We sincerely thank the reviewers for reading our paper and for their comments. We found them very helpful, and we believe that they will improve our paper and make it clearer.

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 assess the effects of wildfire exposure on people whose durable medical equipment (DME) use might indicate that they were medically or socially vulnerable. 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 counts from our partner Kaiser Permanente also include people who rent breast pumps. Renting a breast pump likely does not indicate vulnerability and may in fact indicate a certain level of health. Excluding DME using people under 45 excludes nearly all healthy people renting breast pumps, and leaves those renting other types of DME. We characterized this population in detail in Casey et al. 2021, and so there is more information in that paper.

Unfortunately, we did not have access to demographic data that would have allowed a modification analysis. This would be a great area for future research.

We added additional sentences to the description of the study population:

*“We excluded younger DME renters in order to focus on socially and medically vulnerable older adults, but also to exclude breast pump users, a healthy subgroup of the otherwise vulnerable DME using population, who we did not hypothesize to be particularly vulnerable to wildfire exposure. Electronic health record data included each patient’s Zip Code Tabulation Area (ZCTA) of residence. We obtained daily counts of healthcare visits–not necessarily related to DME use –by this population by residential ZCTA in seven counties in Southern California from January 1st, 2016 to March 15th, 2020.”*

*Page 3 of the main text, under “Study population”.*

We also added revised our limitations section describing the absence of demographic data:

*Second, we lacked individual-level information on participants. Therefore, we did not know if patients sought care for DME-related issues and only used prior DME use as a vulnerability metric. We also were not able to assess differences in healthcare use by type of DME or stratify by age group or sex beyond limiting our study population to those age 45 or older. Excluding younger people excluded most breast pump users, a generally healthy subpopulation who constitute 30% of DME users of all ages at KPSC33. Subgroups such as those using ventilators or those using breast pumps likely have vastly different health needs and outcomes. We chose to focus on DME users aged 45 and older who were likely the most susceptible to wildfire. However, subgroups in our study may also have differing needs and outcomes, which we did not examine.*

*Page 11 of main text, under “Discussion”.*

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.

Unfortunately, as mentioned above, resource limitations precluded us from accessing detailed demographic variables from Kaiser Permanente. We did describe the larger DME population in Casey et al. 2021. Our current study population represents the subset of 45+ DME users who had a healthcare visit during the study period.

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 describe here why DME users may be vulnerable – in short, because they are more likely to have health conditions that may be exacerbated by wildfire exposure, may have less mobility than people who don’t use DME, and may be older than people who don’t use DME (a risk factor in itself).

*People who use durable medical equipment may be particularly vulnerable to both wildfire PM2.5 exposure and stress from wildfire proximity or evacuation. DME use is common among older adults and is associated with respiratory illness and disabilities.29 A prior study among Kaiser Permanente Southern California (KPSC) members found increasing prevalence of DME rentals from 2008-2018 and the highest prevalence of use among older adults.30 DME types included bilevel positive airway pressure (BiPAP) machines, enteral feeding machines, infusion pumps, oxygen equipment, suction pumps, ventilators, and wheelchairs.30*

*This group may face unique challenges during wildfire events. Prior studies have found elevated effect estimates between wildfire smoke exposure and respiratory and cardiovascular disease outcomes among older adults compared to younger populations.16,31 Further, people using DME may have co-occurring medical conditions such as cardiovascular disease that make them more vulnerable to both the effects of wildfire PM2.5 and wildfire-related stressors beyond wildfire smoke (e.g., threatened or actual evacuation). Limited mobility or need for electricity access may result in increased difficulty evacuating disaster zones.30,32*

*Page 2 of main text, in “Introduction”.*

Casey et al., 2021 studied the DME population, and found that the average length of DME rental in the population from which our study population was drawn was a year (Casey et al 2021). We aren’t sure that people were renting DME at the time of their healthcare visit, but we are using DME rental as a proxy for potential vulnerability, and we therefore think that even if someone wasn’t using DME at the time of exposure, we would still be capturing people who are potentially vulnerable.

To summarize, we selected this population because we anticipate that people using DME are more vulnerable to wildfire exposures than people who don’t use DME. Our research question was focused on healthcare visits made by DME users, regardless of whether visits were caused by or associated with DME use. We hypothesize that recent DME use may be an indicator of social and medical vulnerability. We think DME may indicate vulnerability because DME is usually rented to address a disability or medical issue and is associated with some other indicators of social and medical vulnerability, such as older age and Medicaid insurance (Casey et al, 2021).

We also added information regarding DME use at the time of the exposure to the limitations section: *“First, we only had access to data on visits to Kaiser Permanente clinics and hospitals made by Kaiser members who rented DME in the year prior to October 29, 2019, which we used as an indicator of vulnerability.”*

*Discussion, page 9.*

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.

As requested by the reviewer, we included the following information regarding the performance of the wildfire smoke PM2.5 prediction, to the exposure definition of the methods section: “*This ensemble model achieved high accuracy with R2 of 0.86 and RMSE of 3.48 (see details in Aguilera et al.)*.”

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.

All California wildfire activity is logged by CALFIRE in their database, where they create and update geographic fire perimeter files. They timestamp these fire perimeters, so there may be several perimeters associated with a fire that lasted several weeks. Usually, fires expand and the perimeters get progressively bigger.

