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Hydrology and Earth System Sciences Discussions

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Interactive Comment

# *Interactive comment on* "Numerical simulations of the impact of climate variability and change on semiarid watershed response in central New Mexico" *by* E. R. Vivoni et al.

**E. R. Vivoni et al.**

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Responses to Editor: M. Sivapalan

1. The editor comments: "In my opinion this paper falls in the category of "diagnostic" study of the precipitation- runoff response of a semi-arid basin with the use of a semi-distributed water balance model. For examples of a recent diagnostic study, please read Samuel et al., WRR, 44, W06403, 2008; Samuel and Sivapalan, WRR, 44, W09423, 2008)."

We agree that this work is a diagnostic study. Action: We have added this comment to our introduction and referenced several examples of such studies: "Using the model, our main goal is to diagnose the potential impacts of climate variability and change on



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the long-term semiarid watershed response. Similar diagnostic studies on the sensitivity of the basin hydrologic response to climate forcing have been carried out by Vivoni et al. (2007), Maxwell and Kollet (2008), Samuel and Sivapalan (2008), among others."

2. The editor comments: "I think it is a very interesting and valuable study, as any such studies can be, because it gives us insights into the catchment response, and how they might change under changed climatic or landscape conditions. However I was disappointed that in spite of very interesting presentation of the spatio-temporal variations of catchment response (seasonal, regional), and in spite of using a semi-distributed model, the results presented are only at the catchment outlet. Much of the interesting spatio-temporal variations are not presented. In this case I am not even sure a distributed model is even necessary."

We appreciate the comment in regards to the value of the work. We intentionally focused on the response at the catchment outlet for several reasons: (1) the practical need for hydrologic predictions in ungauged tributaries of the Rio Grande, and (2) the explicit linkage required of the model with the decision-support tool of Tidwell et al. (2004) through the tributary inflows. In previous work (Aragon et al., 2006), we presented spatially-distributed runoff estimates for the 68 different HRUs. Since the HRUs are coarse (see Fig. 2c), these results do not capture the fine-resolution dynamics that are expected in the real basin. This was mentioned as a limitation of the work - the spatial and temporal scales of operation of the semi-distributed model are not commensurate with fine-resolution predictions. Nevertheless, these scales are at minimum needed to resolve the semiarid processes in the basin. A fully-lumped, monthly model cannot capture the response to the summer convective storms. We indicate that the resolution limitations could be overcome by fully-distributed hydrologic models (e.g., Vivoni et al. 2007), but at the expense of computational speed. Recall that the semi-distributed model operated on a personal computer (60 year simulation in 15-minute) since the intention is for a decision-support environment. As a result, we have limited our results and discussion to behavior at the catchment outlet and its underlying runoff



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generation processes. This is deemed feasible for the level of complexity of the model and could only be achieved with the semi-distributed nature of the model and its treatment of storm-interstorm events. A more in-depth analysis of the spatial dynamics (e.g. soil moisture maps, internal channel discharge) would require a more complex hydrological simulation. Action: We added a paragraph to section 2.5 that summarizes the response above: "We focus primarily on the sensitivity of the hydrologic response to the climate scenarios at the catchment outlet due to: (1) the need for streamflow predictions in ungauged tributaries of the Río Grande, (2) the linkage of the watershed model with the decision-support tool of Tidwell et al. (2004) through tributary inflows, and (3) the restricted resolution of the semi-distributed, event-based model (see Aragón et al. (2006) for an illustration of the spatial runoff production). While detailed spatial analyses of the hydrologic response are limited, the model resolution does allow for an improved representation of semiarid processes, as compared to lumped, monthly models. This is primarily due to the improved ability of semi-distributed models to capture the response to summer storms in the region, as discussed in Michaud and Sorooshian (1994)". We also added a comment in section 4 that highlights the potential use of more complex distributed models to assess the spatial basin response: "Distributed models, in particular, would provide an opportunity to track the propagation of climate scenarios to the hydrologic patterns in the basin, such as soil moisture, runoff production, evapotranspiration, and channel discharge at internal locations, among others. For example, Vivoni et al. (2009) recently evaluated the impact of high-resolution, regional weather forecasts on the spatially-distributed basin response in the Río Puerco basin of north-central New Mexico."

