Author response to reviewer and public comments for Brief Communication: Differences between Sundowner and Santa Ana wind regimes in the Santa Ynez Mountains, California" by Benjamin J. Hatchett et al.

Responses to reviewer comments are given in **bold**
New or changed text is given in *italics* (***bold italics*** for emphasis where noted)

Interactive Comments from Anonymous Reviewer #2

1. The authors propose a simple method for the detection of Sundowner events from surface temperature observations. From the chosen events, the authors build a climatology for the Sundowner winds. This climatology is compared against a pre-existing Santa Ana index. The paper is well written but presents a very basic analysis. The three dimensional dynamics of the phenomena is missing and can be performed using reanalysis data without the need for further downscaling. The authors fail to provide a clear physical and dynamical description of the differences between the two phenomena. This leads me to not recommend the publication.

We appreciate the reviewer taking the time to evaluate our paper and provide constructive suggestions for improvement.

Our analysis is admittedly simple. In this case, two simple indices (one for Santa Ana winds and one for Sundowners) show markedly different synoptic setups, which has not been previously shown. We also provide a climatology of Sundowners and compare it to the climatology of Santa Anas, which also has not been previously shown as prior Sundowner work has all been focused on case studies (and is mentioned in the original text).

We are in the process now of making a concerted effort to improve the dynamical explanation of the differences between the phenomena. However, with a brief communication-type article whose primary goal is to provide a large scale difference between these two wind regimes, a detailed explanation of each phenomena are beyond the scope of this paper. Such descriptions can be found in previous work for Santa Anas (see references within the paper) and in the case of the Sundowner, we noted in the original manuscript that a more detailed dynamical explanation from a high-resolution numerical modeling study is the subject of ongoing research (Smith et al. in revision for Journal of Applied Meteorology and Climatology). Our target audience is the natural hazards community and not the dynamical meteorology and mountain wave community, so we wanted to be careful to provide information that fire managers, operational forecasters, and landscape ecologists could easily interpret and utilize in their own research and operations regarding fire weather and subsequent impacts of fire (e.g., historical and future vegetation patterns and post-fire debris flows) in this region.

Again, our primary goal with this paper was to use a simple, or basic, method to differentiate these two important downslope windstorm phenomena in Southern California in terms of

seasonality and synoptic structure, as written in the original final paragraph of the introduction: "We hypothesize that Sundowner events are seasonally distinct from SAWs and have differing synoptic scale patterns associated with them.**"**

In light of the reviewer's concerns, we will revise the text to be more explicit in pointing out our primary objectives with this manuscript and will add some additional dynamical descriptions of Sundowner winds while remaining within the length constraints of a brief communication-style article.

2. The Sundowner winds are downslope wind storms and the dynamics of such winds has been described in the literature since early 1950's (e.g. Scorrer 1955; Clark et al. 1977; Klemp and Lilly 1975; Smith1979, 1985; Smith et al. 1993; Durran 1986, 1990; Vosper 2004; Grubisic and Billings 2007, 2008; Jiang and Doyle 2008; Doyle et al. 2011). There are several examples, in the literature, of flow characteristics and approximations which allow the description of the dynamics of such phenomena even with low resolutions such as reanalysis. The differences in the dynamics, upwind characteristic of the flow and boundary layer differences between the Sundowner and Santa Ana are missing from the manuscript and should be provided

We appreciate the reviewer's suggestion to further evaluate the differences and we are now in the process of performing additional analysis using NARR (32 km horizontal resolution) and the available rawinsonde data from Vandenberg (KVBG) to examine upstream composites and perform several standard calculations relating to downslope windstorms (e.g., Brunt-Vaisala frequency, Scorer parameter, and temperature profiles; please see also the response below). We appreciate their providing us with a nice compendium of downslope windstorm references, however given the length limitations for number of references in the NHESS guide to authors for brief communications, we were only able to add the most comprehensive of these (if the reviewer has a special request or two, we have no issue with a substitution). It should be noted that this paper is not intended as a comprehensive literature review on downslope windstorms due to its short format and we did include the key relevant southern California downslope windstorm papers in the original manuscript.

We will upload our findings as a follow-up comment upon completion of this analysis.