However, CALFIRE does not always record a perimeter every day, so there are instances when no boundaries or only one boundary is available 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, which we called final fire perimeters, which represented the largest burned area in the CALFIRE dataset.

We have revised the description to make it clearer:

*We obtained shapefiles of the total areas burned during the Getty and Woolsey fires from the CALFIRE Fire and Resource Assessment Program.39 These perimeters represented the approximately the maximum burned areas of each fire40 and we used them to define exposure. We considered ZCTAs exposed if they were within 20km of a final fire perimeter on days that a fire was active. 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*

*Page 4 of the main text, under ‘Proximity to wildfire’.*

Per your suggestion in the comment, we performed a sensitivity analysis on the buffer size. We changed the buffer size to 30 km around both the evacuation and fire zones, expanding the exposed zone to include people further away from the fire or evacuation boundary. In both cases, the estimates did not change significantly. The tables containing original RR estimates and CIs and sensitivity analyses are in the supplementary digital content resubmission. We thank you for this feedback and feel that the sensitivity analyses have added credibility to our manuscript.

We added a sentence describing this in our methods:

*We also tested all models for sensitivity to the size of the buffer around the wildfire perimeters and evacuation zones, by re-running analyses with a 30km buffer instead of a 20km buffer, expanding the exposed zone to include people further away from the fire or evacuation boundary.*

*Page 6, under Proximity to wildfire and evacuation.*

We also added a sentence in our results describing the outcome:

*None of our results were sensitive to spline flexibility, or the size of the buffer around exposures.*

*Page 8, under Results.*

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)?

Though the negative binomial model may have still produced relatively unbiased estimates of association with reasonable confidence intervals when run with zero-inflated data, because most of the observed daily visit counts in all categories were 0, we wanted to increase our power and get closer to meeting negative binomial distributional assumptions by aggregating to the weekly level. We would have aggregated visit counts to the weekly level in both the proximity and PM 2.5 analyses, but we had reason to believe (from the literature on PM2.5) that there may be lagged effects of PM 2.5 exposure only detectable at the daily level, so we chose to aggregate to larger spatial groupings instead, to keep measurements at the daily level. We have edited the manuscript to reflect the importance of statistical power in our decision:

*Daily visit counts by ZCTA were low and often zero (median outpatient visits = 1, IQR = 3, median ED and inpatient visits = 0, IQR = 0). For the wildfire PM2.5 analyses, to avoid zero-inflation in our models, and to increase statistical power, we could have aggregated ZCTA counts to the weekly level. However, prior studies of wildfire smoke exposure have found associations between same-day air pollution and healthcare visits over the course of the following week.18–21 To evaluate a lagged effect in our data, we required daily healthcare visit counts, therefore, we opted to aggregate our data into higher-level spatial groupings of several ZCTAs based on spatial proximity (hereafter ‘ZCTA groupings’; grouping method described in the eAppendix).*

*Outcome definition, Paragraph 1, page 5.*

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. However, participants would be motivated to access care at a KPSC location because they are insured there. We discuss this in the limitations section:

*First, we only had access to data on visits to Kaiser Permanente clinics and hospitals made by Kaiser members using DME. These patients would be highly motivated to seek care at Kaiser, given their membership status, however they may have sought urgent care at other clinics or hospitals. Such alternate utilization would have produced artificially reduced visit counts, especially for inpatient and emergency visits. If patients sought care at other clinics only during wildfires (whether during evacuations or while a fire was burning nearby) this could have biased association estimates towards the null.*

Additionally, we consider evacuation to be part of our exposure/an effect modifier. We have aimed to capture whether evacuating changed participants’ likelihood of seeking either routine or emergency care.

We agree movement patterns before and during wildfire events would be of great interest scientifically, however, we do not have access to such data. This is an open research question and has been explored in the literature. [[1]](#footnote-1)

We could have performed these analyses at the daily level, which would have prevented this problem, but we opted to analyze at the weekly level for three reasons. First, though the negative binomial models we are using are reasonable when distributional assumptions are violated and there is zero-inflation, aggregating our visit counts still increases our power to detect associations, since most of our daily visit counts were 0. Second, we do not have detailed information about where fire boundaries were located at the daily level – for the two-week Woolsey Fire, only one fire boundary was reported in CALFIRE. We could not capitalize on detailed exposure information about who was close to either fire at the daily level. Third, we had no reason to believe from the existing literature that there would be lagged effects that we could only disentangle at the daily level.

We have edited the manuscript to reflect this reasoning:

*For proximity and evacuation analyses, we used ZCTA level daily visit counts aggregated to the weekly level. We aggregated to the weekly level because we used last recorded fire boundaries and last recorded evacuation zones rather than daily PM2.5 concentrations as we had available for our air pollution analyses. By aggregating, we also removed weekend-weekday patterns in outpatient visits, increased power, and reduced zero inflation. We considered a week exposed if the Woolsey or Getty Fire burned any day that week.*

*Page 5, ‘Outcome definition’.*

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 agree with the reviewer that these non-time-varying covariates are controlled for by design. We include them because they can increase the precision of association estimates in the absence of a random intercept controlling for spatial unit, which we couldn’t include because of computational limitations.

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

Yes – thank you for this comment. We have removed the reporting and interpretation of week 0 due to this potential issue with temporality.