3. The editor comments: "Secondly, the authors did know that the rainfall varies spatially due to the effect of elevation. Therefore, more should have been done to account for the spatial variations of precipitation, rather than discovering the effects of rainfall variability through poor fits of model predictions to observed data at the outlet."

The semiarid western U.S. is poorly gauged with respect to streamflow and rainfall,



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presenting a real challenge for climate change research. This historical reality is partly due to the sparse population in the region and its remoteness. We identified all available rainfall records with sufficient duration, precision and quality (lack of data gaps) for use in the study, with a focus on the period that overlapped with the streamflow data. Our approach was to use a simple Thiessen polygon interpolation of the five stations found to fulfill the selection criteria noted above. In certain circumstances (Datil and Brushy Mountain), we needed to estimate the stochastic rainfall parameters using non-overlapping periods. Nevertheless, this approach still underestimates the precipitation in the region. For comparison, we presented an alternative method - the use of spatially-distributed PRISM data (Daly et al. 1994), as shown in Figure 3. While this is useful in illustrating the mean annual rainfall, the dataset is not currently available at the event time scale required by the semi-distributed model. Other alternatives include use of the NEXRAD system, though this is limited by: (1) the short, non-overlapping extent (1994-present) of the data with respect to the streamflow observations, and (2) the inherent issues in using NEXRAD in the mountainous regions of central New Mexico due to beam blockage and distance from the Albuquerque site. As a result, we consider this study to be an excellent first step in the use of the semi-distributed model with limited rainfall forcing. Current efforts at Sandia National Lab include the development of a temporally-disaggregated PRISM product that would produce event-based rainfall maps (G. Klise, personal communication). Action: We have noted that the PRISM data is not available at the event time scale by modifying in section 3.1: "While the forcing is not distributed as in the PRISM data (Fig. 3, which are unavailable at the event time scale required by the model), the rainfall generation in each Thiessen polygon leads to higher precipitation in the mountain regions." We have also indicated the limitations of the spatially-distributed forcing in section 4 and described some potentially fruitful avenues to pursue: "The sparse precipitation network in the region was found to affect the model testing against the historical streamflow observations. This data limitation could be remedied by using Next Generation Weather Radar (NEXRAD), developing a temporally-disaggregated PRISM product (G. Klise, personal communication) or apply-



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ing a space-time stochastic model (e.g., Mascaro et al., 2008), though the underlying lack of rain gauge data and the streamflow period will still impact each approach." The limitation with respect to historical, ground-based rainfall data in semiarid mountain regions is a real challenge that will undoubtedly affect future efforts in determining the impacts of climate variability and change.

Responses to Reviewer 1: Anonymous

1. The reviewer comments: "The title does not accurately convey what the paper is about. Numerical simulations of the impact of climate variability and change implies that you will be getting some climate change/variability impact scenarios, determining the resulting impact on hydrological drivers (e.g. temp, precip) and subsequently the hydrological response to the altered climate regimes. What has actually been done is the development of a hydrological model, comparison of streamflow produced by this model with historical observations and then the use of this model to test how sensitive the hydrological response is to variations in precipitation intensity, duration and frequency (as well as temp seasonality). Therefore, the thrust of this paper is about modelling hydrological processes, and resulting watershed response in Central New Mexico and then testing how sensitive these processes and hydrological responses are to climate variations/change. As such I suggest the revised title "Semiarid watershed response in central New Mexico and its sensitivity to climate change."

Action: We agree that the title of the paper could be improved. We have adopted the suggested title with a slight modification: "Semiarid watershed response in central New Mexico and its sensitivity to climate variability and change". This title more closely reflects the work performed.

