Thank you for your comments reviewers! I think they are helpful and will make our paper better and more clear.

Reviewer #1: This is a time-series study assessing the association between wildfire exposures and health care usages among a Southern California population who use durable medical equipment.

Major Comments

1. Why the study population was limited to population age 45 and older? Have the authors considered to perform modification analysis by different age group?

We aimed to capture people who use durable medical equipment and therefore might be vulnerable to wildfire exposure - mainly smoke from wildfire, evacuation from wildfire, stress from wildfire - because we hypothesize that durable medical equipment use indicates medical and social vulnerability. Casey et al. 2021 describes how Medicaid insurance is associated with increased prevalence of DME rental. When people rent DME, they are renting Bilevel Positive Airway Pressure (BiPAP) machines, enteral feeding machines, hospital beds, infusion pumps, oxygen equipment, suction pumps, ventilators, and wheelchairs, all of which assist with medical conditions indicating some level of disability. However, DME rental statistics also include people who rent breast pumps, which do not indicate vulnerability and may in fact indicate a certain level of health. Excluding DME using people under 45 excludes those people renting breast pumps, leaving people we hypothesize may be vulnerable to wildfire exposure. See Casey et al. 2021 for a more detailed description of the DME-using population.

TODO: Should we edit anything here? Add more description?

Can quote what we already wrote in the manuscript as well, and say ‘we described this in the manuscript’.

1. The reviewer understand that this is not a traditional cohort study. However, could the author present a traditional Table 1-like summary table to present the study population characteristics for those who had at least one event during the study period? This would be very useful to compare and contrast your study with others' for similar/different populations.

Sorry no. Can’t do this because we don’t have the thing. Can reference the whole DME population int his other paper, but here we don’t have data broken down to the categories. Resources preculd us from going abck and getting this information.

TODO: Yes – I’ll add this. But do we have demographic data?

1. How DME usage in year prior is an indicator of current vulnerability? Are there any previous studies assessed the proportion of patients who stopped DME usage within one-year of usage initiation?

We are not aware of any studies looking at the length of DME rentals. Because we think that DME rental indicates medical vulnerability, though the rental itself may not last several years, we think that any rental in the previous year indicates vulnerability because we count on someone’s health remaining somewhat constant over time?? Help joan

1. The quality of the wildfire PM2.5 is not guaranteed with a published peer-reviewed manuscript. Could the author provides more information to assure the quality of their wildfire PM2.5 estimates which is the key to avoid exposure misclassification.

Yes it is – I messed up and somehow Tarik’s citation wasn’t in the submission. This was bad, ooops. We will add it.

TODO: add it

<https://chemrxiv.org/engage/chemrxiv/article-details/6169e9597d3da5ff02f96022>

1. The quality of fire proximity exposure assessment is also not assured. Please clarify what are "final fire perimeters". Those fire perimeters were "final" in relevant to what? How fire activity is defined? For the 20km buffer, have the authors performed any sensitivity analysis on other buffer size? If not, sensitivity analysis is strongly recommended.

Fire activity is logged by CALFIRE in their database where they create and update fire perimeter shapefiles, which are associated with certain dates and represent the area a fire has burned by a certain date. However, CALFIRE does not always record a perimeter every day, so there are sometimes no boundaries or only one boundary for a given fire. There are several recorded boundaries for the Getty fire, and one for the Woolsey fire. We used the last recorded fire perimeters (final fire perimeters) that represented the largest burned area in the CALFIRE dataset.

**We can do sensitivity analyses on the buffer?**

TODO: sensitivity analyses on the buffer, revise this description a bit

1. There appears to be problem with zero-inflated data. However, based on the reviewers understanding, negative binomial model should solve this problem. Please explain why the authors still aggregated to ZCTA groups to solve this issue? Have the authors considered other statistical models (i.e. two-stage negative binomial model by ZCTA, conditional Poisson model)?

**I’m not sure why we did this.**

1. For the proximity and evacuation analysis, the analysis is done on the weekly level, which could be problematic as the study participants would have already left the affect area and use health care in other locations within a week, and this potential movement could lead to biased effect estimates (likely non-differential bias). Have the authors looked at the movement pattern right after a big fire and/or evacuation order. Also, have the authors tried to perform the analysis on the daily level?