*In our additional analysis examining weekly wildfire PM2.5 levels lagged up to two weeks, a 10 increase in weekly PM2.5 concentration was associated with a next-week increase in outpatient visits (RR = 1.04, 95% CI: 1.00, 1.09), consistent with the daily outpatient visit model. Additionally, there were increases in weekly outpatient visits two weeks later (Table 1b). We did not interpret the same-week coefficient due to issues with temporality – our outcome may have preceded the exposure. Weekly wildfire PM2.5 was not associated with the frequency of any other visits.*

*Results, PM analyses, page 7.*

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?

In response to the reviewer’s request, we have made some edits to the text of the manuscript to highlight the wide confidence intervals. However, we respectfully disagree with the reviewer’s request to note that the CIs include the null value. Abundant literature suggests that an arbitrary cut-off (i.e., P<0.05) for interpretation as significant is a disservice to science. For example, see <https://www.nature.com/articles/d41586-019-00857-9>. The Journal Epidemiology itself “strongly discourage[s] the use of categorized P-values and language referring to statistical significance.”

The journal’s submission guidelines say:

***“Significance Testing:*** For estimates of causal effects, we strongly discourage the use of categorized P-values and language referring to statistical significance (see discussion of this [topic](http://journals.lww.com/epidem/pages/collectiondetails.aspx?TopicalCollectionId=4)). 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 ([see editorial](http://journals.lww.com/epidem/Fulltext/2009/03000/Interaction_Reaction.1.aspx)) and for tests for trend, and with respect to studies using high-dimensional testing, such as genome-wide association or other genomic platforms.”

### Our edits:

### *Woolsey Fire proximity and evacuation exposure*

### 

### *Residence within 20km of the Woolsey Fire boundary during the fire was associated with increased inpatient admissions for cardiorespiratory disease compared residence outside of it (RR = 1.48, 95% CI: 1.01, 2.17), and associated with decreased all-cause outpatient visits compared to residence outside of it (RR = 0.89, 95% CI: 0.79, 1.00), though the confidence interval was wide (Figure 3). We observed similar associations, with narrower confidence intervals, between Woolsey Fire evacuation exposure and healthcare visits. Residence in versus outside of an evacuation zone plus 10km of the Woolsey Fire during the fire was also associated with decreased all-cause outpatient and increased inpatient admissions for cardiorespiratory disease visits (RR = 1.76, 95% CI: 1.02, 3.05, RR = 0.87, 95% CI: 0.73, 1.04 respectively) (Figure 3).*

### *Getty Fire proximity and evacuation exposure*

*We observed reduced risks of all visits types among proximity exposed ZCTAs during the Getty Fire, however, confidence intervals were very wide (Figure 3). We observed similar, if somewhat attenuated, associations among evacuation exposed ZCTAs.*

*Page 8.*

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.

The RRs reported in the abstract were incorrect and from a previous iteration of analyses. We apologize for this error, and we’ve corrected the RRs. Thank you for pointing this out!

From abstract:

*Woolsey Fire proximity (<20km) was associated with reduced all-cause outpatient visits, while evacuation and proximity were associated with increased inpatient cardiorespiratory visits (proximity RR = 1.45, 95% CI: 0.99, 2.12, evacuation RR = 1.72, 95% CI: 1.00, 2.96).*

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 have changed this figure, and separated it into four panels instead of two, with different panels for proximity and evacuation as you suggest. We appreciate this feedback and find the figure better now.

1. Reference 38 is not completed.

Thank you for pointing out this mistake. This was a citation manager issue. We have fixed it by completing the reference.

Thank you again for your comments!

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.

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.

We address these specific points below.

Specific Points:

Introduction:

1. 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).

We have reviewed the literature more carefully and have revised our introduction and included these citations and a few others. The updated paragraph is:

*Several studies have documented the effects of stress, evacuation, property destruction, or injury due to wildfire disasters7,8,21,25,27–31. We seek to expand on this literature. We hypothesize that residential proximity to wildfire, and evacuations due to wildfire, could influence health outcomes primarily through stress. This pathway may operate differently than wildfire PM2.5 exposure. Residents living near active wildfires are exposed to smoke, which in addition to biological effects can cause substantial worry80. Proximate residents also experience the disruption of usual activities in their communities, and face the threat of injury, evacuation, or longer-term displacement. Evacuation may cause more severe stress as these threats materialize. Evacuation, however, may also disrupt access to healthcare and could result in a temporary reduction in utilization.81*

*Intro, paragraph 4.*

Methods:

1. 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?

We confused both reviewers with the naming of this section, so it must have been confusing! We have changed the name of the section ‘study population and outcome data’ to just ‘study population’. There is another section called ‘outcome definition’ where we provide the requested details:

*We obtained daily ZCTA-level counts of all-cause outpatient visits, all-cause inpatient admissions, and all-cause emergency department (ED) visits, as well as inpatient admissions and ED visits specifically for circulatory or respiratory disease outcomes made by KPSC members 45 and older who rented DME. Causes were identified using International Classification of Diseases 10 codes I00-I99 (circulatory) and J00-J99 (respiratory). We included visits from January 1st, 2016 to March 15th, 2020.*

Page 5, ‘Outcome definition’.

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

We appreciate the opportunity to clarify why we opted to study DME users in the present analysis. We selected the study population because we anticipate that people using DME are more vulnerable to wildfire exposures than people who do not use DME. While of interest, we were not trying to evaluate whether healthcare visits made by DME users were for the functioning of DME, or were made for conditions associated with DME. Rather, we hypothesized that DME may be an indicator of social and medical vulnerability. We think DME may indicate vulnerability because DME is usually rented to address a disability or medical issue (Casey et al, 2021).

