

Interactive comment on “Simulating past droughts and associated building damages in France” by T. Corti et al.

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We thank Dr. Florence Habets for her comments and suggestions that were helpful to further improve our manuscript. However, we do not agree with many comments and the overall negative judgment. In the following sections, we provide detailed answers to the issues raised by the referee. We are confident that our answers and the corresponding changes of our manuscript address all points raised by the referee.

Answer to general comments

In the general comments, the referee addresses a few points that are further elaborated in the specific comments. We therefore address the points only shortly here and refer to the corresponding answers to the specific comments for more details.

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a. The referee claims that there are only "few assessments of the modelling". As detailed in point 10 of our answers to the specific comments, we maintain that our model is appropriately tested. The referee does not acknowledge the validations we performed, neither concerning the soil model nor the interannual variability of the soil subsidence damages, which agree well with the observed damages.

b. We agree with the referee that our presentation of results is in a few places unclear, see our detailed answers below, especially points 4 and 7.

c. According to the referee, "some hypotheses or statements don't seem correct, or at least, in contradiction with other sources of information". After reviewing the data provided by the referee, we conclude that they are not appropriate for gauging the present study and not in contradiction of our findings. See points 2 and 8 below.

d. According to the referee, we claim that there was an "increase of the cost by damages in 2003". This opinion probably originates from a misconception on the referee's part. First, the observed damage for 2003 exceeds our simulation results (see Table 1 in our manuscript). Contrary to the referee's implication, our model does not overestimate the 2003 drought. Second, the cost per damaged building should not be confused with the cost per capita. See point 9 for more details.

e. The referee states that "building damages linked to drought are localized in restricted and well known areas (constructed areas with clay soil)". We agree that the presence of constructed areas and clay soils are necessary for subsidence damages. As explained in our study, we consider the distribution of constructed areas by weighting by population density (see also the following point and point 4). In our model, geological characteristics are subsumed in average, assuming that the influences of regional differences geology are negligible on a national level (page 1466, line 3). The presence

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of buildings and clay soil is however not sufficient to explain the occurrence of building damages. Particularly to explain the interannual damage variability, other factors have to be taken into account. The probable dominant role of meteorology in this respect is a main finding of our study. This has been completely disregarded by the referee. In addition, she does not offer any alternative explanation.

f. Finally, the referee claims that "in the modelling, every grid points are used". As detailed under point 4 below, we do not use every grid point equally, but weight by population density. Our model works exactly as the referee thinks it should.

Answer to specific comments

1. "Section 1":

The reviewer mentions the ongoing geological risk assessment in France. We concur that it should be mentioned and do so in the revised manuscript (Page 1466, Line 10).

2. "Section 3.2":

A detailed map showing the geographical distribution of the damage level in France would indeed be valuable if we had detailed observations for comparison. The available damage data (including the sources mentioned by the referee) are however insufficient, since the accuracy of reported damage levels on a regional scale is extremely low (Reasons for that are, among others: regional differences in insurance penetration, report standard, administration, market shares of insurance companies etc.). We therefore only considered national damage estimates (which have acceptable uncertainties, as show in Figure 4) and focused our study on the temporal damage variability. More accurate damage data with regional resolution is needed to validate the geographical distribution of soil subsidence damage simulations. We now mention this in Section 6 of the revised manuscript and thank the referee for raising the issue.

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3. "Section 4":

We agree that the GSWP and BSWB analyses do not focus on the urban area. Our soil model does neither. It is a general soil water balance model, which implies that the results can be compared to other (more sophisticated) soil models for validation. As we state on page 1469, line 120, we validate the simulated soil moisture content, not the entire model. In Figure 2, we therefore compare domain-averaged soil moisture anomalies in France. However, we realize that it is not entirely clear what we show in Figure 2 and mention that more precisely in the revised manuscript (caption of Figure 2 and Page 1469, line 15).

4. "Section 4.2":

The referee wonders why we "did not select only the urbanized area to build the vulnerability curve". We did, actually, do exactly that. As explained in Section 4.2, the vulnerability curve is derived based on damage simulations (page 1470, line 17-27). The damage simulations include a weighting with population density through the multiplication of damage levels with population density (see Page 1470, line 4). Contrary to the referee's understanding, unpopulated grid points (such as "pixels covered by forest, cultivated area and other natural surfaces") do not contribute to the damage computed by our model and therefore have no influence on the derived vulnerability curves. To make this point entirely clear, we state this explicitly in the revised manuscript (Page 1470, line 4).

