Author's Response to the Interactive comment on **"Impact of large wildfires on PM10 levels and human mortality in Portugal"** submitted by Patricia Tarín-Carrasco et al.

A: First, we would like to thank the anonymous referees for their valuable comments in the interactive comment on "Impact of large wildfires on PM10 levels and human mortality in Portugal" by Patricia Tarín-Carrasco et al. The manuscript was revised taking into consideration the reviewer's comments in order to improve the quality of the paper. Please see below our point-by-point replies:

IMPORTANT NOTE: When revising the manuscript, we found some inconsistencies in the data presented in terms of number of fires and burned area. Tables 1 and SM1 were changed accordingly, and although there were some very slight changes in the correlations, the discussion and main conclusions remained the same overall.

Anonymous Referee #1

General comments:

This is an interesting and timely study. There has been a recent increase in studies linking health impacts from climate induced disasters and the manuscript touches on a highly relevant topic. However, I do have some questions about the data and the method, on which I have elaborated below.

A: We thank the kind appreciation of our work and the valuable comments and suggestions proposed. We have made an effort to comply with them.

Detailed comments:

Why did the authors decide to use Global Forest Watch instead of, for example, CORINE?

A: In this study we cover the period from 2000 to 2016. We intended to give the most up-to-date picture possible and this data from GFW refers to 2015, whereas, as stated in its web page "The CORINE Land Cover (CLC) inventory was initiated in 1985 (reference year 1990). Updates have been produced in 2000, 2006, 2012, and 2018." This means no update was produced from 2012 to 2016 and this was the reason to choose otherwise.

Why did the authors select those four months? Would it have been an option to look at large events (>1000ha burnt) any time of the year? Can the authors explain this choice?

Especially in light of air pollution (e.g., from PM) being worse in winter as air gets trapped under cloud cover (see for example Pey et al., 2010).

A: In this paper, one of the aims is to assess the impact of PM10 derived from large wildfires (forest fires) on human mortality. As a first approximation, we gathered data of wildfires, pollutant and mortality data during the whole year for the period in question (2001-2016). But in Portugal, forest fires usually occur during the summer months (June to September), which correspond to the highest temperatures and driest conditions. This was already explained on L.111: "For this study, forest fires occurring in the months of June, July, August and September 2001-2016 (the months with highest temperatures and drier conditions when more than 65% of fires happened)". In terms of large fires, only negligible exceptions occur outside this period, and by focusing our study only on these 4 months, we can have enough data to perform a valid statistical treatment, while avoiding a strong influence of PM10 from other sources in colder months (such as home heating or traffic); and, at the same time, not including in the analysis deaths due to cold and flu, for instance. This point is now further clarified in the revised text (Methodology section).

L.121: it may be helpful for the reader to refer to Fig 2 for a map of the locations of the monitoring stations.

A: We agree with the reviewer's suggestion and the locations of the monitoring stations were added to the map in Figure 2.

L.121-125: could the authors expand on their method to correct PM10 data from wildfires from "normal" non-wildfire causes? I am a bit unclear as to what the authors mean by "background stations".

A: The main anthropogenic sources of PM10 include road traffic, industrial activities, and home heating. In this study, to minimize the influence of non-wildfire causes for the PM10 concentrations, we selected only background stations (encompassing urban and semi-urban ones, which are located within urban areas but with minimum influence of road traffic; and rural stations). Therefore, urban stations with road traffic influence and stations close to industrial complexes were not selected. The influence of home heating was already minimized by selecting the summer period as our target timeframe. This explanation in now included in the revised text.

L.133. What is the impact of the uneven distribution of the background stations on the outcomes of the study? Especially since some of the NUTS-regions don't have any stations, while the urbanized NUTS regions have the highest number of stations but the lowest wildfire risk. Moreover, what did the authors do with the PM10 data in NUTs

regions with more than 1 station? Did they average the PM10 values of all stations in one NUTS region?

A: The uneven distribution of the background stations indeed supposes a difficulty to estimate the PM10 distribution over Portugal. However, this was the most complete database available. For this reason, the NUTS that had no measuring stations were left out of the study, concerning the relation with the wildfires.

Regarding the NUTS with more than one station, the reviewer is right, we estimated the mean of the PM10 concentration of all stations. An explanation in now added to the text: "For the NUTS III regions with more than one measuring station, the mean between the PM10 concentration from all the stations in the NUTS was considered".

L.140-142: I am unclear as to why a reduced number of deaths (over the selected time period?) prevented the authors from including a correlation for COPD and asthma. Then the authors would just find a smaller correlation between PM10 population from wildfires and these particular mortality causes? Can the authors elaborate?