Visits in our paper may or may not be for DME use. We have added specific language to make this clear in the manuscript:

*We used electronic health record data from KPSC to identify all individuals who were 45 or older as of October 28th, 2019 and had rented DME in the year prior. We excluded younger DME renters in order to focus on socially and medically vulnerable older adults, but also to exclude breast pump users, a healthy subgroup of the otherwise vulnerable DME using population, who we did not hypothesize to be particularly vulnerable to wildfire exposure. Electronic health record data included each patient’s Zip Code Tabulation Area (ZCTA) of residence. We obtained daily counts of healthcare visits–not necessarily related to DME use –by this population by residential ZCTA in seven counties in Southern California from January 1st, 2016 to March 15th, 2020. 236,732 DME patients lived in the study area, which covered most of San Bernardino, Orange, Los Angeles, Riverside, San Diego, Ventura, and Kern counties (Figure 1). The area consisted of 582 ZCTAs, each containing 1-1773 patients. During 2018 and 2019, these seven counties experienced 23 wildfires that each burned over 3 km2 in California,34,36 contributing to wildfire smoke in the area.*

*Page 3, Methods, study population.*

We also added a sentence “*These visits were not necessarily related to DME use*.” to the Outcome Definition section on page 5.

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

The outcome definition is covered in the “outcome definition” section below, quoted here:

(This may have been unclear due to our confusing naming of the sections, which we have now corrected.)

## **Outcome Definition**

*We obtained daily counts of all-cause outpatient visits, all-cause inpatient admissions, and all-cause emergency department (ED) visits, as well as inpatient admissions and ED visits specifically for circulatory or respiratory disease outcomes made by KPSC members 45 and older who rented DME. These visits were not necessarily related to DME use. We identified cause-specific visit counts using International Classification of Diseases 10 codes I00-I99 (circulatory) and J00-J99 (respiratory). We included visits from January 1st, 2016 to March 15th, 2020.*

*Daily visit counts by ZCTA were low and often zero (median outpatient visits = 1, IQR = 3, median ED and inpatient visits = 0, IQR = 0). For the wildfire PM2.5 analyses, to avoid zero-inflation in our models, and to increase statistical power, we could have aggregated ZCTA counts to the weekly level. However, prior studies of wildfire smoke exposure have found associations between same-day air pollution and healthcare visits over the course of the following week.18–21 To evaluate a lagged effect in our data, we required daily healthcare visit counts, therefore, we opted to aggregate our data into higher-level spatial groupings of several ZCTAs based on spatial proximity (hereafter ‘ZCTA groupings’; grouping method described in the eAppendix).*

*For proximity and evacuation analyses, we used ZCTA level daily visit counts aggregated to the weekly level. We aggregated to the weekly level because we used last recorded fire boundaries and last recorded evacuation zones rather than daily PM2.5 concentrations as we had available for our air pollution analyses. By aggregating, we also removed weekend-weekday patterns in outpatient visits, increased power, and reduced zero inflation. We considered a week exposed if the Woolsey or Getty fire burned any day that week.*

*Page 5 of main text.*

Regarding demographic data, unfortunately, we were not able to obtain individual-level demographic data due to resource limitations, but we do describe the population from which the present study population is drawn in detail in Casey et al 2021 published in Epidemiology. We have added this to the limitations section:

*Second, we lacked individual-level information on participants. Therefore, we did not know if patients sought care for DME-related issues and only used prior DME use as a vulnerability metric. We also were not able to assess differences in healthcare use by type of DME or stratify by age group or sex beyond limiting our study population to those age 45 or older. Excluding younger people excluded most breast pump users, a generally healthy subpopulation who constitute 30% of DME users of all ages at KPSC33. Subgroups such as those using ventilators or those using breast pumps likely have vastly different health needs and outcomes. We chose to focus on DME users aged 45 and older who were likely the most susceptible to wildfire. However, subgroups in our study may also have differing needs and outcomes, which we did not examine.*

1. Was the data de-identified before sending to the authors and that is how it was considered not human subjects research?

Yes – thank you for pointing this out, we have added this fact to the main text.

*The KPSC Institutional Review Board (IRB) approved this study, and the Columbia IRB did not consider it human subjects research, since the data were fully de-identified before researchers at Columbia received them.*

*Page 3, Methods, Study population.*

1. 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?

Thank you for this comment. For the machine learning models for total PM2.5, we indeed used PM2.5 measurements from US EPA Air Quality System (AQS) along with multiple predictors such as satellite-derived aerosol optical depth and meteorological variables such as temperature and wind patterns. For the imputation models, after identifying ZCTA/days exposed to wildfire smoke (using the NOAA Hazard Mapping System), we relied on PM2.5 from surrounding days -before and after the smoke event- and ZCTA. We revised this section in the methods accordingly.

