We thank Nir Krakauer and the anonymous second reviewer for their comments. Below, we provide an answer to each individually.

Response to Review A Dr.Krakauer's main issue with the substance of our study is cited as follows: *1482: I would be concerned that to "estimate local R to F relationships for points in each 1 by 1 degree square" is essentially fitting interpolation noise, as neither field will have many actual observations within a typical square. This may explain the large variability in the regression coefficients seen in Figures 5 and 6. It may be better to estimate these relationships on a much larger scale, for example over each of the 9 climatic regions mentioned on p. 1472.* Thanks for the suggestion. We are fully aware that the regression on the spatial pattern is not optimal because it might be picking up the fine spatial structures of rainfall that underlie the PRISM interpolation of the erosivity field. We have tried your suggestion.

We have approximated the regional subdivisions of Palecki et al 2005 (see reproduced figure below) with 8 regions of simplified shapes (Palecki's regions 8 and 9, which are intermingled, but separated by elevation in the original study are merged into a single region for this analysis) and regressed the target erosivity R onto either the daily precipitation or the monthly modified Fournier index (we used a linear relation in this case).

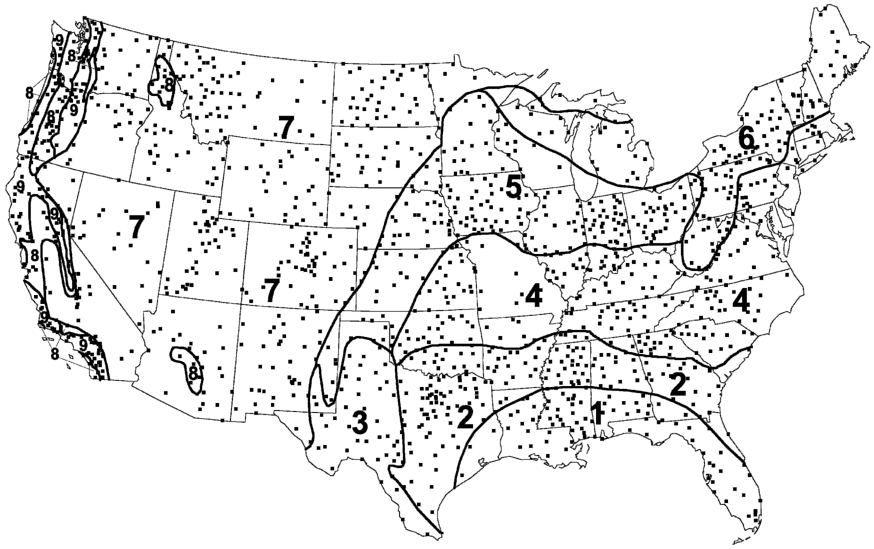


FIG. 1. Storm precipitation characteristic cluster zones. Squares indicate the locations of stations used in this study.

Figure 1: Reproduced from Palecki et al 2005. Note that this map has a different projection from the standard lat-lon grid, so that the regions would appear of different shapes in Figures 1 and 2, even if we had not simplified them.

Figure 2 shows the target R (Fig 2a), the 8 regions (Fig. 2b), and the results of the two regression methods (the regressed R's are in Figs. 2c,d, while the coefficients of the regressions are in the lower panels). The results are not encouraging: the coefficients vary sharply across regional boundaries and the biases of the regressed R are substantial. For example, one can easily see that both methods underestimate R in Florida and overestimate it along the Northeast coast. This is of course to be expected, given that this method is a lot more parsimonious with regression parameters.

The regional regression is overall picking values similar to the local regressions, with the same overall gradients between the east and the west and the northeast and southeast. The clearest exception is for the non-linear regression with daily precipitation: the midwest has a high exponent (red colors in Figure 2g indicate values close to 2; see also Table 1), while the local regressions gives values closer to 1 as in the rest of the eastern US.

