This study documents some of the first radar observations of a nocturnal tornado event in the austral winter season in the State of São Paulo in Brazil. It gives a meteorological overview of storm, complete with synoptic discussion, overview of observations from several radars and a ground-based lightning detection network, and ground survey of tornadic impacts. While the event is noteworthy and publication is appropriate, the manuscript would benefit from efforts to sharpen the analysis, clarify the meaning of results in the context of the broader literature, and refine the logical support used in several arguments to motivate the study and interpret results. I recommend that the authors address these areas with additional analysis and major revision to the manuscript prior to publication. Related comments and specific suggestions are provided below.

Comments on broader issues:

1. Throughout, the authors have omitted a number of important foundational studies directly related to the topics of interest here. Specific examples include but are not limited to extensive background on the definition supercell thunderstorms (i.e., they are defined and set apart from other intense isolated convection as possessing a characteristic mesocyclone, e.g., Lemon and Doswell, 1979), deeper research into the operational implementation of the Enhanced Fujita scale in the United States with which several key comparisons are made, and considerable background in the area of storm electrification through the non-inductive charging mechanism (e.g., Takahashi 1978, Reynolds et al. 1957, Saunders et al. 2006, among others), physical relationships between lightning and convective intensity, and the lightning jump (e.g., Gatlin and Goodman 2010; Schultz et al. 2009, 2015, 2017). Additional notes are included below in specific comments but these should be considered as broad, substantial topics requiring further support from the literature.
2. A separate but related issue is adequate referencing of instruments and techniques at the first mention. For instance, BrasilDAT isn't thoroughly discussed until section 3.5 but it is mentioned earlier in the paper. Please add references for the instrument at the time it is introduced. Similarly, TITAN is introduced early on without documentation. Additionally, the concept of a lightning jump is described quite a bit in advance of the first citations of the foundational studies behind the concept.
3. I can appreciate the difficulty in thoroughly addressing a storm with limited radar resources, particularly when certain radar fields and variables are either unavailable or unusable because of errors. However, a good deal of the argument and discussion herein refers to "supercell" thunderstorms without proving, or at least convincingly supporting, the presence of a mesocyclone with quantitative rotation speed characteristics (e.g., Lemon and Doswell 1979, Brandes 1984, Moller et al. 1994). For instance, how did the authors infer that the storm discussed in line 264 became a supercell without Doppler velocity data (line 275?). Radar reflectivity characteristics can support the possibility that the storm was a supercell but I caution the authors against overextending the available data. This can still be considered a valuable case study by evaluating the radar reflectivity and lightning parameters but there is no benefit gained from approximating supercell characteristics or onset of supercell development without thorough analysis of Doppler velocity data.
4. I find that aspects of the lightning analysis need to be addressed more carefully prior to publication, discussed further below.
    a. For instance, CG flashes are made up of a number of return strokes that are often reported separately in "stroke-level" CG data. If the term "stroke" is used synonymously with

"flash" herein, please be careful to clearly specify that you are not referring to the numerous return strokes that make up each individual flash.

b. The lightning jump began as a concept in the 1980s that has since been fit with an operational, quantifiable definition. Recent work by Schultz et al. between 2009 and 2017 have clarified a quantitative definition based on the change of flash rate over time (delta flash rate/delta time) that can be easily applied to total lightning data. The application of the concept of "lightning jump" seems to have been applied quite loosely here and would benefit from more thorough quantitative treatment without too much effort given the availability of the total lightning data. Moreover, it appears as though in Fig. 12 that best documents lightning flashes associated with the case, there were actually several jumps as defined through the change in flash rate over time, near 0105 UT, 0220 UT, as well as 0310 UT. It's quite possible that the greatest jump occurred near 0314 UT as discussed in the paper, but this really should be quantified and discussed in the proper context.

c. Related, substantial work has been done to address the physical meaning of lightning jumps in the context of severe weather (e.g., discussion in lines 470-474), including tornadoes (e.g., Schultz et al. 2015, 2017) and even with respect to supercell mesocyclone development and intensification (e.g., Stough et al. 2017). The results from this study are quite consistent with and could be connected better with those in the literature, ultimately bolstering the main conclusions of this paper as they are drawn throughout Section 3.5.