*We measured wildfire smoke exposure by estimating daily wildfire and non-wildfire PM2.5 concentrations at the ZCTA level using a multistage approach described elsewhere40. Briefly, we first estimated daily levels of PM2.5 (from any source) at the ZCTA level using a validated ensemble model combining multiple machine learning algorithms (e.g., random forest, gradient boosting) and multiple predictors (e.g. PM2.5 measurements from US EPA Air Quality System (AQS), meteorological factors such as temperature, precipitation or wind patterns, satellite-derived aerosol optical depth or land-use variables). We identified smoke-plume exposed ZCTA codes/days with the National Oceanic and Atmospheric Administration’s (NOAA) Hazard Mapping System (HMS) using a smoke binary variable by intersecting ZCTA polygons with smoke polygons. We then estimated the counterfactual PM2.5 values in the absence of wildfire smoke using spatio-temporal imputation models (relying on estimated PM2.5 on surrounding days and ZCTA). We finally estimated the difference between such counterfactual values (i.e. PM2.5 that would have been observed in the absence of the smoke event on a given ZCTA/day) to observed values of PM2.5 during an exposure to wildfire smoke. This difference between counterfactual values and observed estimated during a smoke event on a given ZCTA/day can thus be interpreted as daily/ZCTA levels of wildfire smoke PM2.5. Said differently, after identifying ZCTA/days exposed to wildfire smoke (yes/no, using HMS products), we imputed the level of PM2.5 that would have been observed in the absence of the smoke event on a given ZCTA/day and then compared this -counterfactual- value to what has been actually observed in such ZCTA/days to obtain wildfire smoke PM2.5. This ensemble model achieved high accuracy with R2 of 0.86 and RMSE of 3.48 (see details in Aguilera et al.).*

*We calculated daily wildfire and non-wildfire PM2.5 by averaging concentrations across the higher-level spatial groupings of several ZCTAs based on spatial proximity (hereafter ‘ZCTA groupings’; grouping method described in the eAppendix).*

*Page 4.*

1. 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?

We rephrased this section as follows: “Said differently, after identifying ZCTA/days exposed to wildfire smoke (yes/no, using HMS products), we imputed the level of PM2.5 that would have been observed in the absence of the smoke event on a given ZCTA/day and then compared this -counterfactual- value to what has been actually observed in such ZCTA/days to obtain wildfire smoke PM2.5.” (page 4, Wildfire PM 2.5). We hope this clarifies this approach.

1. 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’ve added that to the text:

*We finally estimated the difference between such counterfactual values (i.e. PM2.5 that would have been observed in the absence of the smoke event on a given ZCTA/day) to observed values of PM2.5 during an exposure to wildfire smoke. This difference between counterfactual values and observed estimated during a smoke event on a given ZCTA/day can thus be interpreted as daily/ZCTA levels of wildfire smoke PM2.5.*

1. 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.

We have renamed the ‘study population and outcome data’ section to just ‘study population’.

We have moved the text describing ZCTA groupings from the outcome section into the exposure section. We prefer to keep the current order of the sections but hope this edit alleviates the reviewer’s concern. The last sentence of the wildfire PM exposure section now reads: “We calculated daily wildfire and non-wildfire PM2.5 by averaging concentrations across the higher-level spatial groupings of several ZCTAs based on spatial proximity (hereafter ‘ZCTA groupings’; grouping method described in the eAppendix)” (Page 4).

1. 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 have revised the text to more accurately describe how the Thomas Fire is involved. We 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 proximity/evacuation analysis. We do not mention it again, and the only mention of it in this (now-revised) 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, since very few participants would have been exposed to it.*** *Still, smoke from this fire contributed substantially to wildfire PM2.5 in Ventura County in December 2017, and therefore was included in our PM2.5 analyses (Figure 2).” (Page 4).*

We have also added text detailing our selection of control ZCTAs:

*To avoid bias in our analyses due to exposure to fires, we excluded certain observations from specific ZCTAs from the control pool. If a ZCTA was exposed (i.e., within 20km) to the Getty and Woolsey Fires or exposed to any other large fire (>500 km2) during the study period, we excluded observations from that ZCTA after the date the Getty, Woolsey, or large fire ignited. We used a CALFIRE fire perimeter data36 to identify all fires > 500 km2.*

*(Page 7).*

1. 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.

We selected 20km (12.4 miles) as a distance at which study participants would feel stress due to proximity to the fire. We are not aware of literature that assessing changes in stress or mental health or related factors with changing distance from a wildfire. Most studies related to wildfire disaster assess exposure using self-reported impact (e.g., Tally et al. 2012), residence in a community where homes were burned (e.g., Jones et al. 2003), evacuation (e.g., Marshall et al. 2007), or via proximity (e.g., Silveira et al. 2021 and Johnston et al. 2021). At least two articles have estimated exposure via distance to the fire center (Silveira et al. 2021 used individuals living in Chico, a community 10-15 miles from the Camp Fire center for analyses of mental health) and distance to the wildfire boundary (Johnston et al. 2021 used a distance of <15km to the wildfire boundary versus >30km for analyses of wellbeing) as measures of exposure. We selected 20km *a priori* as a relevant distance but now also evaluate, in a sensitivity analysis, exposure comparing ZCTAs <30km to those >30km from the wildfire boundary. We found that the effect estimates at 30km were slightly attenuated compared to using the 20km distance. The results are in our supplemental digital content.