2. The reviewer comments: "Section 3.1 Comparison with historical streamflows. There are some serious problems here and a much more significant demonstration of the adequacy of the hydrological model needs to be presented before this paper can be accepted (as all the results and conclusions of the paper hinge on this). Specific



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problems include:"

Given diagnostic nature of this study, the important contributions are the insights provided on the simulated catchment response variability to the imposed climate changes. In this perspective, the calibration-validation exercises of the model should not be a considered a formal pre-requisite. There are many studies that do not attempt to 'calibrate' a hydrologic model when attempting to inspect sensitivities to parameters, forcing, initial conditions, or other aspects of the simulation. We have attempted to capture the basin response in a semiarid setting with poor data availability during a long historical period. This is a challenging situation for any hydrological model. In the following, we detail responses to each individual item raised by the reviewer.

3. As item a, the reviewer comments: "Excluding extreme event years just because they are not captured by your stochastic model is not acceptable. Extremes (be they droughts or floods) are what cause all the problems and therefore what we (researchers, water resource practitioners, policy makers etc) are interested in. If your stochastic model does not adequately capture extreme events then you need to use a better stochastic model."

The extreme event under question in 1972 is seven times the mean annual discharge and 5 standard deviations above the mean. This is difficult to capture in a stochastic rainfall model that assumes an exponential distribution of its parameters. Over very long simulation periods, however, there is still a probability that such an event will arise from sampling the exponential distributions. More appropriate distributions that capture heavy-tails of a phenomenon exist and could have been applied. For example, the Gumbel distribution is frequently used for extreme events. We opted to retain the simpler exponential distribution for our work as it is commonly used in the hydrologic literature and readily implemented in the underlying software used in the model (Powersim system dynamics framework). While this drawback is acknowledged, our work here does not focus on extreme events and thus this issue is not extremely critical. As a result, in our presentation of the model results, we rely on total streamflow and cumu-



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lative volumes that de-emphasize the role of extreme events. Action: We have noted this issue in the text: "Clearly, the use of an extreme value distribution for the stochastic rainfall model (e.g., the Gumbel distribution, see Bras (1990)) could help capture this rare flood event."

4. As item b, the reviewer comments: "Figure 5, i do not understand. Why are you comparing observations at one point with simulations at 3 points? And then in the text of Sect. 3.1 you refer to an average of 2.22km3. How is this average calculated? And why do you compare an average with the point observation of 9.89km3??"

There is a misunderstanding with respect to Figure 5. Each bar represents a different streamflow simulation run with uniform forcing from one gauge (either Agustin, Laguna or Socorro). The observations are the streamflow data at the basin outlet. The average is an arithmetic average of the total streamflow from each simulation. There are no 'point observations' here, we are looking at model simulations of streamflow at the outlet as forced by different gauges. Action: We have clarified this by adding: "Thus, for example, the label 'Agustin' implies that uniform forcing from the Agustin rain gauge was used to force the model in a spatially uniform fashion." We have further clarified the averaging as: "The model simulations from the uniform forcing at the three low elevation gauges arithmetically average 2.22 km3."

5. As item c, the reviewer comments: "The model validation is not very good. Ignoring the problems listed above in (a) and (b) the obs column in Fig 5 does not come close to any of the modelled values (whether it is based on obs precip or stochastically generated precip). Further, what about presenting some hydrographs and some validation stats (e.g. Nash-Sutcliffe)? Simulation of total streamflow over a 30 year period (which Fig 5 indicates is not done very well anyway) is not a very rigorous test of hydrological model performance."