Overall, the regional regression biases are large enough (using either precipitation metric) to lead us to conclude that basing the regression on larger regions is not beneficial, compared to our original choice of a 1° x 1° grid. We mention this result in the revised version of the paper in the following paragraph:

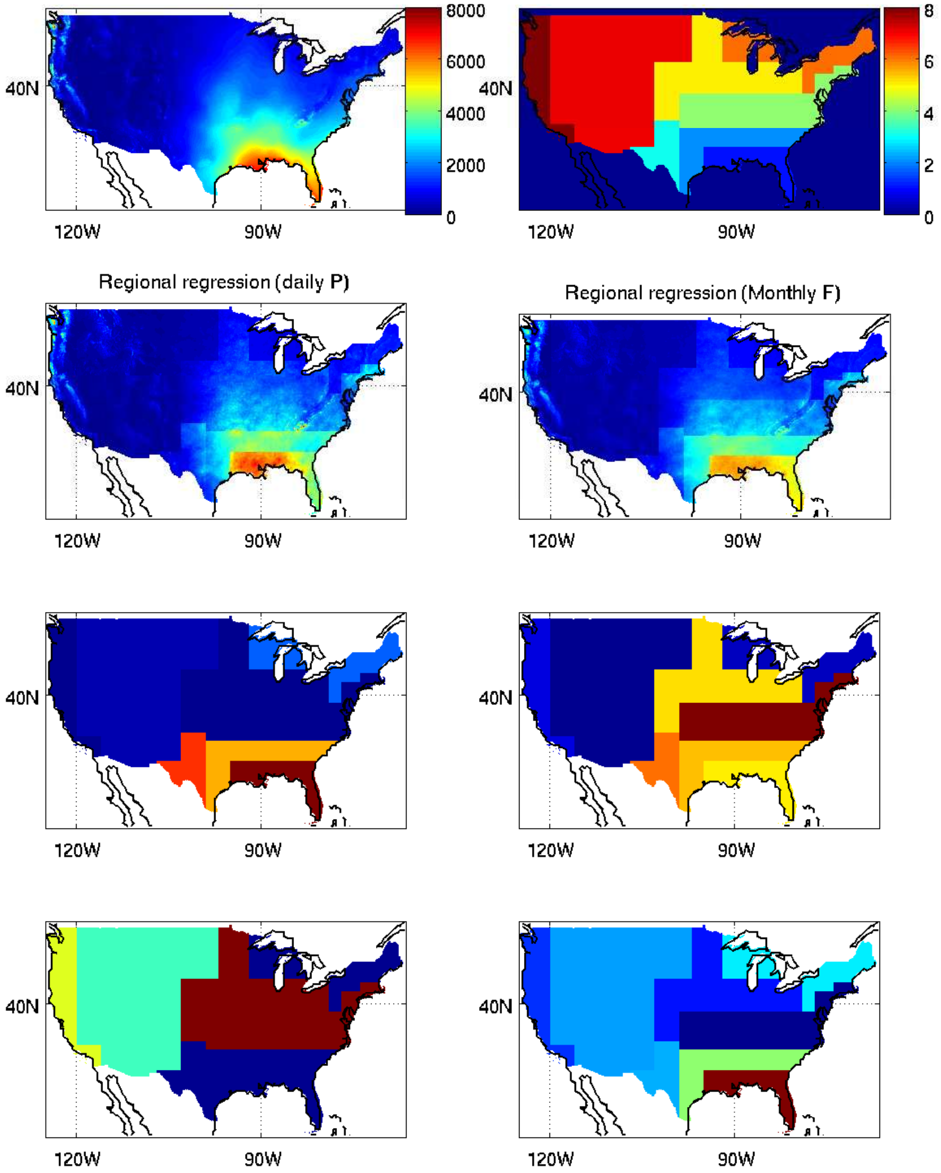
> Another possible choice for building the regression between rainfall (either at daily frequency or aggregated into F) and erosivity would be to base it on larger regions that encompass broader variations in the relevant fields than what is seen in a 1° x 1° square. Taking inspiration from the regional clusters of Palecki et al (2005)—which select regions with fairly homogeneous mean storm characteristics—we have defined 8 regions and regressed rainfall and rainfall characteristics across the gridpoints within each region. The regressions are more biased in this case than in the case of the local regressions, independently of the choice of rainfall variable. This is as expected, given that we use much fewer parameters, and again underscores the uncertainty in the estimates or erosivity that are based on rainfall accumulation. In the rest of the study, we proceed using the estimates obtained by local regressions, mindful that these regressions should be interpreted more as a simple scaling than as a robust physical relationship.