3. The manuscript also, does not provide any analysis of the atmosphere's vertical profile. Although this analysis may be difficult with the NECP reanalysis if model levels are not available, this would not be the case with the Japanese 55-year Reanalysis (JRA-55) or the Modern Era Retrospective-analysis for Research and Applications (MERRA2) which have similar horizontal resolutions to NCEP with 60 and 72 model levels respectively. Both are freely available for research. The analysis of the atmosphere's vertical structure would allow a better understanding of the phenomena and provide clues to the differences between Sundowner and Santa Ana winds. This should be added.

This is a valuable suggestion. So as not to make this brief communication paper unwieldy with numerous additional reanalysis products (as we already are using two, NARR and NCEP/NCAR), we have chosen to evaluate vertical profiles using output from the NARR and 12 hourly observations from the Vandenberg rawinsondes (we will upload our revised map in Figure 1 to show the KVBG launch location and cross section profiles A-A'). We are now calculating Froude Numbers, the Scorer Parameter, Brunt-Vaisala frequencies, and will include a figure showing the vertical profile of winds and temperatures. We are also producing cross sections orthogonal to the Santa Ynez to highlight the differences in vertical winds and potential temperatures between the two wind regimes. The relevant findings from these results will be discussed in the revised text and included in the supplementary material as figures S3 (vertical profiles from observations) and S4 (composite cross sections).

Again, we want to re-iterate that the purpose of this paper was to show that large scale synoptic patterns between two fire weather regimes are different and not to perform a comprehensive dynamical analysis of the regimes. That work is part of a much longer and dynamically comprehensive paper currently undergoing revision (Smith et al. in revision for Journal of Applied Meteorology and Climatology). If the reviewer would like to contact us directly to discuss the findings of Smith et al., we encourage them to do so as it appears they would find this paper to be of interest. The goal of the current short communication paper is to quickly communicate the broad differences between these fire weather regimes to a variety of science and natural resource management communities as well as the general public.

Minor Comments:

4. A description of the SAW index should be more elaborate, so that the reader does not have to interrupt the reading of this paper and review Guzman-Morales et al. (2016) in order to understand the applied methodology.

We agree that this is a useful suggestion (please also see the Interactive Comment from Clive Dorman), and we will add additional text so as to help the reader understand the methods employed by Guzman-Morales et al. (2016) without requiring an interruption from reading the current paper. This text will be uploaded as a follow-up comment upon completion of its revision.

5. There are several time periods referred in the text: 1979-2014, 1981-2010, 1997-2014. Figures 1, 2a, 2b and 3 should be for the same time period, either 1979-2014 or 1981-2010.

We understand the reviewer's concern, as there are many time periods used in the study. However, the time periods selected have important aspects, which we will attempt to clarify in the text. In the spirit of being comprehensive, we prefer to perform climatological studies using all available data, which regrettably may not always line up with other datasets or

model output availability. We will point out this limitation in the revised manuscript and upload our specific response as a follow-up comment.

With regards to changing the time periods of the analysis, we respectfully disagree with the reviewer in changing Figures 1,2, and 3 to the same time period, as 1981-2010 is a standard reference base period for performing climatological evaluations of meteorological processes (notably in Figure 3). The results do not change as a function of time period chosen.

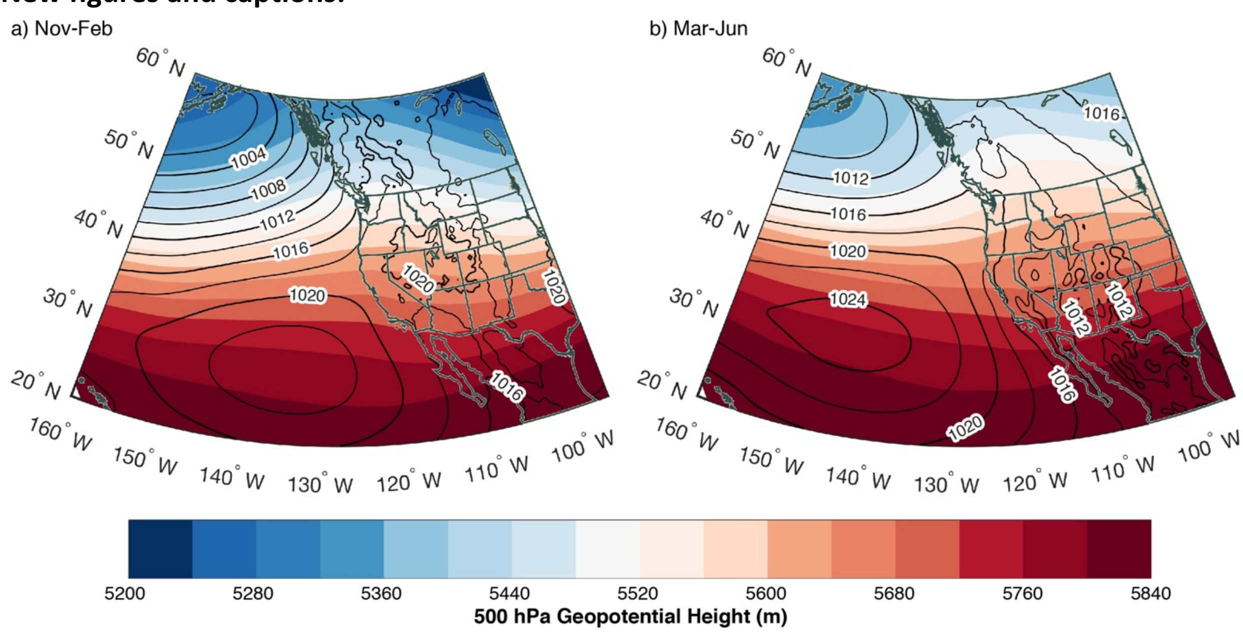
6. Figures 2c and d should be compared to 2a and b for the same time period, i.e. 1997-2014. I suggest adding the latter figures in supplementary material.