Specific comments:

1. Lines 15, (lines 103-104 as well): Could you please elaborate on your reasoning for limited fatalities during this nocturnal event? Typically, nocturnal tornadoes have been linked with greater human casualties (e.g., Ashley 2007, Kis and Straka 2010).
2. Line 57: The statement that no link has been found between climate change and tornado frequency is misleading at best. Please either find references that support this assertion or refer to studies such as Brooks 2013 to revise and remedy.
3. Lines 58-62: This is admittedly a complex topic to address briefly, but should at minimum, include a few additional ideas such as (a) impacts of microphysical modification from aerosols on buoyancy and (b) competition for water vapor that inhibits drop growth and deep convection.
4. Line 195: Are there any reports or news articles that can be referenced for these figures?
5. Figure 4: This is a nice overview. Could you please somehow annotate the storms of interest to help the reader with context?
6. Lines 277-279: Did the storms truly dissipate (verified through another radar to the NE of SRO) or is it possible that the distance from SRO was causing the storms to be sampled at high altitudes, apparently diminishing their reflectivity characteristics?
7. It would be a great help to include the reference radar in each of the figure captions where radar markers aren't visible in any panel (e.g., Fig. 7).
8. Lines 341-347: Especially for non-expert readers, please explain why these radar limitations may have resulted in underestimates of the quantities of interest.
9. Line 363: Please explain how probability of hail and Foote Krauss category quantities calculated, even just briefly if the authors elect to refer back to TITAN documentation.
10. Section 3.4: The overview and figures of damage here are a nice addition to the paper. However, there are some fundamental flaws in the reasoning and logic used in evaluating the damage in the context of the EF scale and comparisons with United States structures. I think the authors'

EF scale assessment is in the right neighborhood but I disagree with claims that comparisons are difficult between countries because of construction or lack of parallels in the EF scale. Please see comments for more detail.

a. First and foremost, the EF scale has a number of damage indicators (DIs) used regularly in operations. It's true that it's difficult to rate the strongest tornadoes in areas where vegetation is limited or homes are of poor construction quality. Simply put, there aren't enough indicators that meet the criteria of withstanding the strongest winds. However, this is not a limitation of the EF scale and it should apply quite readily to other regions. I would encourage the authors to search the literature for applications of the damage scale in rating tornadoes (e.g., Burgess et al. 2014) and/or to reach out to the US National Weather Service to learn more.
b. Second, please find a reputable reference to support claims on structural composition and integrity such as those in line 371. Census data may be a good place to start, there may also be some other published studies on satellite remote sensing to get a better sense of the geographic distribution of structures. Even in the US, the type and quality of home construction can vary quite a bit by region and it would be best to avoid overgeneralizing. Brick and concrete structures can be extensive in parts of the US, but anecdotally, concrete block structures don't withstand convective winds as well as a solid frame home with brick veneer. I'm also unsure of an earlier claim that motor homes (typically thought of as recreational vehicles) are common and would need a reference to back this up as well (line 47 in the abstract).

11. Lines 404-405: How is optimal coverage defined here? Spatial extent, detection efficiency? Please elaborate.
12. Line 406: Have the authors completed a sensitivity analysis of the reflectivity threshold used to properly associated lighting flash counts with the cell of interest? It can be difficult to identify a repeatable, objective method for flash counting. However, anecdotally, restricting analysis to 30 dBZ often results in undercounting flashes and can result in an artificially low flash rate.
13. Line 440: Please note that IC and total lightning flash rates in excess of 110 min^{-1} are not uncommon in deep convection, particularly in supercells. Suggest removing this reference or adding others that discuss supercell flash rates more extensively.
14. Figure 12 and related discussion: How much of the variability in flash rate and the qualitative jumps can be attributed to cell mergers? Similarly, can any of the decreases in flash rate be associated with cell splits? Please discuss any efforts to control for this effect on the data briefly.
15. Line 469: Please reference a few specific related case studies.
16. Line 550: Could the authors please explain a little more about these difficulties? What tools are recommended generally to remedy the issues discussed throughout this section?

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