month. Do you (I hope) fit a sinusoid to the monthly means? Pick the highest month (I hope not)? If there are two similar peaks in different months for example, the latter method could produce unstable results and seemingly large discrepancies may not be statistically or physically significant. I am worried by the fact that the Terra and Aqua datasets sometimes show rather different peak months even though in Fig. 1 they look indistinguishableâĂŤthis could be because of an unstable method of identifying the peak month, or the lack of a strong seasonal cycle in either dataset (if the seasonal cycle amplitude does not exceed some threshold I’d suggest blanking out any measure of its phase).

217: Surely with only 1 1/3 years of data you should not try to compute a seasonal cycle?!? Please set a minimum number of years and tell us what that is.

Figure 3: First, please use a larger font, this is barely readable even if I zoom in. Second, please identify which quantities from MISR is being plotted (and, why not show both of them?)

225-235: Doesn’t Aeronet give a point measurement, which may be a noisy thing to compare to a large satellite footprint? I see no evidence, at least in Fig. 4a and c, that the satellites are saturating systematically at high DOD values. Instead it just looks like the measurements are noisyâĂŤthere is a lot of scatter at all DOD values and the correlation is rather low. When binned according to one of the two variables (implicitly assuming that one is ‘truth’) this will always lead to biases at the high and low end as shown in panels (d-f) even if there are no actual biases, because random errors in the bin variable are causing aliasing via systematic binning errors. How are your results affected if you do more temporal averaging of the data before computing the regression? It will likely improve.

Figure 8: Please clarify whether dust is leading or lagging ENSO. Also please clarify what lag is shown in the maps (I assume lag zero but it needs to say). Finally, it is confusing to have the y-axis located at a lag of six, I would expect it to be at zero. I expect people will misread this and think the leftmost bar is the lag-zero one.

275: This story does not seem to match what is in the figures. The wind speeds are indeed higher in MJO 5-6 (Fig. 11), but the dust is no higher than during the other phases (Fig. 10). Moreover the pattern of winds over the four MJO phases if anything seems opposite to that of dust, with the highest dust anomalies (western region during MJO1-2 in particular) coinciding with below-average winds. On the other hand, the ENSO signals (Figs. 8-9) do look as expected.

Figure 9: There is no color bar for panels i,k.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1206, 2020.