We are using KPSC administrative data, which means that even if the study participants left the affected area, and sought care at another KPSC location, they would still be included in our dataset. It is absolutely true, however, that if they did not seek care within the KPSC system, they would not be included in the dataset. They would be motivated to go to Kaiser because they are insured there. We do discuss this in our limitations section. We do not know the movement pattern before big fires or during evacuation orders, and this is an open research question that we’re interested in.

**We could perform the analysis at a daily level, but initially, we decided on the weekly level because we have imprecise data about where the fire boundary is at the daily level. I know we also did this because our visit counts at the daily level were very small. I’m not sure what to say about this.**

1. The author controlled for ZCTA grouping level covariates like income, home ownership, poverty, age structure, etc. Are those covariates varied over time? If they are not, by this study design, those variables should have been controlled automatically.

**We did this to control for spatial correlation. IDK how to explain this cause I’m not sure I understand it myself.**

1. There is concern for temporality (outcome may occur preceding the exposure) for the same week (week 0) wildfire PM2.5 exposure analysis.

**True. Is there a way to deal with this? Should we exclude week 0?**

1. For the results of proximity and evacuation exposures, most of the RRs are null with 95% CI including the null value. Could the author please acknowledge this while reporting and interpreting the results?

Sure.

Minor Comments

1. There appears to be discrepancies between what is reported in the main text vs. the abstract. Could the author clarify where are those RRs in "proximity RR =1.48, 95% CI:1.01, 2.17, evacuation RR = 1.76, 95% CI: 1.02, 3.05)" presented in the main text? To the reviewer, no RR reported in Figure 3 has a upper 95% CI limit that over 3.

Hmm – I think this is just a mistake/typo, I’ll check!

1. Figure 3 is confusing with contradictory x-axis label and figure legends. Please consider create separate panels for proximity vs. evacuation analyses and specific health care usage outcomes.

We tried. I guess two figures is fine and we can redo it that way, if that’s what this person prefers.

1. Reference 38 is not completed.

This is a citation manager issue I’ll fix.

Reviewer #2: Summary:

This paper evaluates whether people who use durable medical equipment have increased inpatient, outpatient, or ED visits related to exposure to PM2.5 during wildfire events or if they live in proximity to the wildfire or its evacuation zone. In general, this is a very interesting take on understanding the health impacts of wildfire by focusing on a group who may be more affected, however, the paper could use some editing to keep the reader clearer about what was done and what the findings are and who was impacted.

One general point throughout the paper is that it should be made clear that this study is only focused on the durable medical equipment (DME) user population and not comparing this group to those who do not use durable medical equipment. We cannot say, from this study, that the DME population is more affected by wildfires but can just say that some, but not all outcomes, were associated with some, but not all, of these exposures among this population. In the results and discussion sections, many of the results are not stated clearly that it is just among the DME population. A reader could take these sentences out of context and infer these findings apply to all people in the study area, which is not the case.

This reviewer comments more about which sentences in particular they’d like changed, so I have documented those revisions below.

Similarly, there are many other places that require more precise language, as noted in the specific points below.

Another main point is that in the introduction and the discussion, I do not think that the authors have done a sufficient review of the literature on the health impacts of wildfire smoke. It would be advised that the authors do some more reading on these topics and/or change the language - again, see specific points related to this below.

I bet this reviewer is Reid or on those papers – they are the ones that tempered their findings on cardiovascular effects!

Specific Points:

Abstract:

Introduction:

Lines 28-33 may be missing a good amount of literature on the stress and mental health impacts of wildfire exposure not just due to smoke. Many studies comment on the fact that some of the pathways by which wildfires affect a given health outcome could be both through stress or air pollution exposure pathways - for example see (Holstius et al. 2012) or (Cohen et al. 2022) or (Murphy et al. 2021). There is a also a recent literature review on the mental health impacts of wildfire that reviewed 60 studies on the mental health impacts of wildfire exposure (To et al. 2021) and many other papers have come out since that review on these topics. For just a few examples, look at the following: (Obuobi-Donkor et al. 2022; Usher et al. 2022).

I can add these citations, and change our intro accordingly.