A: We believe there is a misunderstanding, as we mean that since in many months and NUTS III in the target time period there were no deaths for COPD and asthma it was not possible to obtain a data series large enough to correlate with PM10 and wildfires series. This is now clarified in the text.

L.145-147: if the authors use Pearson, then why explain Poisson as well? I found this a bit confusing. (Later, in section 3, I read that the authors use the Poisson for the RR. I recommend explaining this more carefully in section 2.3.)

A: All the correlations regarding health parameters were done using the Poisson method, which is more indicated for that. The other correlations were done using the Pearson method. In no case was a duplication of correlation approaches. This is now clarified in the text. In section 2.3 we explain two subsections. 3.2.1 explain the correlation between PM10 and burnt area through Pearson. In 3.2.2 is not possible to use Pearson for the correlations between PM10, burnt area and mortality, for this reason we use Poisson correlation.

L.154. From an earlier sentence (L. 111-113), I got the impression the authors only selected large fires in the time period of June-Sept, but from L.154 it appears that the authors correlated with both types of wildfire sizes (<1000ha and >1000ha). Maybe clarify this both in section 2.2.2 as well as in 2.3.2.

A: Only large wildfires were selected for the analyses. However, it was also considered to study if the associations between PM10 and mortality were stronger in the presence of large wildfires. But in this case, this was only a qualitative correlation, that is, "yes" if

there were large fires in the studies period/region or "no" otherwise. This is now clarified in the revised version of the manuscript.

L.189-192: this paragraph seems out of place. I think it would fit better in the introduction, or – alternatively – link it better to the findings from Table 2.
A: The reviewer is right and this paragraph was moved to the Introduction in the revised version of the manuscript.

Table 3: what is the difference between the 3 instances of columns headed with "All" and "Months w./LF"? From the text I gather it is the three types of death causes but it would be good to make that clear in the table.
A: The reviewer is correct, there was a mistake on this table. It is now corrected in the revised version of the manuscript.

Table 3 mentions 2016 population numbers while figure 2 shows 2011 population numbers. It would be good to match these. Also, were annual (monthly) mortality numbers corrected for annual population numbers? Would it be better to use mortality rates instead?
A: The reviewer is right and Table 3 now has also the 2011 population numbers. Regarding the correction of the mortality by the population on a yearly basis, this was not done as the population had very steady numbers throughout the studied timeframe.

I think the paper could be restructured a bit by moving the content of section 3.1 up to section 2 (each of the different data sections). In my understanding, the sub-sections of 3.1 present (a discussion of) the input data while the actual analysis (and aim of the manuscript) of the relationship between wildfires and mortality are the core of the analysis and should indeed be presented in the results section.
A: We can understand the point of view of the reviewer and rearranged the manuscript to comply with the suggestion. As a result, Table 1 is now in Supplementary Material as Table S1; old Tables 2 and 3 are now, Tables 1 and 2, respectively; old Figure 2 is now Figure 3 in the revised manuscript; and old Figure 3 is now Figure 2.

L.261: how do the authors explain the lack of correlation between wildfires and PM10 for these regions?
A: This lack of correlation could be due to the limited number of stations in those areas (which means fewer data to correlate) and their location and uneven distribution. In fact, in some NUTS, there may be one or only a few stations, which can be far from the

reported forest fires. This is the case of Alto Minho and Algarve. This explanation is now included in the text.

L.268: "in some areas": why only in some and not in others?
A: This can be due to the lack of the data or other confounding factors. For the sake of clarity, this explanation was added to the text.

How do the findings compare to similar studies? How does the paper add to existing work such as the cited Faustini et al. (2015) paper?
A: As in other studies, we find correlation between the increase of PM10 due to wildfires and the increase of mortality due to wildfires and PM10. Following the reviewer's comment, a discussion on this topic is now included in the manuscript.

In the conclusions, the authors remark that many other aspects could have influenced their findings, such as aged population, lower socio-economic status, etc. Why did they not correct their input data for this?
A: It would indeed be an added value to the study to include these aspects, but unfortunately, the scarce data available and the lack of accuracy in the existing ones prevented us from estimating/including a correction regarding their influence.

Minor comments:

- L13-14: "the 48% of wildfires occurred were large fires". This doesn't flow well, there appears to be a word missing.
  A: The mistake was corrected it in the revised version of the manuscript.
- L16: "on the future" should be "in the future"
  A: Corrected as suggested.
- L23: "an increase on" should be "an increase of"
  A: Corrected as suggested.
- L26. I would replace "fustigated" with "hit" or something else. Fustigated is a bit lyrical in this context.
  A: Corrected as suggested.
- L85 (and L.90): "is focused in" should be "focuses on"
  A: Corrected as suggested.
- L94: the urbanized areas are depicted in red in Fig. 1, but it may be helpful to the reader to add the place names referred to in this sentence.
  A: The names of the biggest cities are now included in Fig. 1.
- L99: the use of "allied" and "verified" is incorrect in this sentence.
  A: Corrected as suggested.

