1st June 2018  
  
Dear Dr Hauer,  
  
Your Letter entitled "Climate change projected to reduce European life expectancy" has now been seen by 3 referees, whose comments can be found appended to this letter. In the light of their advice, we have decided that we cannot offer to publish your manuscript in Nature Climate Change.  
  
Although some of the reviewers find your work of interest, all three reviewers raise concerns about the degree of advance your findings represent over earlier work and the strength of the novel conclusions that can be drawn at this stage. We feel that these issues are sufficiently important as to preclude publication in Nature Climate Change.  
  
I am sorry that we cannot be more positive on this occasion but hope that you find the reviewers' comments helpful when preparing your paper for resubmission elsewhere.  
  
Yours sincerely,  
  
Adam Yeeles, PhD  
Editor  
Nature Climate Change  
  
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Reviewers Comments:   
  
Reviewer #1 (Remarks to the Author):  
  
I am afraid I have a different opinion from the authors. The authors claim that life expectancy is an easy-to-understand index for the public, life expectancy is a product of very complex calculation with the set of assumptions and is not intuitively understandable. If the life expectancy at birth was 30, probably the public would think that many of the people died at around the age of 30, say 25 to 35, but in reality, many people died in their infancy or childhood. In this regard, I think excess mortality is easier to understand.  
  
This paper also has a serious problem with cause of death confusion. Most of the heat-related excess deaths, among the climate change related deaths, are due to heart or respiratory diseases. Thus, "climate change" cannot be compared with heart diseases or respiratory diseases in Table 1. If "Climate Change" is one of the causes, other causes should be, for example, air pollution, cigarette smoking and so forth. In Table 1, some of the other causes are respiratory diseases, heart diseases, and suicide, which have been reported to be heat-related (i.e., climate change-related).  
  
For these reasons, I cannot be positive to recommend this paper to be published.  
  
  
Reviewer #2 (Remarks to the Author):  
  
see attached file  
  
  
Reviewer #3 (Remarks to the Author):  
  
This paper reports an estimate of impact of climate change (CC) under business-as-usual on life expectancy in Europe. As it takes the estimates of CC on death rates from a previous publication (ref 3), it is effectively an extension of that paper, translating the death rate to life expectancy changes under assumptions.

The analysis, taking this paper with ref 3, is impressive in its bringing together complex data sets preserving complexity in some dimensions (eg geography), though the health dimension (weather epidemiology) is crude. The way the results are presented claims much greater generality and certainty than the paper actually has. The paper is not at all explicit as to several assumptions which imply a much narrower focus, in particular:  
  
a) The only pathway considered is though “extreme weather events”, which from ref 3 appeared to be 100-year events (though I am unsure, perhaps all events recorded in the disaster data-bases used). Whatever the specifics, for direct impacts of heat and cold, there is abundant evidence that much more moderate heat and especially cold give rise to more excess deaths than extremes. For example Gasparrini Lancet 2015 reported epidemiological estimates of deaths attributable to heat and cold in recent decades from 384 locations from 13 countries. That paper estimated 0.4% of all deaths were due to heat and 7.3% to cold (7.7% overall), but just 0.9% in days outside the location’s 2.5-97.5 centile range of temperatures).

b) Baseline deaths due to extreme heat and cold were those available from disaster data-bases. Quite what deaths are thereby counted is never explicitly described in this MS or its source ref 3. I strongly suspect that these are not standard epidemiological estimates, and are probably deaths certified as due to heat or cold, which would be a small fraction of what overall numbers would be would be. For example Fouillet 2006 Int Arch Occ Env Health estimated that in the 2003 French heat wave of 15,000 deaths attributable to heat, 3,000 were certified as such. The ratio for cold would be much greater. I expand on this in comment 5 below.

c) It is assumed that deaths attributable to heat and cold would have had average life expectancy for their age. But estimates of impact of heat and cold (waves) on mortality virtually all consider only acute effects – deaths in or shortly after the waves. It is widely recognised that it is possible that these are mainly among very frail persons close to the end of life (“mortality displacement” or “harvesting”) How much life shortening those excess represent is thus speculative.  
  