We acknowledged in the manuscript that reproducing the semiarid watershed response at the basin outlet is challenging. There are many reasons for this, including the lack of adequate precipitation data, the limited spatial and temporal resolution of the model, and the inability of the stochastic (exponentially distributed) precipitation model to capture extreme events, among others. While we agree, in principle, that it would be interesting to see the hydrograph comparisons, the lumped 30-year streamflow totals provides the major point that is intended with respect to model validation. More details on the seasonal (monthly) and interannual (year-to-year) comparisons of the observed and simulated hydrographs are presented in a thesis (Aragon, 2008). This comparison reveals that the model adequately captures the seasonality of the response and has year-to-year variations, though the streamflow magnitudes are underpredicted, as expected from the streamflow totals. It should be emphasize that this study is not a traditional calibration-validation exercise (e.g., Ivanov et al., 2004), as our intent is use the model as a diagnostic tool that helps us to understand potential impacts of climate change. It is important that the model captures the semiarid processes at the scale of interest and that its behavior is reasonable. However, the real contribution is the insights provided on the catchment response and its sensitivity to the imposed climate scenarios. Action: We have modified the text to provide more details: "As detailed in Aragón (2008), however, model simulations capture the interannual and seasonal variations in the Río Salado streamflow, though not the correct magnitudes, as anticipated from the comparison in Fig. 5. For example, the simulated streamflow preserves the summer discharge season (July to Sept.) and illustrates year-to-year variations that begin to mimic multi-year wet and dry periods. The reader is referred to Aragón (2008) for the more detailed comparisons."

6. The reviewer comments: "Section 3.2: Analysis of long-term simulations. It needs to be further explained how the long-term simulations were generated and how they differed from each other. What does varying the random number generator seeds actually mean?"

Action: We have improved the description of the long-term simulations in the following manner: "The twenty-five realizations are generated by sampling the exponential dis-



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tributions describing the precipitation (1 - 3) and temperature (8) forcing with a different random seed for each ensemble member. Thus, the forcing for the long-term simulations represents climate uncertainty through the sampling of the underlying statistical distributions." In the Powersim software package utilized to build the model, sampling of uniform or exponential distributions requires the selection of a random seed. Using the same seed for each ensemble member would produce the same identical long-term forcing. Thus, the random seed is a method for producing distinct time series by sampling the same statistical distributions (i.e. with same parameters). No other differences are allowed between ensemble members.

7. The reviewer comments: "Section 3.3: Analysis of precip and temp change scenarios. You state you applied % changes to winter inter-storm duration and summer storm intensity within reasonable ranges. What do you mean by reasonable ranges? How did you decide what is reasonable and what is not, especially given observed records are only about 50 years at most? Did you account for naturally occurring multidecadal variability and if so, how? Even if you can justify your classification of reasonable, how do you know what you consider to be unreasonable now will not be reasonable in the future? Since this is a sensitivity analysis why exclude anything as unreasonable?"

We identified the summer and winter precipitation trends from existing studies (i.e. higher summer intensity and lower winter inter-storm duration) and then tested the sensitivity to a range of different values. Our reference amounts for the parameters, in both cases, are the estimated values from the long-term rainfall records, which vary with month and by rain gauge site, as shown in Figure 4. These reference values do not account for interannual or multidecadal variability since they are long-term monthly mean quantities. Since we do not use specific climate forecasts, our approach is to vary the reference values by using spatially-uniform multipliers that induce parameter changes ranging from 0 to nearly 100% (see Figure 8). This approach has been commonly used in sensitivity analyses. We selected a reasonable range of parameter changes: 0% change implies that the current parameters persist into the future and



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100% change implies a doubling of the parameter value in the future. Admittedly, this does not consider very large potential changes - say a 200% increase in precipitation intensity. However, inspection of the parameter values themselves (see Fig. 8) suggests that no additional decreases are reasonable for the interstorm duration (varies between current ˜140 hrs to future ˜10 hrs). For the storm intensity, the parameter increases from ˜2 to ˜4.5 mm/hr in the summertime. In both extreme cases (high intensity or low interstorm duration), the total precipitation over the basin has nearly doubled. This is another constraint on the reasonable range of the parameters as this magnitude of increase in precipitation may not be observed in the future (though uncertainty regarding this is considerably high, see Serrat-Capdevila et al. 2007). Action: We have addressed this issue and provided more discussion on the selection of the ranges: "These percentage changes were selected in order to: (1) span a range of impacts on the total precipitation, up to a doubling of P; (2) test the variation of DIS and I in the direction of the anticipated change and beyond the observed values in Fig. 4, and (3) retain the spatial variability in the rainfall parameters." In addition, we addressed the issue of potential for greater changes by including: "While shorter inter-storm durations or higher intensities are possible, the winter and summer scenarios should capture trends due to higher precipitation that reveal anticipated catchment behavior."