Methods:

The subsection titled "Study population and outcome data" describes where the data came from, but not much about the people in the dataset nor specifics about the outcomes were collected in those data. The authors state that daily healthcare visits were collected. Were these separated into visiting a physician versus an ED visit?

Going to change this just to ‘study population’, because there is another section called ‘outcome definition’ where we do give all these details. I think this confused both reviewers.

What about if the visit had nothing to do with the person's use of DME?

We looked at a change in the number of visits during wildfires/smoke vs not during wildfires/smoke, so we should be looking at the change in number of visits. The number of visits that have nothing to do with wildfires should not change. We are also not trying to look at differences in wildfire impact between DME users and non-DME users. We are only trying to describe the impact that wildfire exposure has on DME users. Many visits probably do have nothing to do with DME, but I don’t think this is a threat to the validity of our study.

It would be good to know what the outcome is defined as and if any demographic data were collected on the patients.

This is covered in the “outcome definition” section below.

Was the data de-identified before sending to the authors and that is how it was considered not human subjects research? Yes – we can add this.

Lines 54-56 in "Wildfire PM2.5" section- what data were used in the imputation models for the counterfactual PM2.5? Presumably, some monitoring data only? Were monitoring data used in the machine learning models for total PM2.5?

We can refer here to Tarik’s citation.

Lines 56-59 in "Wildfire PM2.5" section: I am a bit confused by this sentence: "We finally estimated the difference between such counterfactual values to observed values during an exposure to wildfire smoke to estimate daily/ZCTA levels of wildfire smoke PM2.5." What do the authors mean by "During an exposure to wildfire smoke"? Were the estimates only made during specific time periods during which the air quality was affected by wildfire smoke?

Yes – can refer again to Tarik’s citation.

Line 59 in "Wildfire PM2.5" section - How did the authors "obtain daily/ZCTA levels of non-wildfire PM2.5"? Is this from their imputation models or did someone else provide it and thus it was obtained?

This was from the imputation models – we can again refer to Tarik’s citation.

Lines 4-6 in in "Wildfire PM2.5" section (on next page): I don't see an explanation of the "higher-level ZCTA groupings" in the outcome definition section (presuming that this is referring to the section titled "Study population and outcome data".

Now that I see the Outcome Definition section, I suggest renaming the first section so that it does not refer to outcome data and putting the outcome definition before the exposure section so that the reader understands what is meant by 'ZCTA groupings" when first mentioned.

When we mention these, we say ‘see the outcome definition section below for a description of what these are’. The next time we mention them is to define them. There is a “study population and outcome data” section *above,* which is where this reviewer did not find the desired explanation, but below there is an ‘outcome definition’ section with the descriptions. We will happily rename the above section to just ‘study population’ – I think this also confused the first reviewer, so it’s a good point. However, I personally think that restructuring in the way this reviewer is suggesting would disrupt the flow of the paper? **Joan what do you think?**

Suggestion to mention the Thomas Fire in the study period explanation just before the methods section. Additionally, if "most" of the Thomas fire burned outside of the study area, that implies that "some" of the fire was inside the study area and thus some people in the study population may have been within a region to be considered exposed. By excluding this exposure, those people's exposure would be misclassified.

We accounted for this by excluding ZCTAs that were exposed to any other fires during the study period, including the Thomas Fire. See the comment below about the Thomas Fire.

I looked at the three cited papers for the 20km threshold of affected by a fire and didn't see any of them mention 20km. Please clarify how this distance was chosen.

What we wrote was this: “We hypothesized that living within 20km of a fire perimeter could elicit a stress response, similar to effects described in previous studies.7,8,41”

We have no particular reason for choosing 20km beyond the fact that it is ‘close’. We know that being close to a fire and being in the actual burned area are stressful, and no study we know of has evaluated how that stress changes quantitatively with distance. These studies all show that being close to a fire is stressful, and don’t mention 20 km.