The first two of these are concerns I have about ref 3. This was published in a new but generally respected health journal. I acknowledge that my views differ from the referees of that journal, but I must call things as I see them.  
  
More specific comments  
1. I found the title (“Climate change projected to reduce European life expectancy”) misleading in claiming a generality the estimate does not have. I would suggest something like “Climate change impacts from extreme weather projected to reduce European life expectancy”.

2. The impact of CC on death rates is taken entirely from one previous report (Forzieri 2017 ref 3). Even taking the focus on extreme events as given, this is a limitation. OK to reduce complexity but requires acknowledgement as a limitation.

3. Critical to estimates of impact of CC on mortality (whether as rates or life expectancy) is the extent to which increase in heat-related deaths might be compensated by reduction in cold-related deaths. Other prominent publications have baseline data indicating that cold may play a much greater role that estimated in this submission. To illustrate, Table 1 for reference 3 used for the present estimates note baseline rates attributable to heat and cold as 0.98,0.2 in Northern Europe and 105.77,0.33 in Southern. Gasparrini (2015 lancet) estimates fractions attributable to heat and cold in several countries in these regions over recent past periods to be some many times HIGHER for cold than heat. This difference seems likely due to the current paper considering only extreme weather events and deaths counted in disaster data-bases, whereas the Gasparrini paper cited above considered temperature as a continuum and deaths estimated epidemiologically.

4. That different approaches estimating different things lead to different conclusions (see eg Gasparrini lancet PH CC projection sequel to the lancet paper) is not surprising. My plea is that new contributions maximally clarify assumptions, in particular those that might explain difference from alternative estimates of apparently similar things, and cross-reference them.

5. I found it hard to track down the source for assumed “human vulnerabilities” (proportion of persons experiencing a hazard – “exposed” - dying because of it). However the Appendix of ref 3 cited two “disaster data-bases” for the numbers of deaths (2000-2010) and given numbers exposed, for which methods are described, vulnerabilities could I saw be computed. These thus depend critically on what deaths the disaster data-bases describe. The description in the ref 3 appendix suggests that the complexity of this, for heat and cold, is not understood. It is not possible to identify more than a small fraction of deaths caused by heat or especially cold (unlike, say, floods) from the deaths themselves. People may die for example from a heart attack which look like any other. Only epidemiological analysis comparing observed numbers with those expected from usual pattern can estimate an attributable proportion and hence number. The disaster data-bases probably include  
only deaths on which heat/cold was noted as underlying cause on the death certificate. Certainly numbers are very small compared to epidemiological estimates. There should be a clear statement of what deaths this estimate is including.

6. The method adopted appeared to consider simply the size of population exposed to heat and cold extremes, not duration of exposure. Even if this paper does indeed just consider reference-period 100-year events, given that in the projection period most of some populations seem exposed suggests that duration could be an important factor.

7. The Methods section of the current paper refers to “mortality rates (mx) for mortality.  
from environmental heat and cold exposure” in the context of its mention of use of the GBD data. None of the GBD papers I am aware of mention that these were estimated, and the recent GBD “comparative risk assessment” (2017 lancet; GBD 2016 risk factor collaborators) explicitly mention that temperature-related deaths would be estimated in the future but have not been to date (p1411).

8. Some of the difference in results across countries are surprising enough to require more comment – indeed they cast doubt on the validity of the estimates overall without such comment. In particular, it should be possible to clarify why Luxembourg has such a high estimate (eg which event types contributed so differently).

9. It is natural that this paper draws heavily from the results and methods from reference 3. However, it is not always clear when references are to the current paper and when to that source. For example, I spent some time searching for a “supplementary table S8” before finding that ONE of the references to it explicitly mentioned that it was from ref 3 not the currently submitted one.

In summary, my main concern is that this paper is unclear as to its true scope as considering impact of extreme weather events on life expectancy through deaths recognisable individually as caused by such events, NOT total impact of cold and heat on life expectancy.