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Dear Dr. Tonks,

Thank you and the other editors and reviewers for the thoughtful review of our manuscript. We have edited the manuscript based on these suggestions, which we think has led to a stronger piece. Below are our itemized responses to the suggestions.

Many thanks,

Anupam B. Jena, MD, PhD

Response to Editors' comments

Thank you for your thoughtful comments.

We enjoyed reading your paper and appreciated the work you had done so far. The following questions and comments came up during our discussions:

*The title implies a causal link between election and death, which isn't appropriate. Also we questioned whether it was appropriate to imply your paper is about the stress of politics, since you don't measure the stress of politics Please re draft.

This is a good point. We have renamed the paper a less assertive title (Do heads of government age more quickly? An observational study comparing mortality between elected leaders and runners-up in national elections of 17 countries."

*Why did you select these countries?

The 17 countries that we analyzed were chosen because of their similarity to France and the UK, for which reliable life tables exist dating back to the 19th century." Further, these countries carry appeal for their long history of relatively stable democratic institutions (compared to countries in South America or Africa, for instance). We highlight this on page 8 (just before the section heading 'Analysis').

*Please remove causal inference from the "what this paper adds" box, and tone down causal inference throughout the manuscript. This is observational work so we shouldn't imply cause and effect, just association.

We have modified our language in both the "what this paper adds" box and the remainder of the manuscript.

*One of our European editors identified some "winners" who had not actually won elections-but ended up in office just the same:

Austria 1920: The authors list Michael Mayr. He wasn't elected in general elections but appointed by the ‚Provisorische Nationalversammlung für Deutschösterreich'.

Austria 1938: the authors list Arthur Seyss-Inquart as elected chancellor. This isn't true. He was an Austrian Nazi politician who served as Chancellor (I think for two days before the ‚Anschluß') without being elected after Hitlers pressure on the Austrian President. He was ‚Reichstatthalter' in Vienna during the Nazi times. At the Nuremberg trials, he was found guilty of crimes against humanity and sentenced to death.

In Austria in the 1999 election, Wolfgang Schüssel's peoples party finished third behind the social democrats and Jörg Haider's Freedom Party. Schüssel formed a coalition government with the Freedom party. The heads of the governments of the other 14 EU members decided to cease co-operation with the Austrian government immediately. Schüssel lost the election but ended up as prime minister.

In Italy the authors list Berlusconi as winner of the 2013 election. This isn't true. The Democratic Party under Bersani won the election. Bersani could not form a government and Enrico Letta was prime

minister of a grand coalition. Berlusconi only followed him about six months later after he had broken the coalition.

Are you confident that you handled the above "participants" correctly?

Thank you for noting this. We reviewed each of these candidates and agree that they do not fit within our defined treatment group. We have now removed these candidates from our analysis. As is probably clear, obtaining the electoral histories of each candidate is actually more difficult than it would seem, particularly in parliamentary systems where candidates often run in multiple years and those appointed to prime minister are sometimes not those that win the election (e.g., the second and third parties may form coalitions which supersede the winning party). These issues are in addition to the examples kindly raised above. Although we did our best to verify the accuracy of each observation, measurement error is always possible though we do not believe it should result in any systematic biasing of our results. We now include this issue as a potential limitation of our study in the Discussion.

*Please add some study dates to the abstract [elections included over what period?]. Also, add more details to the results section of the abstract, not just summary results [eg total numbers of winner and runners up, years survived for both groups etc]

We have added this information to the abstract.

*You assembled your data from “on line sources”. Which online sources? Readers need enough methods detail to be able to replicate your study. Technical details can go in an on line appendix, but put as much detail as you can in to the paper.

We have clarified within the text. We used two main sets of online sources, national lists of leaders (from country websites) and Wikipedia.

*In the methods section, you mention “Similar approaches to ours have been used in prior studies which compare mortality among winners and losers of specific events to identify the effect of winning that event on mortality (e.g., comparison of mortality among actors winning versus losing an Academy Award, or “Oscar”, nomination; baseball players inducted into the Hall of Fame; and Nobel Prize winners).7-9 …..these papers have been criticised for "immortal time bias" . Does your design have a similar problem?

Thank you for raising this excellent question. We do not think that our study suffers from immortal time bias, which was an issue raised by Sylvestre et al (*Ann Intern Med*. 2006; 145(5):361-363) in response to the original Annals of Internal Medicine publication on Oscar winners. In the original article, the authors showed that Oscar winners had longer lives (i.e., years alive from birth) than losers. The concern raised by Sylvestre et al. was that total life expectancy was compared between Oscar winners and losers, which was not the right outcome because those alive longer would be more likely to win an Oscar, hence the “immortal time bias.” The solution of Sylvestre et al. to this issue was to measure “years lived after an Oscar” rather “years lived from birth.” We employed a similar approach, measuring “years lived after election,” rather than “age at death” for winners versus runners-up.

*Was the discrepancy between winners and losers more marked before the second half of the 20th Century (ie, is it an historical problem)? Eg Margaret Thatcher lived much longer than expected according to table 2.