**We can revise this statement to: We hypothesized that living within 20km of a fire perimeter could elicit a stress response, since stress responses have been described in previous studies at various distances from wildfires.7,8,41**

This sentence, "The DID estimators subtracted the change in visit frequency during a fire among control ZCTAs (difference 1) from the change in visit frequency during a fire among ZCTAs exposed to the fire or evacuation zone (difference 2)." could be stated more clearly, especially for someone who is unfamiliar with DID. The first thing that could be stated clearer is how control ZCTAs are defined (this could go into the section on assigning exposure to wildfire or evacuation) and stating that the "DID estimators subtracted the change in visit frequency during a fire compared to when there was not a fire (difference 1)" and the same for difference 2. Additionally, the authors need to explain how the time periods of "not a fire" were defined, if indeed, I am interpreting what was done correctly. If I was not, then this should be revised to be clearer about the difference.

Can revise.

When the authors say that they performed 20 regression analyses in this section due to evaluating each relationship separately for each fire and each type of healthcare visit, I would presume that they also evaluated them separately for exposure to the fire as well as exposure to evacuation orders, given that these were two separate exposures. Is this correct? If so, please revise that to be clear.

Can revise – the reviewer’s assumption is correct.

Additionally, please clarify if there were two of three fires assessed as the paper in some places only refers to the Getty and Woolsey fires and in other places also refers to the Thomas fire.

We only mention the Thomas Fire once, because it was a huge fire that burned during our study period, and we wanted to explain why we did not choose to include it in our analysis. We do not mention it again, and the only mention of it in this excerpt:

*“Notably, The Thomas Fire also burned over 1100 km2 during our study period.38 However, most of the fire burned in the rural northern corner of Ventura County and outside the study area. Therefore, we did not include the Thomas Fire in the proximity analyses. Still, smoke from this fire contributed significantly to wildfire PM2.5 in Ventura County in December 2017 (Figure 2).”*

Lines 38-39 in the "proximity to wildfire and evacuation" section - when the authors say that they excluded all ZCRAs exposure to other large fires, is this just other large fires within the study period or all large fires ever?

During the study period – we can add that to be clear.

Additionally, this sentence, "Therefore, we excluded observations from these ZCTAs made during and after fire exposures." is unclear - during and after which fire exposures?

Personally I think this is pretty clear when you read it in context, but I can put a revised version here.

Given the study area has been exposed to fires and evacuations over many decades, it would prove hard to ensure that control ZCTAs had never in the tenancy of people living in those ZCTAs been exposed to a fire or an evacuation zone.

Yes, this is why we only excluded ZCTAs that were exposed during the study period.

And in the last sentence of this paragraph, the fire ignition date of which fire?

From each > 500 sq km fire ignition date. Will revise.

Results:

Line 28 of "PM2.5 exposure" section - what USEPA limit are the authors referring to? (Presuming the daily PM2.5 NAAQS standard (which is not technically a "limit"), but this should be clarified).

Yes.

For all results, to be clearer, the authors should state the population being studied. For example, I suggest adding "among DME users" after "outpatient visits" to the sentence "In adjusted negative binomial models, a daily 10 /3 increase in wildfire PM2.5 was associated with a decrease in risk of outpatient visits one day later (RR = 0.96, 95% CI: 0.94, 0.99),". The reason for this suggestion is so that someone doesn't read this sentence out of context and think that all outpatient visits declined in the study area on days with higher wildfire PM2.5 concentration. Similarly, the term "among DME users" should be added to the second sentence in this paragraph.

Yes no problem – thank you for suggesting which sentences in particular, that makes it really easy.

It is also interesting that the authors decided to highlight in the writing the one day (lag 1) with a protective effect for wildfire PM2.5 when on lag days 2, 3, 5, and 6, they found a significant adverse effect (and a null finding on lag days 0 and 4). In my interpretation of Table 1a, there are more significant effects for all-cause outpatient visits to increase due to wildfire PM2.5 than decrease and additionally, a distributed lag estimate across all of these days would be informative, rather than just each lag day separately.

We did this in the weekly analysis and we commented on it briefly.

Lines 43-45 of the "PM2.5 exposure" section should state that "weekly wildfire PM2.5" was not associated with frequency of other visits (emphasis added).

OK.

