Reply to Referee 2

We thank the Referee for his appreciation of our work and his constructive and very useful comments. We think that the changes to the manuscript and the underlying dataset have improved the manuscript's quality and will meet the Referee's agreement.

Please find the Referee's comments (COM), our reply (REP), and the according underlined changes (CHA) to the manuscript below:

1) COM: *TC surface wind has wide variability. For practical purpose, one often approximates the surface wind as a mean radial profile, plus an additional left-to-right motion-induced asymmetry, like Holland08. The limitation of such approximates on describing the full range of surface wind variable (i.e., the asymmetrical wind) is discussed in the manuscript. Additionally, it is worth to note that a recent study (Uhlhorn 2013) using aircraft observations found that the magnitudes of the motion-induced asymmetries at the surface do not necessary increase proportionally with the translation speed, as what is assumed conventionally (and in the manuscript). Another comment is that the wind damage primary from wind gust. Holland08, and other existing parametric wind models, despite being practical, are not able to accurate depict the probability of the wind gust. I understand that Holland08 is a somewhat standard model, but as a reader, I would still suggest to show its equations here.*

   REP: We agree that our treatment of surface wind asymmetry is fairly simple and can be improved in future work. We added a more detailed discussion to this specific section (P6, L2). We also agree that wind gusts are an important factor for TC damage and that sustained maximum wind speed is only a proxy for maximum gust speeds. However, modeling realistic wind gusts at the current resolution of 0.1° is problematic and also beyond the scope of this current work where TC damage is not in the focus. We added a sentence discussing the importance of gusts to the manuscript (P2, L14). Similarly, we added Holland's equation to the manuscript (P5, L21).

   CHA: <u>Although this attenuation factor can be thought to resemble surface friction effects, we neither explicitly account for surface friction and the resulting reduction and rotation in the translational speed's magnitude and direction, respectively [*Lin and Chavas*, 2012], nor do we incorporate that the magnitudes of the motion-induced asymmetries at the surface do not necessarily increase proportionally with the translation speed [*Uhlhorn et al.*, 2014].</u>

   <u>Additional impact categories to quantify exposure could account for duration or gustiness of strong winds, TC-related precipitation, and or storm surges.</u>

2) *COM: L25 at Page 6: You mentioned that there is an underreporting in IBTrACs for earlier period. I agree, but this is usually for non-landfall TCs. Storms that made landfall should be reported even without satellite measurement.*

   REP: We agree that non-landfalling events should be more affected by underreporting than landfalling storms. However, there is a significant drop in number of landfalls before 1980 for the Northern and Southern Indian Ocean and partially also for the Southern Pacific (Figure 3) that seems to relate to some sort of reporting bias, intermixture with monsoonal depressions, and

other effects that we are not aware of. We therefore kept our cautionary note with slight modifications (P7, L21).

CHA: When using TCE-DAT to analyze trends in TC risk (see Fig. 3), one should be aware of potential underreporting in IBTrACS for earlier periods that might even affect landfalling TCs and can be one reason for trends.

3) *COM: L20, page 9: I will make it clear that the differences in the exposure measures is due to the differences in the wind estimates from Holland08 and HURDAText. The spatial (geographic) and temporal distributions of population and the GDP are unchanged here. In other words, you are not evaluating the exposure measures, but their sensitivity to the surface wind estimates.*

REP: For clarification we added the following sentence to the manuscript (P10, L21).

CHA: This indicates the sensitivity of exposure measures to different surface wind estimates by Holland08 and HURDAText.

4) COM: *Data: The current data is the exposure per storm per country, which is great. However, I think gridded data will further extend the utility of this global-consistent TC exposure dataset. Especially, the current spread sheet is calculated from gridded data (this is how I understood). The data will be large, though.*
Another comment on the data is that the authors should also provide GDP and population estimations as well. The current data contains only probabilities of the wind and the final exposure measures. As I mentioned in my general comments, I appreciate this study not only the final product, but also the approach. Release the GDP and population data will enable non-expert (non-economist) to create another globally consistent exposure measure with different parametric wind models.

REP: We agree with both Referees that spatially-explicit exposure data could further enhance the usability of TCE-DAT. We therefore added spatially-explicit TC exposure data for both historically-consistent and time-independent socio-economic exposure at the event level available at http://doi.org/10.5880/pik.2017.008. All TCE-DAT data sets are additionally summarized by a new data collection DOI at http://doi.org/10.5880/pik.2017.011.
We further created another data archive that contains the spatially-explicit GDP data plus GDP-assets conversion factors used for this study, see http://doi.org/10.5880/pik.2017.007. The population data is already freely available at http://themasites.pbl.nl/tridion/en/themasites/hyde/download/index-2.html. We now highlight this fact more clearly in the manuscript.

CHA: We added the information about new data sources and data availability to the manuscript at numerous instances; please see the new manuscript with tracked changes for details.