We have revised this statement to: “*We obtained shapefiles of the total areas burned during the Getty and Woolsey fires from the CALFIRE Fire and Resource Assessment Program.39 These perimeters represented the approximately the maximum burned areas of each fire40 and we used them to define exposure. US-based studies have evaluated exposure to wildfire disasters in different ways, including self-reported impact,68 wildfire damage to own home,69 evacuation from own home,70,71 residence in a community where structures burned72, residence in a county where a wildfire burned74, and residential proximity to a wildfire75. We selected the Getty and Woolsey wildfires a priori, then linked exposure via proximity to the wildfire boundaries, selecting a distance of 20km as one that could elicit a stress response; prior studies have found impacts on wellbeing and mental health at similar distances.76”*

*Page 4, Proximity to wildfire.*

1. 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.

We now more clearly describe the composition of the control group:

*To avoid bias in our analyses due to exposure to fires, we excluded certain observations from specific ZCTAs from the control pool. If a ZCTA was exposed (i.e., within 20km) to the Getty and Woolsey Fires or exposed to any other large fire (>500 km2) during the study period, we excluded observations from that ZCTA after the date the Getty, Woolsey, or large fire ignited. We used a CALFIRE fire perimeter data36 to identify all fires > 500 km2.*

*Page 7, proximity to wildfire and evacuation.*

Regarding the description of the difference in differences model, we’ve added a citation for readers who are not familiar with DID in order to clarify, and we have rewritten the sentence to reflect that ‘not a fire’ means times when the Getty and Woolsey fires were not burning:

*The DID estimators subtracted the change in visit frequency when the Getty or Woolsey Fire was burning versus not burning among control ZCTAs (difference 1) from the change in visit frequency when the Getty or Woolsey Fire was burning versus not burning among ZCTAs exposed to the fire or evacuation zone (difference 2).*

*Page 6, proximity to wildfire and evacuation.*

1. 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.

Thank you – we have revised this for clarity:

*To evaluate the association between proximity to and evacuation from wildfire and weekly ZCTA-level healthcare visit counts, we used a difference-in-differences (DID) analysis with negative binomial regression. We evaluated each relationship separately for each fire, for evacuation and proximity, and for each type of healthcare visit.*

*Page 6, proximity to wildfire and evacuation.*

1. 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 have revised the text to more accurately describe how the Thomas Fire was (or was not) involved. We 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 proximity/evacuation analysis. We do not mention it again, and the only mention of it in this (now-revised) 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, since very few participants would have been exposed to it. Still, smoke from this fire contributed substantially to wildfire PM2.5 in Ventura County in December 2017, and therefore was included in our PM2.5 analyses (Figure 2b).”*

*Page 4, proximity to wildfire.*

1. Lines 38-39 in the "proximity to wildfire and evacuation" section - when the authors say that they excluded all ZCTAs exposure to other large fires, is this just other large fires within the study period or all large fires ever?
2. Additionally, this sentence, "Therefore, we excluded observations from these ZCTAs made during and after fire exposures." is unclear - during and after which fire exposures?

We excluded just large fires during the study period. We edited the text that refers to this per comments above, here it is:

*To avoid bias in our analyses due to exposure to fires, we excluded certain observations from specific ZCTAs from the control pool. If a ZCTA was exposed (i.e., within 20km) to the Getty and Woolsey Fires or exposed to any other large fire (>500 km2) during the study period, we excluded observations from that ZCTA after the date the Getty, Woolsey, or large fire ignited. We used a CALFIRE fire perimeter data36 to identify all fires > 500 km2.*

*Page 7, proximity to wildfire and evacuation.*

1. 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, we agree with the reviewer here - this is one reason why we only excluded ZCTAs that were exposed during the study period.

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

We added a sentence detailing this:

*If a ZCTA was exposed (i.e., within 20km) to the Getty and Woolsey Fires or exposed to any other large fire (>500 km2) during the study period, we excluded observations from that ZCTA after the date the Getty, Woolsey, or large fire ignited. We used a CALFIRE fire perimeter data36 to identify all fires > 500 km2.*

*Page 7, proximity to wildfire and evacuation.*

Results:

1. 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).

This is a great point. We revised this to say *“just under the annual USEPA National Ambient Air Quality Standard of 12 (Figure 2a).”*

1. 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.

This makes sense – we have revised both the sentences suggested here, and the discussion in general, to better reflect the study population. Thank you for this suggestion. These are the new sentences:

*Surprisingly, we observed no association between wildfire PM2.5 and ED or inpatient visits* ***among DME users****.*

*However, during the Woolsey Fire, we observed an increase in cardiorespiratory inpatient visits and a decrease in all-cause outpatient visits with both fire proximity and evacuation* ***among DME users.***

1. 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 agree that this was of interest to us. One reason we ran a distributed lag model was to assess differences by day. We believe it is plausible that next day outpatient visits might truly be reduced after exposure to elevated wildfire PM and thus wanted to report this potentially heterogeneity in associations within the first week of exposure. We do update the text as suggested by the reviewer to highlight overall trends:

*In models with daily lags, we observed an initial same-day and next-day decrease in all-cause outpatient visits, and then a positive association between wildfire PM2.5 and all-cause outpatient visits among DME users for the week following exposure. In models with weekly lags, we observed increased outpatient visits in the two weeks following exposure, suggesting that there is overall an increase in all-cause outpatient visits among DME users following wildfire PM exposure. These findings are consistent with much of the literature in that they indicate increased healthcare utilization following smoke exposure. Very few prior studies have evaluated lags of short-term exposure to wildfire PM2.5 beyond 7 days, but our results indicate that outpatient visits remained elevated for up to two weeks.*

1. 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).