Throughout, I would suggest not referring to the EPA PM2.5 NAAQS as a "recommended daily limit" because the NAAQS is more of a regulatory standard than it is a recommended limit. Although it is supposed to be based on health, we know there are health impacts below that threshold. Additionally, with the NAAQS, areas are allowed to exceed the NAAQS on 2 percent of days, essentially. This is a minor point, but I think helps to keep what the EPA threshold values are through the NAAQS clear to a reader. The WHO Air Quality guidelines are more of a "recommended" daily and annual limit as they are not regulatory in nature and are recommendations that have no 'teeth' to them and are more in-line with what is known about health impacts of PM2.5.

Sounds smart.

Figure 3 - the title should be clarified - the change in frequency of visits during a fire compared to what?

OK.

Figure 3 - the two orangey/yellow colors are hard to discern. Given that these outcomes are not a scale related to each other, there isn't really a need to have a color scheme that increases from one shade to another. I would suggest a color scheme where every color is very different from each other or a vertical axis where each healthcare visit type is written.

Got contradictory advice on this – I’ll just do what this reviewer says.

Lines 7-8 in the "Woolsey Fire proximity and evacuation exposure" section - the outpatient visits remained the same during the fire compared to what? The same goes for the next sentence - compared to what? And "outpatient visits" should be "all-cause outpatient visits".

Sure.

The paragraphs about the increase in visits during the Woolsey Fire need to be prefaced with the confidence intervals, as the cardiorespiratory emergency and cardiorespiratory inpatient are really null based on these confidence intervals.

Ok.

I suggest rewording the findings for the Getty Fire. Someone who is not an epidemiologist would read this as all of these visits increased during the fire and miss the nuance that there was an increase everywhere and therefore it was not associated with proximity the fire or being in the evacuation zone and that all of those findings were null.

Ok.

Discussion:

I would change the first sentence of the discussion to have a "but" instead of an "and". It is very interesting that next day visits decreased but then for almost all other days that week and the following week there were increased visits.

Ok.

The first paragraph of the discussion should mention that proximity and evacuation during the Getty Fire were not associated with any visits among this population group.

Yes.

Second paragraph of the discussion: I disagree that there is a strong relationship between wildfire smoke exposure and cardiorespiratory health in the literature. There is a strong relationship for respiratory health impacts of wildfire smoke and growing, but mixed, evidence of impacts of wildfire smoke on cardiovascular health.

There are some studies that have found that wildfire PM2.5 affects people who are not using rescue medication usage for asthma more than those who are - see a discussion of this hypothesis in (Reid et al. 2016) and (Lipner et al. 2019).

Sure can revise.

I find the statement "These findings are consistent with much of the literature" to be strange, and in my opinion, not true. I do not know of any study that has found significant impacts of wildfire PM2.5 on health outcomes beyond the first week and the paragraph that this is part of mentions that very few studies have even investigated whether wildfire PM2.5 is associated with outpatient visits at all (which I agree is the case).

Can revise.

In the limitation section about using the Kaiser data - I wonder whether people, due to evacuation, may have been farther from the clinic they normally use and may have gone elsewhere?

We are using KPSC administrative data, which means that even if the study participants left the affected area, and sought care at another KPSC location, they would still be included in our dataset. It is absolutely true, however, that if they did not seek care within the KPSC system, they would not be included in the dataset. They would be motivated to go to Kaiser because they are insured there. We do discuss this, but we can make it more clear that other Kaiser clinics would have been included in our data.

Additionally, the authors should mention that this population is likely different from the non-Kaiser population in significant ways such as they all have health insurance likely through their employer.

**Yeah I mean some of these people have Medicaid, right? Joan is there a citation we can use that evaluates Kaiser’s representativeness?**

Can the authors defend the statement that "all visits were infrequent during the study period"? Were the visits more infrequent than normal?

I just meant that a lot of days were 0 visits. Will revise.

Related to this, was an assessment of power calculated to defend the statement that the models may have been underpowered. This is a big deal if the analyses were underpowered to even have detected an association. For the statement about the differences in findings by proximity and evacuation to the Woolsey Fire, wasn't it true that evacuation zones are essentially subsets of proximity areas? Thus, it is not surprising that the confidence interval was larger for the evacuation zone.

