Dear Referee,

Hi! Thanks a lot for your report, which is very detailed. Your comments are very valuable for us. We agree with you that we should revise the paper very carefully. However, we don't think it is an impossible task. Because we still believe that our paper with highlights is valuable and original on the whole. Please believe that we have the ability to make it. Our replies to your comments are as follows.

1) The authors need to provide a clearer theoretical motivation of why carbon emissions are supposed to be spatially dependent as well as why neighboring characteristics/explanatory variables are supposed to be spatially dependent. This argument relates to the core message of the paper by Gibbons & Overman (2012). Although you give some indications regarding the theoretical motivation in the introduction on p. 4 (population migration and industrial transfer), the argumentation is not quite clear to me and lacks literature references.

Reply:

Yes. We will provide a clearer theoretical motivation based on several important literatures and add some related literature references.

2) You should at least estimate your model in a non-spatial formulation as a first step and check whether the residuals are spatially correlated. This would be 'good practice' as Gibbons & Overman (2012) put it (see p. 17). Perfectly relating to this suggestion, I found a paper by Zhengxia He et al. (2016) who estimate a non-spatial panel model closely related to yours (extended STIRPAT) with the very same data for the 29 provinces from 1995-2013. You happen to quote a different paper of these authors but not this one which, if you are not aware of it, is worth looking into as a starting point for my suggestion.

Reply:

Yes, I know it is good to estimate the model in a non-spatial formulation as a first step. But I didn't think it was essential, because I also found some good papers didn't do it at all. So considering the space limitation, in the paper this step is omitted. Anyway we really value your opinion. So I will think it over again after studying more related papers.

Thank you for providing the suggestions and references.

3) Regarding the spatial autocorrelation tests it would maybe also good to show the LISA significance maps (p-values for the local Moran's) besides the LISA cluster maps you provided in Fig. 3-6. Also, the names of the provinces in the maps are helpful since you elaborate on specific province results on p. 18.

Reply:

Yes, you are right that we should supplement with p-values for the local Moran's, which can make this part more elaborate.

Also, we need to add the names of Chinese provinces. The names of the provinces hadn't been shown in the map because we thought the readers were familiar with them. But now we realize that many foreign readers are not familiar with them at all.

4) Regarding the estimation results, I was wondering whether you have an identification problem given the number of observations in your sample. In your case, you exploit the asymptotics via the spatial domain. Therefore, the question is whether the regional dimension is “large enough”? Maybe you could find something in the spatial panel model literature about this and then at least elaborate a bit on that issue. Moreover, the question in the literature arises whether the spatial panel model is then compatible with fixed effects (see Anselin (2001) versus Elhorst (2010)). This should be mentioned and maybe you should check whether there exist significant differences among your coefficient estimates for your three models with and without fixed effects (small time dimension means small variation for identification) and thus the effects of variables with no or little change over time cannot be identified. Florax and Rey (1995) also discuss whether the time-series dimension is too small for efficient estimation of the covariance or parameters.

Reply:

Firstly, after reading some previous researches I think the time-series dimension is large enough. If necessary, I can provide the references to support my view. Secondly, the samples of Chinese provinces are not random but limited to certain individual provinces, and so the models with fixed effects should be better (Baltagi, 2001). Thirdly, you are right that I should add some discussion about whether there exist significant differences among our coefficient estimates for the three models.

5) why I do not understand why you include both the urbanization rate *UR* as well as the urban primary index *US* in your model specification. If there is a valid argument for including both I am wondering whether they are collinear, did you test for multicollinearity?

Reply:

Firstly, The urbanization rate UR used is very common in this study field. But I don't think it is enough. And I want to study the effect of urbanization on carbon emission from different perspectives. So the model specification includes the urban primary index US. I understand that it need to add more explanation for the underlying theory of urbanization and city size distribution.

Secondly, I did test for multicollinearity and the two variables are not collinear. I am sorry for omitting it because of the space limitation.

6) The entire paper is quite hard to read and follow. The structure needs to be thought over from section 3 on. For example, is it necessary to derive the general forms of spatial panel

models in an extra section? The contents of section 4 and 5 probably do not need own sections.

Reply:

Yes. We will optimize its structure according to your suggestions.

7) Sentences are often too long and not linked to one another. The paper needs to be better cast in the context of other studies like the one of Liu et al. (2014), i.e. what is the contribution to this specific paper? In the beginning and elsewhere the authors need to use the occasion to educate the reader by elaborating on meanings here and there (f. e. STIRPAT model is mentioned for the first time but not explained, what are the important meanings of city size with literature references). Just helping the reader get from A to B. Many parts are far too long and you lose the reader by jumping around so many topics/thoughts.

Reply:

Firstly, I will shorten a few long sentences. Secondly, I agree that I should introduce more earlier work like the one of Liu et al. Thirdly, we just gave a few concise introductions for STIRPAT, because STIRPAT Model is classical and well-known and the space is limited. Fourthly, I agree that some parts need be more succinct.

8) finally the formatting is not appropriate. I do not want to be petty but it makes the piece overall hard to read. For instance the last line of Table 1 is out of range and not readable. Equation (9) and (10) has the same left hand side, how can that be if they show short-term and long-term effects? It is R2 but sigma^2. A sentence ends with a period (p. 22 top).

Reply:

Firstly, I will improve the format. Secondly, the two equations are cited from the paper of J. Paul Elhorst (2012), which title is "Dynamic spatial panels: models, methods, and inferences". I am sure they are right.

Thanks again for your comments and suggestion. There are my replies above, and I have answered most of your questions. But very soon I will reply the rest questions which are very few.

Best,

Honglei