1481: for observations of change in intense daily rainfall, can also now cite Asadieh and Krakauer (2015), Global trends in extreme precipitation: climate models versus observations, HESS, 19(2): 877-891
Done, thanks.

Figure 10b: Specify the intensity measure (e.g. "mean daily intensity").
Done, thanks

Figures 11-13: I suggest using fewer distinct colors to make them easier to distinguish (similar to the color scale of Figure 10).
We have used the color scale of Figure 10 for the new figure (see answer to the other reviewer), but for changes in erosivity we have chosen to leave the color scale unchanged. The reason is that changes in erosivity are mostly positive and need more green shades to be able to show a similar amount of detail.

There are a few typos
We fixed the ones you mention and we read it through one more time to make sure there are no others.



Regional regression (daily P)

Regional regression (Monthly F)

Figure 2: Erosivity Regressions Using Regional Data. (a) Target erosivity R. (b) The definition of the 8 regions taken to approximate those chosen by Palecki et al, 2005;a value of 0 is assigned to ocean points and land points outside the continental US, values 1 to 8 are used to label the regions. (c,e,g) Regression based on daily values of precipitation. (d,f,h) Regression based on the monthly modified Fournier Index. (c and d) Regression estimates of R, (e and f) coefficient "a", (g and h) coefficient "b". See main article for the regression formulas. The regression coefficients are also given in tabular form below.

Table 1: Regression coefficients for the 8 regions and 2 regression methods. Values in the first set of rows are "a" and "b" for the regression based on daily rainfall ($R \simeq \frac{a}{N} \sum_{n=1}^{N} \sum_{d=1}^{365} P_d^b$) . Values in the second set of rows are "a" and "b" for the regression based on the Fournier index ($R = aF + b$)

1	2	3	4	5	6	7	8
0.1533	0.3265	3.6950	0.1320	3.1929	0.1270	4.4898	1.0485
1.5237	1.3774	1	1.8841	1	1.8771	1	1
6.8674	5.0917	21.2874	18.7407	19.6682	26.3194	1	6.1067
-0.3252	0.0409	0.1307	-0.4255	0.8941	-0.9292	2.7112	0.3703

Response to Review B Reviewer B's main issue with the substance of our study is cited as follows: *[...] the uncertainty from climate models (using the 21 simulations) is rather superficially addressed in Figs. 10-12.* We regret that we did not present a more fuller picture of the uncertainty due to climate models. No doubt, this stems from the fact that we are more familiar with that literature and took it too much for granted. We have now added one figure that goes some way into addressing this issue. We have also added more reference to the relevant literature. The new figure shows the projections in mean accumulation and in the modified Fournier index in the 21 models that were used in the downscaled dataset and in a much larger ensemble (38 CMIP5 models, some of them with several ensemble members). This analysis allows us to comment on the role of the downscaling technique (in some regions downscaling changes the sign of future anomalies) as well as on the role of model choice and ensemble size.

Particular comments:
Title: Should be more specific (e.g. "Projected changes in US rainfall erosivity").
Done!

Tables: Many experiments are performed and the paper is not easy to read. A Table summarizing the experiments, the corresponding equations and Figures would be very helpful.
We are sorry that you found navigation of our exposition difficult. We took your concern to heart, but preferred to guide the reader throughout the text, instead than with a table. What we have done is to introduce in Section 2.2 the 3 methods of estimating R as methods 1, 2, and 3 and afterwards we have added a reminder of what is what throughout Sections 2 and 3 and in all the figure captions.

P. 1471, L. 26-27: In the end, are your results valid for crops only ? Please clarify.
We have added a comment clarifying that our statement pertain only to the climatic changes, and that erosion will change differently in response to changes in rainfall erosivity, depending on all other factors, including the geography of the terrain and the vegetation cover. The full paragraph now reads:

> While climate, vegetation, and soils can all be coupled in complex ways (for example, excessive heat and drought can kill the vegetation cover and induce more soil loss, even if rainfall erosivity declines; see Nearin et al. 2004), we can start to address the role of climate change on soil erosion by first isolating the changes in the erosivity of

> rainfall while setting aside the impacts of changes in vegetation, soil composition and topography, land use and management. That is: while we address changes in the direct climatic driver of erosion, namely erosive rainfall, future changes in erosion will depend on local susceptibility, thus on geography, land use and management, and the health of the vegetation cover. Addressing erosion changes is beyond the scope of this work.