- L105-108: check referencing style and sentence structure
  A: To clarify, the following sentence was added to the text: "NUTS is a geocode standard for referencing the subdivisions of countries for statistical purposes developed by the European Union. The geocode is divided in three levels (I, II, III) which are established by each EU member country. NUTS III from mainland Portugal (in total, 23) at a 1:60 million scale were retrieved from the Eurostat web page (Eurostat, 2019) and treated with QGIS3 software.". Moreover, the following reference was added to the text and Reference list: Eurostat: NUTS 2016, version 14/03/2019, 1:60 million scale. Retrieved at https://ec.europa.eu/eurostat/web/gisco/geodata/reference-data/administrative-units-statistical-units/nuts (last accessed on 19 June 2021)
- L120: "network" is this a network of measuring points/stations?
  A: Yes, it is. We mean the location of the stations over the country. The text was modified to increase clarity.
- L128: this sentence seems to belong in the previous section on wildfire data?
  A: In fact, that is not the case. Our intent is to say that the pollution data follows the same pattern "as wildfires data". The text was modified to increase clarity.
- I think sentence L.169-170 and L.171-173 can be merged? Also, sentence L.171-173 doesn't read well.
  A: We made an attempt to comply with the suggestions by merging the sentences and rewriting the text as follows: "The north and centre of Portugal present the most extensive forest cover in the country (Nunes et al., 2019), particularly abundant in pine and eucalyptus trees, two highly combustible species that have been associated with extreme wildfire events (Maia et al., 2014). Consequently, both areas show the highest number of wildfires and burned area (being Beiras e Serra da Estrela and Médio Tejo the most affected NUTS III), but with also Alto Alentejo and Algarve (more to the south, see Figure 1) among the NUTS III with more incidence."
- L189: check referencing style. When the authors say "area most affected" do they mean globally, or in Europe?
  A: We mean between Europe, Middle East and North Africa (the areas covered by the EFFIS report) and this is now clarified in the text. Also, the sentences was moved to the Introduction and the following reference was added: Jesús San-Miguel-Ayanz, Tracy Durrant, Roberto Boca, Giorgio Libertà, Alfredo Branco, Daniele de Rigo, Davide Ferrari, Pieralberto Maianti, Tomàs Artés Vivancos, Ernste Schulte, Peter Loffler; Forest Fires in Europe, Middle East and North Africa 2016. EUR 28707 EN, Publications Office, Luxembourg, 2017, ISBN 978-92-79-71292-0, doi: 10.2760/17690
- L194: "unequal spatial" -> "unequally spatially"
  A: Corrected as suggested.

- L195 “in the in” -> “in the”
  A: Corrected as suggested.
- L203: “represented by black dots” -> dotted or shaded area
  A: Corrected as suggested.
- L216: “cause death then” -> causes of deaths than
  A: Corrected as suggested.
- L235 – 237: sentence doesn’t read well.
  A: We have tried to rephrase the sentence to increase its clarity. “As shown in Figure 6a, three NUTS III (Alto Tâmega, Beiras e Serra da Estrela, and Viseu Dão-Lafões) present associations between PM10 and all-cause mortality during the studied period, None showed a direct significant association with the occurrence of large fires, likely due to the fact that their contribution to the total burned area in each year from 2001 to 2016 (see Table 1) was highly variable (from 12.1% in 2011 to 79.5% in 2003). However, the wildfire origin of PM10 is corroborated by the positive significant correlations obtained for these three NUTS between PM10 and burnt area (Figure 5), with Viseu Dão-Lafões displaying the highest correlations.”
- L 282 – 284: sentence doesn’t read well.
  A: For clarity, this sentence was removed, also following a comment from another reviewer.

Anonymous Referee #2

General comments:

The manuscript presents a valuable study of the impacts of wildfires on human health. While other studies of this kind have been more rigorous, the work presented here is for an understudied region, and therefore provides guidance for this region regarding the concern of wildfire smoke on human health. The work is generally OK, but the presentation needs a reasonable amount of revision. Additionally, the authors should work on providing a more complete discussion regarding the limitations and benefits of their study. See below.

A: We thank the kind appreciation of our work and the valuable comments and suggestions proposed. We have made an effort to comply with them.

