

Referee Report on “Carbon Emission Effect of Urbanization at Regional Level: Empirical Evidence from China”

The authors analyze the effects of urbanization on carbon emissions in order to support local policymakers in developing low-carbon urbanization strategies. They substantiate their recommendations with an empirical test for China by applying a spatial panel regression analysis for 29 provinces and the years 2002 to 2013. The authors find spatial dependence in carbon emissions as well as in urbanization. Thus, they urge regional policymakers to collaborate on designing low-carbon urbanization strategies in order to account for spillover effects. Finally, they stress the importance to consider both short-term and long-term strategies for maintaining low-carbon urbanization, especially with regard to the optimal size of a city.

The paper analyzes a very important topic which is reasonably introduced at the beginning. However, I do not consider the paper worthy of publication in the *Economics Journal* unless it is revised considerably. The major reasons are as follows.

1) **The empirical strategy** is questionable and not motivated appropriately.

First of all, 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.

Secondly, 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.

Thirdly, if you still decide on a spatial formulation you need to elaborate more on why exactly you choose the SDPDM. Of course, you give a brief explanation on the paragraph starting at the bottom of p. 11. But only saying that the SDPDM is technically able to cover all aspects without elaborating on why one would need to cover all aspects is not too convincing. My point goes directly back to my first one of theoretically motivating the regression model: why would you assume a time lag of the dependent variable or why not? Why also a space-time lag dependent variable or not? Why also a spatial lag of all the explanatory variables? Although I am not an expert in econometrics, my impression is that you (intentionally or unintentionally) chose the most complex spatial regression model available. It has to be made clear why. Only because no one else applied it to the underlying data is not a sufficient motivation. This complexity is also reflected by your interpretation results which I comment on next.

2) **The empirical results** and their interpretation.

First of all, 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.

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.

Further, 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?

The R^2 of your model is probably that high because the time lag or space-time lag of the dependent variable is very persistent. There are mistakes in Table 3 regarding the significance levels and T-stat. For example, in the second entry of model (1) 0.0028^{***} does not correspond to a T-stat of 0.46? In your preferred model(2), your variable of interest $\ln(US)$ has a T-stat of 0.034 but is significant at 5%? This makes it hard to check whether your results are valid. You should also give an interpretation of the value of your estimated coefficients, given that all is in logs I assume these are all percentage changes? Do you find them high or low compared to other studies? Following p. 21, you give a very (and too) long explanation of why the variable UR might have a positive or negative sign, such as household size, industry structure etc. Why not include further variables that cover these aspects since you quote Yu Liu et al. (2014), who include variables such as industry structure and energy intensity? In general, I do not quite see the contribution of your paper when looking at the publication of Yu Liu et al. (2014) who also apply a spatial durbin panel model for Chinese regions? Except that you include the urban primary index which I am not sure suffices for a significant contribution to this existing literature, especially since its significance is much weaker than the urbanization rate in your results. Given the direct and indirect short-term and long-term effects, would you say that also find a U-shaped relationship? By the way, regarding these multipliers you must definitely have a look at Anselin et. al (2008) and quote them.

Last but not least, 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.

Further, 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.

And 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 R^2 but σ^2 . A sentence ends with a period (p. 22 top).

All in all, the paper would need considerable work in order to be publication worthy.

References

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