8. The reviewer comments: "JAS to represent summer? why not JJA (i.e. the standard summer season, similar to DJF is standard for winter)?"

In the southwestern United States, the selection of JAS as the summer period is more appropriate due to the strong influence of the North American monsoon. This is well known in the climate and hydrology literature for the region. Action: We have justified use of JAS as the summer period in section 3.3: "The months of JAS are an appropriate selection for the summer period in the region as it coincides with the extent and duration of the North American monsoon (Douglas et al., 1993)." A prior comment on this extent is made in the introduction as well.

9. The reviewer comments: "The combination of scenarios..ideally it would be good



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to see a worst case scenario (i.e. a scenario when all precipitation changes are in the same direction). This is alluded to in the last paragraph before the conclusions. The scenario presented was a decrease in winter storm duration (which would make things drier) and an increase in summer storm intensity (which would make things wetter), possibly cancelling each other out to some degree when considered in the annual context. An analysis of decreased winter storm duration + decreased summer storm intensity would give an indication of the DRY worst case. Similarly, an increased winter storm duration + increased summer storm intensity would give an indication of the WET worst."

This is an interesting suggestion. However, it must be clarified that both winter and summer scenarios in this study lead to precipitation increases and that these effects do not cancel out. The reviewer likely misunderstood that we are decreasing the interstorm duration in the winter, thus making rainfall more frequent. We have not created a scenario dealing with decreased storm duration, as this has not been observed in the region (Molnar and Ramirez, 2004). As a result, the possibility of creating a 'worst scenario' is limited (i.e. all scenarios lead to more rainfall, thus the worst scenario is the present condition). We do acknowledge that it would be possible to create a 'best condition' (i.e. simultaneous increase in summer intensity and decrease in winter interstorm duration). However, we deemed it more prudent to consider the summer and winter scenarios separately to avoid multiple combinations (i.e. mixing different levels of winter and summer changes) that could confuse the delivery of the main message of the paper. It was more interesting to us to compare and contrast the effects of summer and winter scenarios that matched in their total precipitation volume, as this allows a clearer diagnosis of the effects on the basin hydrologic response. As a result, this was the targeted comparison in our work.

10. The reviewer comments: "Fairly trivial but there are several typos and grammatical errors that should be corrected. For example: 'El Niño/Southern Oscillation (ENSO)' is the standard terminology not 'El-Nino- Southern Oscillation&' ; 'little is known on'



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should be ’little is known about’ ; ’Sect. 3’; etc should be fully spelled out as ’Section 3’ ; ’winter season’, ’summer season’..the inclusion of season is redundant."

Action: The changes have been performed.

Responses to Reviewer 2: Anonymous

1. The reviewer comments: "The title of paper is slightly misleading and could be revised to better reflect the research presented. In particular it may be better to replace the word ’impact’ with ’sensitivity’. The title implies that the authors will present an analysis of climate variability and change impacts on the watershed response, however the authors actually present a sensitivity analysis of the watershed response to varying climate conditions."

Action: We agree that the title of the paper could be improved. We have adopted the suggested title with a slight modification: ’Semiarid watershed response in central New Mexico and its sensitivity to climate variability and change’. This title more closely reflects the work performed.

2. The reviewer comments: "The calibration of the semi-distributed watershed model is based on data from 1949 to 1978. While it is acknowledged that this is due to limited data being available for the basin being studied, this period corresponds to an epoch of predominantly negative Pacific Decadal Oscillation (PDO). Previous research has shown that the PDO influences rainfall in New Mexico on multi-decadal timescales. In particular, the negative phase of the PDO results in dry conditions in New Mexico. This is mainly due to the higher frequency of La Nina events when the PDO is negative. Basing the calibration of the watershed mode on this period may therefore bias the model to replicate dry conditions well; however the model may not perform well under wetter conditions. It is suggested that this be acknowledged in the paper or if possible, calibration results shown for wet and dry years within the calibration period. This may be important if future simulations include ’wetter’ climate conditions."



