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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of Pollution-Health-Economics Nexus in China" *by* Yang Xia et al.

Anonymous Referee #1

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This study examined the economic loss resulting from disease-induced working time reduction from PM2.5 exposure in China. The authors estimated both direct and indirect economic loss in industrial sector based on a supply-driven input-output model. Compared to their previous 2007 study, this study is superior in several aspects, including use of the most up-to-date input-output dataset of the year 2012, analysis of cross-regional impacts due to interdependence, and illustration of detailed sectoral information on economic loss in secondary industry. The manuscript is clearly written, although some sentences need to be polished (as detailed below). I would recommend publication in ACP if the authors could address the concerns I raise.

Major concerns:

1. One methodological issue is the translation of mortality into working days lost. In

section 4.4, this is described as "each death will result in a total 250 working days lost . . .". It confuses me why one death only results in one-year working days lost instead of years he supposes to work till retirement. We often consider the PM2.5-induced health impacts in a cumulative way, and that's what the disability-adjusted life year (DALY) stand for. DALYs represent the lost years of "healthy" life. DALYs for one premature death is determined as the standard life expectancy at age of death in years. If one death only accounted for one lost year of healthy life, then DALY would be significantly underestimated. It's rather the same to calculate lost years for working. One may argue that working days lost in years after 2012 don't account for the economic loss in 2012. Then mortality occurring before 2012 should be taken into account to comprehend the 2012 economic loss. Otherwise, the economic loss would be underestimated. The authors should clarify this.

2. To cascade the impacts of working days lost into production supply chain, the percentage reductions in labor time loss were directly used as the percentage reductions in industrial value added in the IO model. This should be based on the presumption that labor inputs dominate the industrial values added, which might not be the case in certain capital-intensive industry. The authors may need to clarify to what extent labor inputs contribute to industrial values added in different sectors, and if not dominant, how this assumption will bias their results.

Specific comments:

1. The Methods section should go before the Results section.

2. How was the direct economic loss calculated?

3. The Introduction only contains one citation. A plenty of sentences need references. For example, "Serious air pollution in China has largely inspired epidemic studies that examine . . ."; "Existing epidemic studies simulate a exposure-response relationships between Particulate Matter (PM) concentration levels and relative risks (RRs) for a particular disease"; "while health costs assessments frequently stem from patients'

perspective at microeconomic level. . .”; “Inspired by our previous work. . .”, and etc.

4. Line 85, “Guangdong province. . ., where a substantial increase can be observed at 175 thousands compared with results in 2007”. Why did it increase? Additionally, "175 thousands" should be "175 thousand".

5. There’re several sentences start with “I” such as sentences in Line 107, 121, 159, and etc, where “we” is expected.

6. Line 112, ” Primary industry includes agriculture and fishing suffered the economic loss at 19.12 billion Yuan”. The sentence needs to be rewritten.

7. Line 123, “The left-hand side shows the regional indirect economic loss while the right-hand side denotes the sources for these indirect economic loss. The proportion of regional indirect loss among regional total economic loss is displayed next to each region’s name on the left-hand side”. Some of the information has been mentioned in the figure caption and thus don’t need to be mentioned in the main text.

8. Line 355, “We referred to an integrated exposure-response (IER) model developed by Burnett et al (2014) . . .”. The reference was not found in the reference list. Also, the IER model in Burnett’s study cannot be used to estimate hospital admissions and outpatient visits. need more accurate description here.

9. Line 370, “Then, the calculated RR was then converted into an attributable fraction (AF) in Eq.(2)”. Delete the second (or the first) “then”.

10. Line 385, “ÎŠ: the parameter that describes the depth of the curve (Table SI-1 in Supplementary Information)”. There’re two Table S1 in Supplementary Information.

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