We believe that this an acceptable place to compromise on time periods. We will change Figure 2 to follow the suggestion of the reviewer, however the original figure (so as not to eliminate good data from KSBA in light of the philosophical comment above regarding inclusion of all useful data) will now be added to the supplementary material.

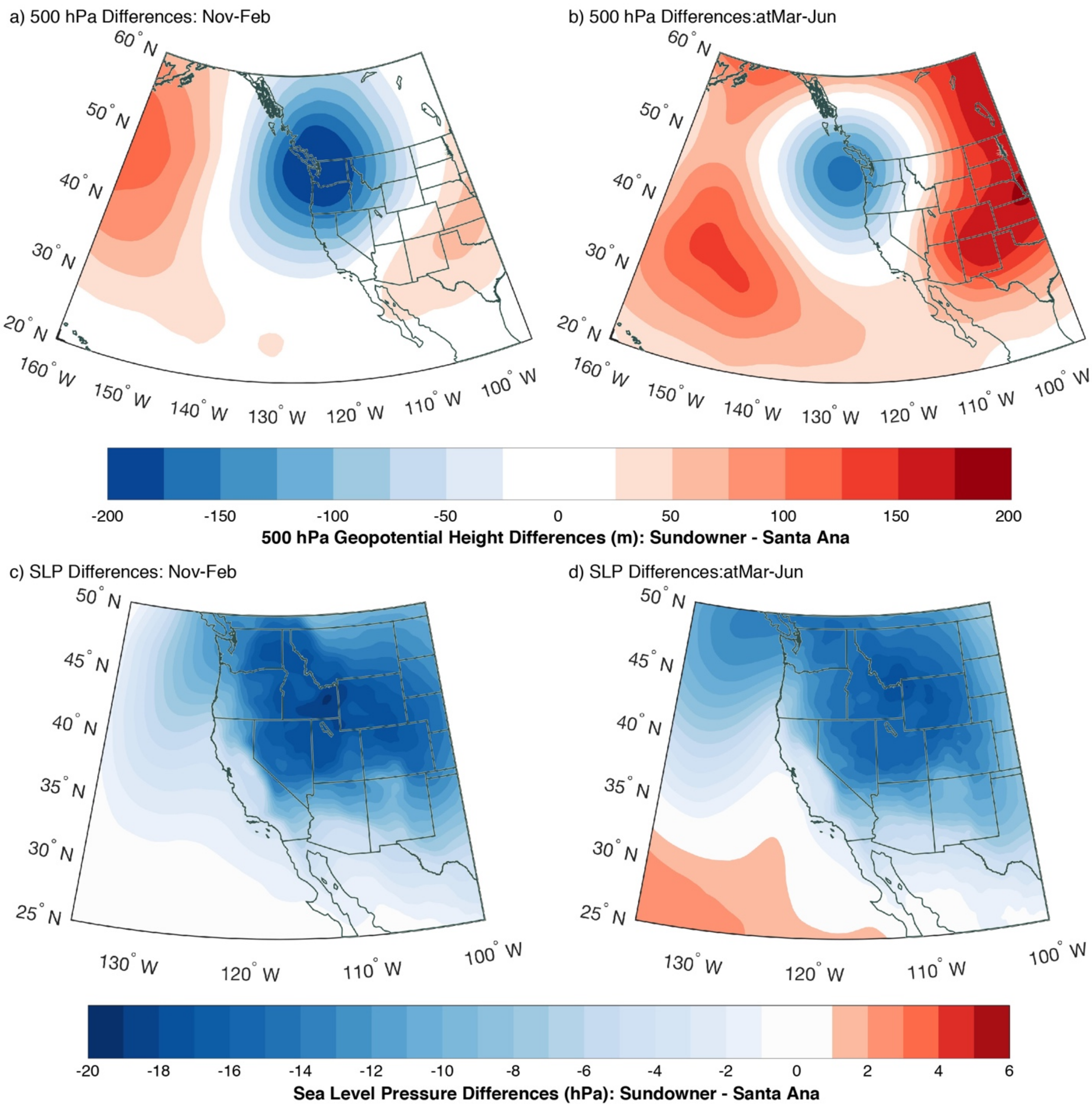
7. In figure 3 I suggest adding a composite of the 500hPa and mean sea level pressure for both seasons in order to facilitate the interpretation of the different differences.

