I thank the authors for their thorough response to the reviewers' comments. I believe you've done a good job of responding to their comments, and with a few very minor additional changes I'll be happy to accept this paper. Please see my specific comments below. In addition, while making these last revisions, it would be useful to improve the grammar. While your response says that you asked the two native English speakers to read the paper, you do not actually say if they did so, and in any case a final look by the native speakers is warranted.
Thank you for all your work on this paper, which I think will be a very valuable contribution to the community, and I look forward to accepting it.
Regards,
Drew Shindell

Response: We thank the editor for supporting the publication of our manuscript and the additional comments which help us to improve the quality of the manuscript. Mr. Chris P. Nielsen, one of the English-speaking co-authors, did review the manuscript very carefully and gave detailed comments on the grammar. We revised the manuscript following his suggestions. In case of additional grammar errors while addressing the editor's new comments, Mr. Chris P. Nielsen gave a final editing of the manuscript.

We address the editor's specific comments below. The original comments are in blue and our responses are in black.

Uncertainty analysis (P18, starting line 15): I appreciate very much that you've added a discussion on uncertainties following the suggestion of reviewer 2. This is very helpful. At the start of this section, the text states that you use a Monte Carlo analysis 'following the methodology described in Bo et al (2008) and Wei et al (2008)'. I would like you to please add here a brief description of where the emission factor and activity data uncertainty estimates come from. That is, please explain (1) do the values themselves come from the two cited papers, or do you just use the methodology of those papers but the data comes from elsewhere, and (2) whether the uncertainty estimates come from those two papers or not, provide readers a general description of where those uncertainty estimates come from.
Response: We appreciate the editor's valuable comment. In the revised manuscript, we have provided more details of the methodology for uncertainty analysis, in particular how we estimated the uncertainties of the activity data and emission factors (from Page 18, Line 15 to Page 19, Line 4 of the revised manuscript). The revised method description is as follows:
A Monte Carlo uncertainty analysis was performed on the emission inventories of East Asia for 2005-2010, following the methodology described in Bo et al. (2008) and Wei et al. (2008, 2011). The probability distribution of the activity data and emission factors were determined using the method and data used in Wei et al. (2008, 2011) as a starting point. We re-evaluated the uncertainties of the emission factors taking account of new field measurements in recent years. Specifically, we assumed that the

uncertainties of the activity data and emission factors had lognormal distributions. The uncertainties of activity levels were rated from level I to level V, corresponding to coefficients of variation (CVs, defined as the ratio of the standard deviation to the mean of a probability distribution, indicating the extent of variability in relation to the mean of the population) of ±30%, ±80%, ±100%, ±150%, and ±300%, respectively. For example, the activity levels derived directly from statistics were rated as level I, and those calculated using nonstatistical data and empirical conversion factors were rated as level V. The uncertainties of emission factors were also rated from level I to level V, with the corresponding CVs of ±50%, ±80%, ±150%, ±300%, and ±500%, respectively. The emission factors for the sources with stable emission rates and over 10 local field measurements were rated as level I, while a rating of level V was assigned when the emission factors for similar sources were applied due to the lack of measurements. With the determined probability distribution of the activity data and emission factors of each source, the Monte Carlo method was used to propagate these uncertainties into an uncertainty for the total inventory.

References:

Bo, Y., Cai, H., and Xie, S. D.: Spatial and temporal variation of historical anthropogenic NMVOCs emission inventories in China, Atmos. Chem. Phys., 8, 7297–7316, 2008.

Wei, W., Wang, S. X., Chatani, S., Klimont, Z., Cofala, J., and Hao, J. M.: Emission and speciation of non-methane volatile organic compounds from anthropogenic sources in China, Atmos. Environ., 42, 4976–4988, 2008.

Wei, W., Wang, S. X., and Hao, J. M.: Uncertainty Analysis of Emission Inventory for Volatile Organic Compounds from Anthropogenic Sources in China, Environmental Science, 32, 305-312, 2011 (in Chinese).

P18, L29: Please define the ‘coefficient of variation’ at the first usage (I think that’s here) as this statistic is likely not so familiar to many readers.

