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Interactive comment on "Health and Economic Impacts of Ozone Pollution in China: a provincial level analysis" *by* Yang Xie et al.

Anonymous Referee #2

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This paper addresses an important topic - health and economic impacts of ozone pollution in China - with a set of state-of-the-art models. This type of analysis would be very doable given the models selected, but I have significant concerns about the choices made in this particular study, and the descriptions here lack enough detail to fully evaluate the outcomes. The information provided, however, suggests that there are some serious limitations in how the analysis was conducted.

On the health impact analysis, it is unclear which functions are used, whether analysis was done for ozone as well as PM. In particular, the literature lacks the most recent citations. For example, the recent results of Turner et al. (2016) suggest a larger impact of long-term ozone on mortality relative to PM2.5 than the functions used int his study. If the authors wish to make a point about the relative impacts, they should at least

discuss the implications of different choices of exposure-response functions.

With respect to the GEOS-Chem simulations, there is a lack of detail in explaining the simulations. Though GEOS-Chem is a well known model, the authors should provide some information on whether this particular version can capture the chemistry of ozone in China (whether this be through their own work or through citations to the same version of GEOS-Chem). Basic information is missing, for example the meteorological data. I assume that this is a nested-grid simulation, given the resolution and version of GEOS-Chem used; the description lacks the appropriate citations for this as well as details on how boundary conditions were used, as there are several different versions in the literature.

The economics results are described in ways that do not make sense with economic intuition. In particular, for the ACP audience, it would be useful to discuss the basis for using CGE vs. VSL. It is unclear whether the authors intend to use these together, and how they can justify this choice given the very different economic assumptions made in these two different approaches.

Overall, while the paper addresses an important topic with well-regarded models, the implementation has some major issues.

Specific comments:

Abstract, line 27-29: I assume that these numbers are talking about ozone only damages, but the result implies that analysis was conducted for PM2.5 as well. This should be clarified.

Introduction: It would be useful here to review other studies of air pollution which use CGE methods, and the pros and cons of using such approaches, relevant to this particular study.

Line 95-96: What previous study is being referred to here?

Table A1: I am confused by Table A1 here. It refers to concentration-response functions

from ozone related health impacts in the title, but the sources (e.g. Apte et al., 2015) refer to PM2.5. This could be clarified. I would suggest that the particular studies be credited in an additional column of the table, and the specific endpoints (O3 or PM) be identified.

Line 109-110: This is not a correct summary of the reference cited. In fact, Berman et al. (2012) note that the US EPA science assessment identify no threshold for the relationship between ozone exposure and premature mortality.

Line 110-112: It would be useful to recap the methods here. In particular, the methods for PM2.5 should be different from ozone, given the differences in types of outcomes. It is not clear how mortalities are covered in the CGE application. It does not really make economic sense for the morbidity to be evaluated using CGE and mortalities using VSL.

Line 187: Is this actually showing a realistic result, given the nonlinearities in ozone formation? Also, I'm not sure that the "natural background" is accurate as this level of ozone is not 'natural' in the sense of non-anthropogenic - non-China background would be a more accurate term.