Thank you for the suggestion, we have now added a composite for each season to the supplementary material (Figure S1). We also calculated differences for each season between Sundowner and Santa Ana Only events to further aid readers in interpreting the differences in 500 hPa geopotential heights and SLP (new Figure S2).

New figures and captions:



"Figure S1: Seasonal mean 500 hPa geopotential heights (filled contours, contour interval 40 m) and sea level pressures (contours every 2 hPa, thicker contours show 4 hPa intervals) for extended winter (a) and extended spring (b)."



"Figure S2: 500 hPa geopotential height differences between Sundowner Events and Santa Ana Only events during extended winter (a) and extended spring (b). Contour interval is 25 m. (c-d) As in (a-b) except for sea level pressure differences. Contour interval is 1 hPa."

Doyle, J.D., et al., 2011. An intercomparison of T-REX mountain-wave simulations and implications for mesoscale predictability. Mon. Weather Rev. 139, 2811–2831. Durran, D. R., 1986: Another look at downslope windstorms. Part I: The development of analogs to supercritical flow in an infinitely deep, continuously stratified fluid. J. Atmos. Sci., 43, 2527–2543. Durran, D.R., 1990. Mountain waves and downslope winds. Meteorol. Monogr. 23, 60–83. Grubisic, V., Billings, B., 2007. The intense leewave rotor event of sierra rotors IOP8. J. Atmos. Sci. 64, 4178–4201. Grubisic, V., Billings, B., 2008. Summary of the sierra rotors project wave and rotor events. Atmos. Sci. Lett. 9, 176–181. Jiang, Q., Doyle, J.D., 2008. Diurnal variation of downslope winds in Owens Valley during the sierra rotor experiment. Mon. Weather Rev. 136, 3760–3780. Klemp, J.B., Lilly, D.K., 1975. The dynamics of wave induced downslope winds. J. Atmos. Sci. 32, 320–339. Mobbs, S. D., Vosper, S. B., Sheridan, P. F., Cardoso, R., Burton, R. R., Arnold, S. J., Hill, M. K., Horlacher, V. and Gadian, A. M., 2005: Observations of downslope winds and rotors in the Falkland Islands. Quart. J. Roy. Meteor. Soc., 131, 329-351 Scorer, R. S., 1955: The theory of airflow over mountainsâĂˇTIV. Separation of flow from the surface. Quart. J. Roy. Meteor. Soc., 81, 340–350. Smith, R. B., 1979: The influence of mountains on the atmosphere. Advances in Geophysics, Vol. 21, Academic Press, 87–230. Smith, R.B., 1985. On severe downslope winds. J. Atmos. Sci. 42, 269–297. Smith, R. B. and S. Grøna◦s, 1993: Stagnation points and bifurcation in 3D mountain airflow. Tellus, 45A, 28–43. Vosper, 2004: Inversion effects on mountain lee waves. Quart. J. Roy. Meteor. Soc., 130, 1723–1748

We appreciate the reviewer providing us with these additional references, and although the NHESS guide to authors dictates a limit of 20 references for brief communications, we have added several to our manuscript where we believe them to be most relevant. We have also borrowed several techniques noted in the papers to address the major concern of reviewer 2 in terms of dynamics (please see responses above).