**No power analyses…. And the last part is true. Not sure what to say here.**

The authors should say more about whether they think that adjustment for the ZCTA-level SES variables sufficiently dealt with spatial confounding. If the authors are truly concerned about spatial confounding, they should assess for spatial autocorrelation in the residuals of their models and if they find some, they should then run a spatial error model.

**Maybe we can chat about this?**

The paper needs a better concluding paragraph and not end on a statement about a limitation.

Will add.

References:

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Obuobi-Donkor G, Eboreime E, Shalaby R, Agyapong B, Oluwasina F, Adu M, et al. 2022. Evaluating the Prevalence and Predictors of Moderate to Severe Depression in Fort McMurray, Canada during the COVID-19 Pandemic. Int J Environ Res Public Health 19:7090; doi:10.3390/ijerph19127090.

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Usher K, Durkin J, Douglas L, Coffey Y, Bhullar N. 2022. Coping styles and mental health outcomes of community members affected by black summer 2019-20 bushfires in Australia. Int J Ment Health Nurs; doi:10.1111/inm.13035.

\* \* \* \* \*

Preparing a revision

1. For estimates of causal effects, we strongly discourage the use of categorized P-values and language referring to statistical significance, including whether a confidence interval covers the null. We prefer instead interval estimation, which conveys the precision of the estimate with respect to sampling variability. We are more open to testing with respect to modeling decisions, such as for tests of interaction and for tests for trend.

2. We do not permit acronyms unless they are generally recognized by epidemiologists (e.g. HIV is okay, but LVA is not). When in doubt, we recommend that you spell out.

3. Please do not include uninformative precision (excessive decimal places). For example, percents should be rounded to nn%, n.n%, or 0.0n% and risk ratios should be rounded to nn, n.n, or 0.nn unless clarity of the presentation and the sample size justify more significant digits.

4. Please be sure to include explicit information about approval of human subjects research by an independent review board. If no such review was required, include an explicit statement about why the requirement for review was waived.

5. Do not include public health policy recommendations in Brief Reports or Original Articles that present new research findings.

6. Data appearing in the abstract must also be cited in the main text, not just in tables or figures.

7. Resubmissions must adhere to word limits. The word limits for main text (generally the introduction, methods, results, and discussion) are 1500 words for Brief Reports (plus 150 words for its abstract), 4000 words for Original Articles (plus 250 words for its abstract), 5000 words for reviews (plus 250 words for its abstract), 2000 words for Commentaries (no abstract), 600 words for Research Letters (no abstract), and 400 words for Letters to the Editor (no abstract).

8. We advise that total word counts for Original Articles should not exceed 7500 words and for Brief Reports should not exceed 3500 words. The total word count includes main text (introduction, methods, results, and discussion), bibliography, figure legends, tables, and figures (250 words per figure, including each figure in a panel). The title page, abstract, acknowledgments, and funding information do not count in the total word count.

9. Figure labels: Make font size as large as possible, so as to be legible when figures are reduced for publication (typically one column [8.5cm] in width).

10. Footnotes to tables and figures should use superscript lowercase letters to link content to the footnote, not symbols or numerals.

11. Do not use parenthetical phrases like “(data not shown), (results not shown), or (available from the authors upon request).” In these circumstances, the data or results should be provided in Supplementary Digital Content.

12. Additional details regarding submission requirements can be found in the Instructions for Authors, which are posted at http://edmgr.ovid.com/epid/accounts/ifauth.htm .

Preparing for resubmission

13. Prepare a response document for the Editor that responds point-by-point to the reviewers' comments (presenting each comment followed by your response). Give the page number where revised text can be found and, where practical, paste revised text directly into the reply document.

14. Submit versions of the manuscript with and without your changes displayed.

15. Supplementary Digital Content should be submitted as a single PDF file, and you should use our convention - e.g. eFigure 1, eAppendix 2 - to label and refer to online content.

16. Authors should submit copies of any closely related manuscripts (published, in press, or under review).

17. Please revisit information about page charges and color printing charges available in the Instructions for Authors, which are posted at http://edmgr.ovid.com/epid/accounts/ifauth.htm .

18. We request that the complete revised manuscript (with all tables and figures) be completed by 23 Nov 2022. If you are not able to meet this deadline, please notify the editorial office.

Resubmitting via Editorial Manager