We thank the editorial committee and the reviewers for their thorough review of our manuscript and for their helpful recommendations, to which we respond point by point in the following.

“We felt that the study adds only incrementally but since it is such a large study, it may be more definitive than others, and the message that smoking cessation efforts should continue, even among those older than 60, is important (are those 60-69 elderly?)” (Manuscript committee, comment #1)

Thank you for your kind appraisal of our study, we indeed believe that the public health implications of our findings are highly relevant and important. With regards the question whether those 60-69 are elderly, we would like to note that there is currently no standard definition of “the elderly”. In our study, we use the cut-off of 60+ years to refer to the elderly population, which is in agreement with the UN/WHO’s agreed cut-off (http://www.who.int/healthinfo/survey/ageingdefnolder/en/).

“This study is well done. One limitation is the use of a single measure of smoking status, at baseline, rather than repeated measures. There do not seem to be repeated assessments of smoking status in the cohorts and baseline status is presumed to remain constant. Can the authors please comment on this issue?” (Manuscript committee, comment #2)

It is indeed a limitation of our study that the smoking variables were measured at baseline only. While harmonising and bringing together the data of so many different cohorts in CHANCES created a unique large-scale and multi-national database for health- and aging-related research questions, the limited amount of repeated measurements of exposure is a limitation. In our study, having only baseline smoking status available for analyses has probably led to an underestimation of the risks associated with smoking and of the benefits related to quitting. We have edited the limitations section in order to further emphasise this limitation, and we added a literature reference and expanded the discussion of this issue and its implications. (page 16, para 2, reference #32).

“We would like more description of the calculation of the risk advancement period (RAP) – this is not standard output from these models, but seems a good way of summarizing the impact.” (Manuscript committee, comment #3)

Following this recommendation, we now provide more details on the calculation and interpretation of this metric in the appendix (appendix, page 7). The reference to the appendix is given in the methods section (second paragraph on page 10).

“The title should be modified since it should be descriptive but not announce the findings. The authors may want to look for advice in the BMJ instructions for authors.” (Manuscript committee, comment #4)

We have changed the title accordingly. The revised title now reads “Impact of smoking and smoking cessation on cardiovascular events and mortality among older adults – Meta-analysis of individual participant data from prospective cohort studies of the CHANCES consortium”.

“The “What this study adds” box needs to be in bullet points, it seems too narrative at the moment.”
(Manuscript committee, comment #5)

We have revised the “What this study adds”-box accordingly (page 5).

“The authors insist in the Background section about the paucity of previous studies in old people. Although this is important, the authors should strengthen that encouraging smoking cessation in old people has been usually neglected and hence smoking cessation in people over 60 is not frequent (perhaps some figures from the last available Eurobarometer on smoking could illustrate it)”
(Reviewer #2, comment #2)

We thank the reviewer for this suggestion – it is indeed an important point that promotion of smoking cessation in older adults has been neglected in the past. We have incorporated this point in our discussion and have added literature references to support this (first para of page 17, references #33-35).

“The authors offer a detailed description of the cohorts included in the meta-analysis. 5. The CHANCES Consortium is introduced in the Methods section (page 8, lines 10-22). The authors should include the CHANCES website address within the text.”
(Reviewer #2, comment #5)

Following this helpful advice, we have included the link to the CHANCES website in the methods section. We have also added a reference to a recent paper describing the CHANCES consortium (page 7, para 1, reference #16).

“From the description of the cohorts, it is clear that some other non-CHANCES cohorts were included. Thus, the inclusion in the title of the reference to the CHANCES consortium is misleading (“...of the CHANCES consortium”).”
(Reviewer #2, comment #6)

We understand that the reviewer finds it misleading that we only reference the CHANCES consortium in the title even though we also include two non-CHANCES cohorts. However, the whole study has been conducted within the framework of the CHANCES consortium and only two out of 25 cohorts are not part of the consortium but are open-access cohort studies. The inclusion of these non-CHANCES cohorts is clearly described in the methods section (page 7, para 1-2). In order to keep the title as concise as possible, we would therefore prefer to not mention the non-CHANCES cohorts separately in the article title.

“The authors have chosen a Cox regression model for the analysis of the cohorts. This non- or semi-parametric model relies in the assumption of the proportionality of hazards. Have the authors checked (statistically or graphically) this assumption in their individual datasets and in the pooled dataset?”
(Reviewer #2, comment #7)
The proportional hazards assumption has indeed been checked graphically for each cohort. We have added this information to the methods section (page 10, para 1).

“The authors have presented some results to assess a dose-response relationship (HR according to smoking status categories, or according to number of cigarettes per day smoked or time since quitting). However, there are no statistics testing the dose-response associations observed. Chi-squared tests for trends could be included in tables and figures and hence strengthen the reliability of the associations observed.” (Reviewer #2, comment #9)

We are thankful for this valuable advice. In order to assess the dose-response relationships across categories of smoking intensity and time since smoking cessation, we now use a meta-analytic approach for dose-response relations (description added to the methods section in para 1 and 2 on page 11). We now report summary estimates and p-values for the linear trend of the dose-response relationships in the results section (para 1 on page 12, last para on page 12/first para on page 13).

“The authors should strengthen the utility of the results for prevention, especially the use of RAPs in the context of clinical or community programs.” (Reviewer #2, comment #9)

We believe that RAPs are indeed a useful in communicating risks of smoking and benefits of smoking cessation to smokers. However, we are not aware of any studies that actually evaluated the use of this measure. Therefore, we can only hypothesise about the utility of RAPs in smoking-related risk communication and strongly encourage others to realise experimental or intervention studies that actually evaluate the utility of RAPs in the context of clinical or community programs. We have revised the respective section in the discussion to strengthen this point (page 17, end of para 1).