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This is an excellent point. The calibration period was determined by data availability - stream gauge observations and the precision of rainfall observations. This happens to overlap, coincidentally, with only one phase of the Pacific Decadal Oscillation (PDO), which primarily influences the winter precipitation considered here. Interestingly, the major discharge event during this period occurred in 1972 during the summer. We only have a few years of streamflow data after 1978 (until 1984), so the short record does not have events as large as 1972, even after the PDO shift. While this certainly merits more detailed inspection, we believe this is a bit beyond the scope of our work. To our knowledge, the effects of PDO on streamflow observations in New Mexico basins spanning the climate gradient mentioned in the paper has not been conducted. This would be a natural first step, prior to investigations with the model. Given that the stochastic rainfall and temperature models do not account for multiannual or multidecadal variability, it would be premature to consider the effects of PDO or ENSO on the hydrologic response using the model forcing. Furthermore, it is unclear that the model calibration is biased dry, as suggested by the reviewer, since the long simulation extent has both wet and dry spells, despite being in the same phase of the PDO. Guan et al. (2005) also illustrated that it is the destructive or constructive interference between PDO and ENSO that is a major determinant in the regional precipitation. Action: We have added: "Note that this period coincides with a single phase of the Pacific Decadal Oscillation (PDO) and a range of different ENSO conditions, which have been shown to influence precipitation in the region (Guan et al., 2005)."

3. The reviewer comments: "The use of 25 realisations is too low to represent uncertainty associated with the hydrological model response. It is suggested that this be raised to 100 at a minimum."

While the use of twenty-five realizations may be limiting, we found these exhibited sufficient forcing uncertainty for the purposes of this study, in particular given that the simulation lengths exceeded 60 years with the storm/interstorm time step. Thus, a large number of events were simulated in each realization. A detailed analysis of the sensitivity to the ensemble size was not performed. However, we believe that the study conclusions obtained from the 25 realizations would hold for the larger ensemble sizes. Action: We have recognized the small ensemble size in section 2.5: "While the ensemble size is small, the long simulation duration (60-year) and the storm-interstorm event time step ensure a large sample size of wet and dry periods in each ensemble member."

4. The reviewer comments: "The months December to February are chosen to represent winter, while July to September is used to represent summer. An explanation of the choice of summer months is required since they do not correspond to the 'typical' summer season (JJA).";

In the southwestern United States, the selection of JAS as the summer period is more appropriate due to the strong influence of the North American monsoon. This is well known in the climate and hydrology literature for the region. Action: We have justified use of JAS as the summer period in section 3.3: "The months of JAS are an appropriate selection for the summer period in the region as it coincides with the extent and duration of the North American monsoon (Douglas et al., 1993)." A prior comment on this extent is made in the introduction as well.

5. The reviewer comments: "Section 3.2. line 15. 'the long-term simulations allow quantifying how precipitation' should read 'the long-term simulations aid to quantify how precipitation' or similar."

Action: The change has been performed.

6. The reviewer comments: "Section 4, line 24. 'this allows gaining insights' should read 'this allows insights to be gained' or similar."

Action: The change has been performed.

Responses to Reviewer 3: Anonymous

1. The reviewer comments: "The paper by Vivoni et al is an excellent contribution to



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the literature on hydrological modeling of climate change. While I agree that the mean annual rainfall is underestimated by using just rainfall data from the lower elevations, I think the authors should also consider adding another cause of underestimation. It is widely known that un-shielded raingages are affected by wind-induced catch deficit. In this area of New Mexico, the average catch deficit is about 10 percent when the average wind speed of about 8 mph is considered. This further increases the underestimation of the total basin rainfall and should be considered in the hydrological modeling. I think the manuscript should be accepted subject to the authors considering this additional cause of precipitation data inaccuracy."

We agree that the problem of precipitation undercatch is a potential source of error in the observations used in this study. Action: We have noted this as a potential error source: "Some of the datasets are of limited lengths, may not be completely representative of the historical rainfall at their respective locations or may exhibit problems of precipitation undercatch in unshielded rain gauges, particularly under high wind conditions."

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