Dear Referee,

Hi! There are the other replies to your comments. Thanks a lot for your report again!

1. 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.

Reply:

I choose to set up such kind of model because of several reasons. Firstly, the Dynamic Spatial Durbin Panel data models we use have the advantages of panel data and spatial econometric approach. Secondly, such models cover all aspects, which is meaningful and useful for my research and I can give some related theoretical analysis . Thirdly, I can set up the model on the basis of some theory and evidence.

2. 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 urban primary index, 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.

Reply:

I think our paper have several different respects compared with the paper of Yu Liu et al.. Firstly, our study is aimed specially at the analysis of urbanization's influence rather than extensive analysis of all factors. Although to a certain extent, the spatial panel data model and the extended STRPAT Model have been used to analyze various influencing factors of carbon emission in some researches, only a few of them touched on the factor of urbanization, and most of them were rarely aimed specially at the urbanization's influence and failed to make an all-round and deep-going analysis on the carbon emission effect of urbanization. Secondly, our spatial Durbin Panel model is dynamic, which is different from the static spatial Durbin Panel model of Yu Liu et al. The dynamic model can express the statistical relationship between the current variable and the previous one to reflect the continuity of carbon emission change more in line with reality. Thirdly, I think it is very necessary to add the variable of urban primary index. Besides the urbanization level, another related important question being discussed is whether cities' sizes meet national or regional strategies for reducing energy consumption and greenhouse gas emissions during the urbanization process. Besides, the average commuting distance may be a major contribution to the relation between greenhouse gas emissions and city size (eg. Bento et al., 2006; Brownstone and Golob, 2009; Glaeser and Kahn, 2010) in that compact cities might be more green because of shorter trip distances, on average. There are conflicting results as to the relation between city size, CO_2 emissions and environmental footprint of cities (eg. Dodman, 2009; Glaeser and Kahn, 2010; Fragkias et al., 2013; Oliveira et al., 2014). In fact, it is as yet unclear if large cities are more energy efficient and more environmentally friendly than small cities. For example, two recent studies from North American cities reached different conclusions as to the scaling relation between city size and CO_2 emissions (Fragkias et al., 2013; Oliveira et al., 2014). However, most of

the studies belong to the research field of city size's influences on carbon emission only from local respective. So in this paper the variable of city size distribution but not city size is selected to measure the holistic level of city size during the urbanization. Obviously, if the important factor was overlooked, the results might be adversely affected.