Response: We have added the definition of the “coefficient of variation” at its first usage. The sentence is given as follows:

The uncertainties of activity levels were rated from level I to level V, corresponding to coefficients of variation (CVs, defined as the ratio of the standard deviation to the mean of a probability distribution, indicating the extent of variability in relation to the mean of the population) of ±30%, ±80%, ±100%, ±150%, and ±300%, respectively. (see Page 18, Line 21-24 of the revised manuscript)

P2, L3: Change ‘estimated’ to ‘projected’.

Response: Revision has been made.

P2, L9: You describe that these ‘maximum feasible reductions’ are based on energy-saving policies and end-of-pipe technologies. Hence please add something like “using energy-saving policies and end-of-pipe technologies” after ‘maximum feasible reductions’ since of course one could eliminate even more emissions if society

changed more drastically, so 'maximum feasible' needs some context about the assumptions of what's considered feasible.

Response: We thank the editor for this valuable comment. We have revised this sentence as shown below. In addition, we have revised similar expressions in the main text accordingly.

Assuming the full application of technically feasible energy-saving policies and end-of-pipe control technologies, the emissions of NO_X, SO_2, and $PM_{2.5}$ in East Asia would account for only about one quarter, and NMVOC for one third, of the levels of the baseline projection. (Page 2, Line 9-11 of the revised manuscript)

P19, L12: Delete 'the' before 'emission factor'.

Response: Revision has been made.

P20, L29: I recommend adding the paper Shindell et al., Science, 2012 along with the citation to UNEP and WMO, 2011 as the latter can sometimes draw criticism for not being a peer-reviewed journal (which is part of the reason we published the Science paper, to deflect such criticism).

Response: Revision has been made.

P21, L27: Add 'an' before 'elasticity'.

Response: Revision has been made.

P21, paragraph starting with L21: The response to reviewers contains a much more detailed explanation of how the elasticity coefficients are used, and I think the authors should seriously consider adding this to the text. It may be in Zhao et al, but far easier for readers to have these few additional lines of explanation here.

Response: We appreciate the editor's valuable comment. We have added the explanation in the "response to reviewers" into the revised manuscript, which is also shown below. (from Page 22, Line 17 to Page 23, Line 1 of the revised manuscript)

We applied an elasticity coefficient method for the estimation of future production of industrial products, the governing equation of which is as follows:

$$Y_{t1} = Y_{t0}\left(\frac{dv_{t1}}{dv_{t0}}\right)^{\delta} \quad (1)$$

where, $t0$, $t1$ are time periods, e.g., $t0 = 2010$, and $t1 = 2030$; Y is the yield of a specific industrial product; dv is the driving force, namely sectoral value added or population; and δ is the product-specific elasticity coefficient. The values of δ are determined through (1) historical trends during 1995-2010; (2) the experience of developed countries; and (3) projections of industrial associations. Generally speaking, production of most energy-intensive commodities used in construction of infrastructure are expected to increase until 2020, and then to stabilize or even decline after 2020, whereas products associated with household consumption are expected to increase through 2030, although at a declining rate. We projected lower production of industrial products in the PC scenario than those of the BAU scenario because of

more energy-conserving lifestyles.

P22, L17: Change ‘with’ to ‘as’ and change ‘of’ to ‘in’.
Response: Revision has been made.

P40, L66: Change ‘as of’ to ‘through’ (I believe that’s what’s meant).
Response: Revision has been made.

P40, L22: While true that these agree fairly well (and I’d add ‘fairly’ before ‘well’), I think you should add a statement such as “although this agreement is very sensitive to the trend start and end year choices”. Clearly the apparent agreement with SCIAMACHY would be lost if you started in 2006 instead of 2005, for example, so you shouldn’t overstate this.
Response: We thank the editor for this comment. We have revised this sentence as follows:
Despite the inconsistency above, the estimated overall change rate in SO_2 emissions from 2005 to 2010 agrees fairly well with satellite observations, although this agreement is very sensitive to the choice of trend start and end years. (Page 41, Line 21-24 of the revised manuscript)