P. 1476 (Section 2.1): The considered future period (2079?2099) is only given in the caption of Fig. 10. It should be mentioned here.
Sorry. The paragraph now starts:

> Climate change portrayed in this study as the difference from the historical period and the decades 2079?2099 simulated under a business-as-usual scenario (the representative concentration pathway RCP8.5).

P. 1479, L. 28 ("625 grid points"): Not mentioned before. Why 625 grid points ?
We regret the confusion: the high-resolution datasets are 1/24th of a degree, and so in each 1x1 degree, one would expect 24*24=576 points. But the grids line up so that there are 25 vertex point on each side, making the set 25*25=625 points. We have tried to clarify this by amending the manuscript to read:

> For every 1 x 1 square, we take the mean 1970-2000 erosivity and daily time series for the period 1980-2000 (PRISM daily rainfall is not available prior to 1980) and use the 625 vertex points of the 4km-resolution datasets that are included in this subdomain to determine the coefficients a and b in the formula $R \simeq \frac{a}{N}\sum_{n=1}^{N}\sum_{d=1}^{365} P_d^b$.

P. 1488, L. 16-18: I am not convinced, because your analysis of the uncertainty from climate models is rather superficial. As mentioned above in our answer to the general comments, we have added a figure that address uncertainty ion rainfall projections in a more detailed way and we have expanded Section 3 to include better references to the literature and a clearer indication of where in the US the uncertainty in the climatic signal is less consequential than the uncertainty in the relationship between rainfall accumulation and erosivity. Confident that the new manuscript makes our case more solidly, we have edited this paragraph to clarify what we mean. It now reads:

> Overall, we confirm that the general expectation of worsening erosivity under climate change is correct, but we suggest that previous estimates have been too confident for large swaths of the United States, especially in the South and in the interior. The pattern of erosivity change estimated in our study from CMIP5 changes in F is consistent with what was found by Segura et al. (2014) using just 3 models (with 3 scenario each) from the older CMIP3 archive But the pattern changes when we estimate erosivity with a different method: when erosivity is estimated from daily rainfall values, the multi-model mean indicates a decrease in Texas and the southern Plains, while an increase in erosivity is simulated by a majority of models when the estimate is done using monthly precipitation. Within this region there are locations where the climate signal itself is

> not very robust (some climate models project wetting, others drying) but there are also locations where the climate models agree on the rainfall changes. There, uncertainty in the sign of the erosivity changes is solely a consequence of our poor knowledge of how best to link erosivity to rainfall accumulation. Thus we conclude that uncertainty in the method of estimation of R can be more consequential than uncertainty derived from the spread in climate simulations.

Figures: Figure 3 is cited in the text after Fig. 4. Figures 6, 8, and 9 are not cited in the text. Are these figures useful ? Uncommented figures should be removed. Thanks for catching these problems! The early citation of Figure 4 was actually a typo: it should have been Figure 3a. As for the other figures, we discussed the biases without pointing explicitly to the figures where the biases were sown. We now guide the reader throughout the arguments in Section 2.5 pointing to the relevant panels in those figures. Overall, while we do have a lot of figures, we thought it was best to show how each method perform and to treat them all equally. So even if we discuss the results of one method more in detail because it is the first time that we bring up the analysis, we do want the reader to see for herself how other methods perform.

Conclusions: To what extent could this work be performed in other places of the globe
We are not aware of suitable datasets that would allow a similar, regional assessment elsewhere. Therefore, we now conclude the paper with the following comment:

> Outside the Unites States (and few scattered localities), the lack of high-frequency, high-resolution data and a worse uncertainty in the relationship between rainfall characteristics and erosion potential (van Dijk et al, 2002) combine to make the challenge even more formidable.