We revised the line in question: *“Weekly lagged wildfire PM2.5 was not associated with the frequency of any other visits.”*

1. 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.

We think this is a great point and have changed our language throughout.

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

We have updated the figure description:

*“The DID estimators subtracted the change in visit frequency when the Getty or Woolsey Fire was burning versus not burning among control ZCTAs (difference 1) from the change in visit frequency when the Getty or Woolsey Fire was burning versus not burning among ZCTAs exposed to the fire or evacuation zone (difference 2).”*

*Figure 3 description.*

**Add THIS**

1. 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.

We have reworked this figure not to include colours and hopefully to be clearer to everyone. Reviewer 1 also found the original figure confusing. New Figure 3:

**ADD IT**

1. 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".

We revised this to: “*Residence in an evacuation zone of the Woolsey Fire during the fire was also associated with increased inpatient admissions for cardiorespiratory disease compared residence outside of it, and associated with decreased all-cause outpatient visits, though the confidence interval was wide (Figure 3).”*

1. 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.

We have revised this paragraph to include confidence intervals and improved descriptions of results:

### *Residence within 20km of the Woolsey Fire boundary during the fire was associated with increased inpatient admissions for cardiorespiratory disease compared residence outside of it (RR = 1.48, 95% CI: 1.01, 2.17), and associated with decreased all-cause outpatient visits compared to residence outside of it (RR = 0.89, 95% CI: 0.79, 1.00), though the confidence interval was wide (Figure 3). We observed similar associations, with narrower confidence intervals, between Woolsey Fire evacuation exposure and healthcare visits. Residence in versus outside of an evacuation zone plus 10km of the Woolsey Fire during the fire was also associated with decreased all-cause outpatient and increased inpatient admissions for cardiorespiratory disease visits (RR = 1.76, 95% CI: 1.02, 3.05, RR = 0.87, 95% CI: 0.73, 1.04 respectively) (Figure 3).*

*Page 7, Woolsey Fire proximity and evacuation exposure*

1. 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.

In reworking the text, we’ve removed the sentence about increasing visits completely. The text now reads: “We observed no association between Getty Fire proximity during the fire and risk of all visits types (Figure 3). Residence within an evacuation zone plus 10km was associated with reduced risk of all types of visits, though confidence intervals were very wide”.

*Page 7, Getty Fire proximity and evacuation exposure*

Discussion:

1. 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.

We think this is a good point and have changed the sentence in question: *“Using electronic health data describing 236,732 Kaiser Permanente DME-using patients from 2016-2020, we found that an increase in wildfire PM2.5 concentration was associated with brief (next-day) decreases in all-cause outpatient visits but increases in all-cause outpatient visits up to two weeks later.”*

*Discussion, paragraph 1.*

1. 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.

We edited the first paragraph of the discussion and have added this sentence:

*“Using electronic health data describing 236,732 Kaiser Permanente DME-using patients from 2016-2020, we found that an increase in wildfire PM2.5 concentration was associated with brief (next-day) decreases in all-cause outpatient visits but increases in all-cause outpatient visits up to two weeks later. Increases in wildfire PM2.5 were not associated with the frequency of ED or inpatient visits among DME users. Residential proximity of DME users to the large Woolsey Fire was also associated with fewer all-cause outpatient visits, as well as more cardiorespiratory inpatient visits, but not with other visit types. Results for Woolsey Fire evacuation exposure were similar.* ***Getty Fire evacuation or proximity was not significantly associated with frequency any kind of healthcare visit.*** *Our study was unique in that we evaluated healthcare utilization among DME users, a group hypothesized to be susceptible to disaster and wildfire smoke exposures, included inpatient, ED, and outpatient visits, and examined residence near a wildfire or an evacuation zone in addition to wildfire PM2.5 exposure.”*

*Discussion, paragraph 1.*

1. 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).

We have revised this sentence to: *The literature describes a strong relationship between wildfire smoke exposure and respiratory health20, and a strong relationship between PM 2.5 exposure and cardiovascular health, though the relationship between wildfire PM2.5 and cardiovascular health are still being characterized.*

1. 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).

We have revised this to: *These findings are consistent with much of the literature in that they indicate increased healthcare utilization following smoke exposure. Very few prior studies have evaluated lags of short-term exposure to wildfire PM2.5 beyond 7 days78, but our results indicate that outpatient visits remained elevated for up to two weeks.*

1. 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. However, participants would be motivated to access care at a KPSC location because they are insured there. We discuss this in the limitations:

*First, we only had access to data on visits to Kaiser Permanente clinics and hospitals made by Kaiser members using DME. These patients would be highly motivated to seek care at Kaiser, given their membership status, however they may have sought urgent care at other clinics or hospitals. Such alternate utilization would have produced artificially reduced visit counts, especially for inpatient and emergency visits. If patients sought care at other clinics only during wildfires (whether during evacuations or while a fire was burning nearby) this could have biased association estimates towards the null.*

*Limitations, page 10.*

1. 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.

Data shows that KPSC patients are a fairly representative sample of the underlying population of the region, except that they underrepresent people at the highest and lowest ends of the SES spectrum (such as those who don’t have insurance at all). See Koebnick C, Langer-Gould AM, Gould MK, et al. Sociodemographic characteristics of members of a large, integrated health care system: comparison with US Census Bureau data. Perm J 2012; 16:37–41.