5. "Section 5":

As in the comment concerning Section 3.2, the referee suggests a validation of the model through the geographical damage distribution. We know these damage data as shown in the Interactive-Comment-Fig. 1 and can say here that our simulations agree quite well with the observed damage distribution. However, as stated in our answer to Section 3.2, the accuracy of these data on departmental level is low, which can be seen in the large differences in damages, reported by different insurance companies and

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agencies. We need more accurate damage observations for a quantitative validation. Until then, the representation of the interannual damage variability is the best way of quantitatively testing the model. This seems especially to be the more valuable validation to us, since meteorology is the only factor that can contribute substantially to year-to-year fluctuations. We are surprised that the referee does not acknowledge the value of the good performance of our model in that respect at all.

6. "Line 20", (Page 1472), discussion of Figure 5:

The referee asks how "averaged soil moisture over France can be sufficient to explain the occurrence of drought". As stated in our answer to Section 4.2, we do not use averaged soil moisture to simulate soil subsidence damages, but simulate the local damage occurrence and weight by population afterwards. This is also stated very clearly in our manuscript, e.g., at the beginning of Section 4.1 ("We simulate soil subsidence damage on a grid..."). Moreover, we state in the discussion of Figure 5 that the histograms presented there have been derived employing weighting by population density (e.g., see caption of Figure 5). We hold that this point should be clear to the attentive reader. Moreover, we stress the point of weighting by population in the revised manuscript as explained in answer to Section 4.2.

7. "Section 5.3":

We agree that the discussion of Figure 7 is not very clear in the original manuscript. This is improved in the revised manuscript. Specifically, we explain that the different areas (blue, red and gray) are derived from our simulated annual soil moisture deficit, and that the damage levels are computed from the damage data for the year 2003 mentioned in Section 3.2. We are grateful to the referee to pointing out this lack of clarity.

8. Performance in 2003:

We agree that the blue area in our Figure 7 does not entirely coincide with the Figure C906

1 provided by the referee. However, the agreement is quite good; in both figures, the affected areas essentially form a C-shape spanning across France. Concerning the areas affected in 2003, we doubt that Figure 2 is of great value to discuss the case. The figure was produced in 2006. At that time, the damage assessment for 2003 was far from finished. Still, most departments in Figure 2 feature green spots, probably originating from 2003. In addition, the large extent of the 2003 drought in France is well recognised in the scientific literature (e.g., see references on page 1473, line 9 of our manuscript). We conclude that the points raised by the referee are not sufficient to reject our model. Specifically, the provided figures do not contradict our results. We strongly disagree that the model is "also wrong for the previous period" and that there is a "bias in the vulnerability curve". Our model might not be perfect, but it shows good results, especially concerning the representation of the interannual variability and in the light of its pioneering character. Qualifying the model as "wrong" is entirely inappropriate.

9. Regional differences in sensitivity to soil subsidence:

The referee points to a report on soil subsidence (the link is unfortunately broken, we assume it is Dumas et al. (2005), as cited in our manuscript) stating that the average cost per damage is stable for each year, and around 10 kEur per damaged property. We agree that the average cost per damaged property might be constant. Contrary to the referee's opinion, this does however not mean that the damage per capita has to be the same in all regions affected by droughts, since differences in the sensitivity to soil subsidence can not only arise from differences in cost per damaged building. Differences in the number of properties affected by soil subsidence in case of a drought can also lead to different sensitivities. This means that the lack of preparedness and adaptation in some regions can lead to a higher fraction of buildings affected by soil subsidence in case of a drought and thus result in a higher damage per capita.

10. "Conclusions":

The referee concludes that "there are not enough assessments of the results in the article to be confident with the conclusion presented." We strongly disagree. Citing referee #1, we maintain that our "model is appropriately tested and the conclusions are prudently restrained." Referee #2 does not acknowledge the validations we performed, neither concerning the soil model nor the interannual variability of the soil subsidence damages, which agree well with the observed damages. The referee does not consider that we discuss the limits of our study and that our conclusions are restrained (e.g., our results "suggest a strong meteorological influence"). Furthermore, the referee is unable to disprove any results and conclusions of our study, particularly on a scientific level.

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