This is a good question. We did analyze this in an earlier stage of the research and did not find that the effects varied by century. Due to the 1700 word space limitation we did not include this but would be happy to add a sentence to this effect if desired.

*Our statistician would like you to respond to the following comments:

1.The authors report a significant difference between the survival curves - how did they test for this? Log rank test, what was the result to this test?

Our apologies for not including this - we used the log-rank test for survival function equality. We now report the results in the manuscript.

2. It's unclear why they presented a model on just those officials that had deceased (presume a logistic regression model). This seems surplus - the cox regression model censoring those officials as still being alive seemed adequate.

We agree. We have removed the cox model which focused on only deceased candidates. We now include the results of (1) the linear regression model of observed life years as a function of winner status and life expectancy at election and (2) the cox model that includes all candidates, i.e., both deceased and living candidates (the latter being censored observations). The purpose of including the linear model (1) was to have a simple illustration of the difference in life-years associated with being elected head of government, adjusting for life expectancy at election.

3. I wondered if it would be worthwhile doing a sensitivity analysis using time from first election to death (as well as time from last election). It's possible there could be differences in stress levels if this is a re-election (or candidates are more familair with the process) or feel under more pressure to win an election following a failed previous campaign.

This is an excellent suggestion which unfortunately would be difficult for us to implement since we specifically collected information on last election rather than the set of all elections for each candidate. Obtaining the electoral histories of each candidate is actually more difficult than it would seem in parliamentary systems because candidates run in multiple years and those appointed to prime minister are sometimes not those that win the election (e.g., the second and third parties may form coalitions which supersede the winning party).

We agree that those candidates who are running for re-election may have added stress compared to runner-up candidates, but this would seem to be a mechanism by which exposure to serving as head of state mediates an 'effect' on mortality, rather than an issue of confounding. In other words, it might be that focusing on candidate survival from first election does not yield differences in mortality whereas survival from last election does, precisely because the latter group would select for people who ran for re-election and were therefore more stressed compared to runners-up.

*Do you have any ideas for illustrations? Perhaps a photo of an internationally well known premier who died younger than expected? William Pitt the Younger?

We believe that William Pitt the Younger is an excellent selection. He died significantly younger than expected. Further, it may be useful to note that he led the United Kingdom against France during the

Napoleonic Wars, which was surely a period of tremendous stress that may have accelerated his aging, consistent with our findings.

Response to Reviewer 1's comments

Thank you for your thoughtful comments.

This paper has been markedly improved by the authors in responding to the comments of the reviewers.

However in making the excellent improvements to the methodology to avoid double counting (e.g. counting the same person as a loser at one election and a winner at another), the authors may have introduced a different methodological bias - closely related to the so called "healthy worker effect" (1,2,3).

By considering only the last election at which a candidate stood for office, it is arguable that those who were not already employed as a leader would only stand if they were fit and healthy enough to take on the challenge of the job (known as fitness at recruitment or selection). However those already in post may have developed a health condition since their election through the normal morbidity incidence rates in the population - i.e. have lost their initial fitness at selection for the post(4). But illness is often not seen as a barrier to seeking re-election. So they would already be less healthy than their opponent, or the general population, on the final occasion on which they were elected. There is some evidence for this, at least in the records for 18th and 19th Century politicians whose death in office prompted a new election or an inter-regnum.

Thank you for raising this point, which we agree is a potential limitation of our new analysis. Put differently, if the 'cost' of running for election is lower for those candidates running for re-election (perhaps because they are more familiar with the process) than it is possible that those who run for re-election are healthier on average than those who were not previously in office simply because they have a lower threshold to run (and presumably one's level of health contributes to this threshold). However, it is difficult to gauge the magnitude of this 'healthy worker bias.' One potential solution to this problem would be to look at survival from the first election (rather than the last election); however, our database does not include all elections for a candidate, just the last election. We focused on last election in order to identify the point of most recent 'exposure.' We now identify this 'healthy worker bias' as a potential limitation of the article and cite the suggested references.

Response to Reviewer 2's comments

Thank you for your thoughtful comments.

I can be brief (and I am aware that Christmas is a-coming) and say that I think this is a very much improved version of the paper from the last draft. The data are better, the analyses are far better, and the figure gives a much better sense of what is going on. I also thank the authors for their detailed and careful responses to the various suggestions of the reviewers.

My only suggestion concerns the figure, where it is very unclear what is the N at various times. I did look at some images on Google, and there seem to be two standard ways of indicating N at various times. If N is fairly small then a little tick on the curve can indicate censorship (see http://www.breast-cancer-research.com/content/10/4/R73/figure/F3?highres=y). Alternatively, as in a recent BMJ Statistical Question, the Ns in the two groups can be indicated along the bottom as figures (see BMJ 2013; 347 doi: http://dx.doi.org/10.1136/bmj.f7118). One or other would probably help the reader.

Otherwise I am happy to see this paper published.

We appreciate the suggestion and have added the sample sizes at each label to our figure.