In other papers, we have said before: “*KPSC patients represent the underlying population in the region, except for slight under-representation of individuals living in the highest and lowest SES communities.*” We have added this sentence to the ‘study population’ section of the methods.

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

By ‘visits were infrequent’, we meant that the number of days on which the number of visits were 0 was large. We didn’t mean to compare the number of visits to the average frequency, but we see how this is unclear. We have revised it to: *Third, days with 0 visits made by patients living in a spatial grouping were common. Inpatient and ED visits were much less frequent over the study period (both mean = 0.1 daily visits) than outpatient visits (mean = 2.5 daily visits). All models may have been underpowered to detect visit changes. For example, during the Woolsey Fire, we observed decreased outpatient visits in ZCTAs proximate to the fire and among evacuation exposed ZCTAs but, for both, confidence intervals were wide, likely due to sample size.*

1. 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.

We did not perform power calculations prior to our analysis, because this study was based on the largest dataset available to us, i.e., all DME renters at Kaiser. We consider the DME-using population to be an important and vulnerable subpopulation of people exposed to wildfires and wildfire smoke. There have been no studies done before on this population, so despite possible power limitations, we wanted to study this group with the largest dataset we could find - we utilized data from a healthcare system used by 5 million people.

When we refer to our analysis possibly being underpowered, we refer to the wide confidence intervals around association estimates. We agree with the reviewer that it makes sense that the evacuation CIs are larger than the proximity CIs. We were just referring to the large confidence intervals in all our analyses, here.

We have tried to clarify this in the discussion by adding the following sentence:

*Though we used the largest sample available, all models may have been underpowered to detect changes in these visits.*

1. 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.

We thank you for this good suggestion, and in response, we tested for spatial correlation by plotting the mean residuals on a map and calculated Moran’s I for each of our models. Visual inspection of the maps and results of the Moran’s I did not indicate the presence of residual spatial autocorrelation, so we think that the adjustments we made were sufficient. We’ve included a map from the residual spatial autocorrelation tests in the supplemental digital content and noted results of the Moran’s I test there as well.

We added the following sentence to the results:

*“Visual inspection of model residuals and Moran’s I results indicated that our model residuals were not exhibit spatial autocorrelation (plots are included in the eAppendix)”*

*Page 10, lines 1-3.*

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

We have added a short concluding paragraph: *“As wildfires become more frequent and severe with climate change, we must understand how they affect both local populations and those exposed to wildfire PM2.5. Protecting vulnerable populations that may be harmed by exposures which others can avoid or endure is essential. More work is needed to understand we can best support those affected by smoke, fire, and evacuation.”*

Page 11, Conclusion

Editorial notes:

The reviewers recognize strengths of your study, including the different exposure definitions applied and the unique study population. However, a number of concerns and comments are also raised, including the need for enhanced clarity in the text and motivation for different elements of your study design. Please consider all reviewer comments.

1. In addition, please clarify the motivation for the different exposure metrics considered (i.e., daily wildfire, proximity zone, evacuation zone) and how these different exposure definitions may be similar or different as risk factors for healthcare utilization among the DME population.

Thank you for the opportunity to fully explicate the choice of three different wildfire-related exposure metrics. We have amended the introduction to include additional citations and explanation of the exposures:

*Fewer studies21,25–27 have examined smoke exposure in vulnerable populations, or wildfire-related exposures other than smoke. Still, several have documented the effects of stress, evacuation, property destruction, or injury due to wildfire disasters7,8,28–31. We hypothesize that residential proximity to wildfire, and evacuations due to wildfire, could influence health outcomes primarily through stress. This pathway may operate differently than wildfire PM2.5 exposure. Residents living near active wildfires are exposed to smoke, which in addition to biological effects can cause substantial worry80. Proximate residents also experience the disruption of usual activities in their communities, and face the threat of injury, evacuation, or longer-term displacement. Evacuation may cause more severe stress as these threats materialize. Evacuation, however, may also disrupt access to healthcare and could result in a temporary reduction in utilization.81*

Introduction, page 2, paragraph 4.

1. In addition, the DME study population included is not well characterized and appears to be quite broad, including individuals with ailments other than cardiorespiratory diseases (which appears to be a pathway of interest). Enhanced characterization of the study population, and motivation for including specific subgroups, will be helpful for interpretation of results from this study.

We will try to interpret this comment broadly and provide some information here about our motivations. We have previously published a paper characterizing the DME population at Kaiser Permanente from which the current study population is drawn (Casey et al. 2021). We aimed to assess the effects of wildfire exposure on people whose durable medical equipment (DME) use might indicate that they were medically or socially vulnerable. 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 counts from our partner Kaiser Permanente also include people who rent breast pumps. Renting a breast pump likely does not indicate vulnerability and may in fact indicate a certain level of health. Excluding DME using people under 45 excludes nearly all healthy people renting breast pumps, and leaves those renting other types of DME. There is more information in Casey et al. 2021.

Unfortunately, due to resource limitations, we are not able to derive additional information about the DME population included in the study, or break down analyses into further subgroups. Again, we chose to focus on the population of people using DME because we think DME may be an indicator of medical and social vulnerability. We believe this group may be particularly vulnerable to climate-related disasters in general.

References:

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\* \* \* \* \*

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.

1. We find these citations relevant and interesting: <https://www.sciencedirect.com/science/article/abs/pii/S136192092200102X>

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