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Interactive comment on "Will the role of intercontinental transport change in a changing climate?" *by* T. Glotfelty et al.

Anonymous Referee #2

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The authors employ a global coupled chemistry-climate model to study the effects of intercontinental pollution transport, specifically of East Asian Anthropogenic Emissions (EAAE), on global atmospheric composition and climate. Unsurprisingly, the largest influences of EAAE are seen over East Asia itself, but the effects of EAAE are also shown to be global in nature, with large effects seen downwind of East Asia, as far away as the North American continent.

The paper is generally well written, and the work is usually adequately described, although there are a number of exceptions to this. I also have a number of serious concerns about some methodological aspects of the work which appear to be glossed over in the current manuscript. All in all, the manuscript falls short in a number of important areas, and must undergo major revision before it can be reconsidered for

publication in ACP. Detailed comments follow.

Abstract

A nice summary of the work is presented, including key quantitative results. One thing which I find missing from the abstract though is a short mention of how the contributions of EAAEs are determined. There are a number of possible approaches for doing this: tagging methods; adjoint methods; and perturbation methods. This study uses a zeroing-out approach (an extreme form of the perturbation method). The chosen methodology should be mentioned in the abstract to help orient the reader.

Section 1: Introduction

This section gives a very comprehensive overview of the state of the literature, providing ample background for the work which follows. The authors could however consider adding a brief mention of tagging and adjoint approaches, with appropriate references, in order to better place their work in context.

Section 2: Experimental Design

Unlike the other anonymous reviewer, I did not find the reasoning behind the choice of the years 2001 and 2050 easy to follow. Why have the authors not performed the zeroing-out of the EAAEs for of the simulated years (including 2010, 2020, 2030, and 2040)? My guess would be that the authors wished to avoid the extra computational expense of running these extra model cases, but I did not see this stated explicitly anywhere.

Related to this is the question of having enough simulation data to obtain reliable statistics about the magnitude of climate feedbacks. When performing time-slice experiments with a coupled model, such as the authors do in this case, it is standard practice to run the model for multiple years, and present an average climatology, rather than just results from a single run. When the model forcing is changed (for example, by zeroing out particular emissions), the model "weather" will also change in response to that.

Comparison of two such simulations must be done in such a way as to avoid simply comparing these different weather states. Such comparison is usually done by comparing climatological averages over multiple years. For example, the recent ACCMIP project required modelling groups to perform simulations of at least 4 years for each time slice (most groups chose 10, see Lamarque et al. (2013), Geoscientific Model Development), and in another study looking at the climatic forcing of emissions perturbations, Folberth et al. (2012), Urban Climate, compared 10-year simulations with and without the emission perturbations. The authors of the present manuscript must justify their approach of (seemingly) comparing model results from only a single three month (MAM) period of data, rather than averaging 10 years of MAM data with the different forcings (with and without EAAEs). A clear justification, and the implications of the chosen approach for the interpretation of their results is a prerequisite for final publication in ACP.

Another criticism of the methodology employed in the present manuscript is the decision to completely zero-out all EAAEs. Especially in the case of ozone, this can potentially lead to significant nonlinearities related to the ozone production chemistry. In the HTAP project, to which the authors refer, the magnitude of the emission perturbations was chosen to be 20% precisely in order to avoid these chemical nonlinearities. The authors of the present manuscript offer no comparison of their 100% perturbation approach with the more conservative 20% approach, and provide no discussion at all of the issue of chemical nonlinearity. Appropriate treatment of this issue is mandatory before publication in ACP can be considered.

Section 3: Representativeness of 2001 and 2050

See my comments above about the choice of 2001 and 2050. While reading this section, I started off wondering why the authors needed to establish the representativeness of these years at all. By the end of the section I was none the wiser. Perhaps the authors feel this is necessary to justify the lack of climatological statistics from decadal simulations (see above), but in any case, the rationale for establishing the representa-

tiveness of these years must be made clearer.

In the second paragraph of this section, the authors refer to a four degree warm bias of their model compared to a well-evaluated climate model. They appear to justify this by mentioning that their model is also biased four degrees warm relative to observations! A four degree bias in the model seems very extreme to me. In this case the model must be simulating a completely different climate to the present day. To me this casts grave doubt on the suitability of trusting this model for any purpose whatsoever, and could represent grounds for rejecting the manuscript outright. Have I misunderstood this? In any case, the authors should put their model bias in the context of the general spread in model biases when simulating present-day and future climates. Such information should be readily available through datasets such as the CMPI5 model intercomparison.

Section 4: Changes in future emissions

Unlike the other anonymous reviewer, I did not find this section very clear at all. To start with, it would be very helpful to the reader to have a reminder in the very first sentence of this section that the future emissions are based on growth factors taken from the A1B scenario. The final paragraph, in which the future emissions are compared to the well established RCP emissions scenarios, does not read clearly at all. Since much of the change in the future impacts of EAAEs is determined by the future changes in emissions (Section 5), it is crucial to put the emissions used in this study in the context of the currently most widely used emissions scenarios, and the authors must do a better job of this. It would be very useful to see timeseries plots of the evolution of the different pollutant emissions for at least global and East Asian totals.

Page 26500, Line 16: RCP stands REPRESENTATIVE Concentration Pathway (not REGIONAL Concentration Pathway as the authors state).

The terms “optimistic” and “pessimistic” should be explained. Are the authors referring to the differences in total radiative forcing between different scenarios here, or to the magnitude of the trends in air pollution emissions? High radiative forcing in RCP8.5 is called "pessimistic", while an increase in air pollutant emissions in "China and some parts of Africa" is considered "optimistic". This is confusing.

Page 26500, Line 25: "The RCP8.5 scenario more closely matches...". More closely than what?

Page 26500, Lines 25-29: This is confusing, first the authors state that emissions of NOx and VOC largely increase in East Asia and North America, then later in the same sentence they state that emissions of most species except NH3 decrease in North America. This is self-contradictory.

Sections 5 and 6

The quantitative results of the study are generally well explained, but based on the methodological shortcomings identified above, I have to wonder how reliable they are.

Concluding remarks

"Controlling EAAEs can reduce...". I think it is a bit of a stretch to make any firm conclusions about what can be achieved by controlling EAAEs based on the results of zeroing-out these emissions. A complete zeroing-out is totally infeasible from a policy point of view. Of course a 100% reduction in anthropogenic emissions can reduce pollutant concentrations "by up to 100%", but this should not be news.

The effects seen over Scandinavia may be just due to a comparison of different weather states under different forcings (see above). I would need to see comparison of climatological averages before believing this conclusion.

The authors should focus on the contribution of EAAEs to global average composition and to the downwind North American continent, which are more likely to be robust results.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 26489, 2013.