Specific comments:

1. The paper needs some attention to the use of English. While not too bad, some attention to editing the text to correct wording, syntax, and general use of English will improve the manuscript and help readers to better understand the study. See below for specific location and suggestions. Some specific edits are required, while others are suggestions.

   A: The text was thoroughly reviewed concerning the English language.

2. The figures can be consolidated. Many of the figures are maps of Portugal – all good to have. I suggest the maps be combined for all "input" data (Figs 1 - 3) and a separate figure for results (Figs 4 - 6). (see note below regarding Fig 3) A layout similar to Fig 1 – a 3-up side-by-side – will work well.

   A: We have considered the reviewer's suggestion, but given the new rearrangement of the manuscript following his/her suggestion below, we respectfully decided to keep the figures separate.

3. Abstract – Has too much introduction and info on the "purpose". Sentences on lines 2-7 can be condensed.

   A: We agree with the reviewer's suggestion. The Abstract was condensed in the revised version of the manuscript.

4. Introduction – This can be consolidated. The first part of the Intro through line 34 – should be consolidated into one short paragraph. Throughout the manuscript text, many sentences can be trimmed to make a better presentation. For example, the

opening sentence can be changed to: "Wildfires have a considerable impact on the environment and humans worldwide." (I clipped out several phrases)."
A: We agree with the reviewer's suggestion. The Introduction was consolidated in the revised version of the manuscript.

(Section 2.1 is very good – very helpful to have this description for the purposes of this paper).
A: We thank the reviewer for his/her positive views on the manuscript.

5. Datasets – It is unclear if the study is considering only forest fires or all wildland fires. Please clarify. It would be helpful to have a map of fires across the study region – dots on a map or polygons of areas burned, if appropriate. Total burned area – Figure 3 – is not a project result, it is input data. So, section 3.1.1 should be integrated into section 2.2.2. In 3.1.1 you can keep info that is derived from the distribution of fires, but the data itself (shown in Fig 3) is not results.
   A: To clarify that we work with forest fires we add to the sentence "For this study only forest fires where considered…". Moreover, we have rearranged the sections in line with the reviewer's comment. However, we decided not to include the map of fires, as it would be confusing to read, given the number of events. We hope the reviewer understands our point of view in this case. But we understand the point of view of the reviewer in terms of the arrangement of the mentioned sections and rearranged the manuscript to comply with the suggestion. As a result, Table 1 is now in Supplementary Material as Table S1; old Tables 2 and 3 are now, Tables 1 and 2, respectively; old Figure 2 is now Figure 3 in the revised manuscript; and old Figure 3 is now Figure 2.

6. The manuscript lacks a proper "discussion" section. Much of the results and the text starting on line 214 (page 13) should be turned into Discussion. Also, some of the material in the "Conclusions" can be integrated into "Discussion". The Conclusions should be a summary of the primary findings and relevance, but should not have anything new in it.
   A: We agree with the reviewer and rearranged the text in the revised version to comply with the suggestion.

7. A Discussion section should be formulated (comment 6) and should include more from the authors reviewing the shortcomings, assumptions, and limitations of the study and the primary outcomes, and how they are relevant. Bring out the fact that this is new information for the region that can help with decision-making. In the limitations, there should be some acknowledgment of the coarse spatial (level III NUTS – why not better)

and temporal (Monthly – why not better) scales, and what it means to interpretation of the results. These coarse-scale approaches will "dilute" the results meaning there may be health effects that are not found because wildfire smoke is so "episodic" and "local". You may not be able to show this here, so say what is needed for a more complete understanding? Why is your study still useful?
Guide the reader to help them understand why this work is "good" or "helpful". Otherwise, it may be seen as not as good as studies that use finer resolutions, smoke transport models, etc. I think it is worthwhile, but other readers may find it too simplistic.
Discuss the value of AQ monitors – why they are helpful and why they are not for assessment of fire exposure.
A: We totally agree with this comment and the need to include the mentioned topics in the discussion. There was an effort on our part to comply with all suggestions and to have the revised text reflect the intended improvement.

Detailed comments:

Line 9; What does "these" refer to?
A: It refers to wildfires. The text is now corrected.

Line 78: "… the effects of short-term pollutants exposure …" It is unclear if this is short-term exposure or short-term pollutants. Revise this sentence to be clear.
A: The sentence was clarified in the revised version of the manuscript.

Lines 161-164: Can be deleted.
A: We agree with the reviewer and have deleted this part.

Line 202: Many sentences start with "Regarding …". This is poor sentence structure. Please revise.
A: This was corrected in the revised version of the manuscript.

Line 282- 284: It is unclear why this is relevant to your study. Remove.
A: Removed as suggested.