

## Review of “Governance, vulnerability to climate change, and green growth: international evidence”

MS 2140

May 10<sup>th</sup>, 2017

### Summary:

The authors’ investigate the relationship between governance, vulnerability to climate change, income, and environmental quality. They use multi-country data for PM2.5, GDP per capita, an index Economic Freedom, an index of vulnerability to climate change, and energy use per capita to estimate a panel data regression in growth rates. They conclude that Economic Freedom decreases PM2.5 concentrations and vulnerability to climate change increases PM2.5 concentrations.

### Assessment:

This study represents an extremely minor addition to past studies on the topic; their contribution is the addition of a measure of Vulnerability to Climate Change. However, after reading the manuscript, I fail to see the theoretical causal connection between how a change in a country’s vulnerability to climate change would result in a change in its concentrations of PM2.5 (a local air pollutant not related to anthropogenic climate change). I do not think you can draw any conclusions about policy implications without first justifying that there is, at the very least, a theoretical causal connection. There are also several other major issues that I outline in comments below.

1. I am concerned about potential incidental plagiarism in the Literature Review and possibly throughout the rest of the paper. The authors’ often attempt to paraphrase, but only change one or two words, resulting in sentences that are too close to what is written in the original source.

For example, on page 5, the authors’ state “Wood and Herzog (2014) assert that economic freedom, one measure of the quality of economic institutions, plays a critical role in the linkage between economic development and environmental quality. Failure to incorporate this factor in economic models of pollution could thus cause spurious results.”

These two sentences are too similar to what Wood and Herzog (2014) wrote:

“...that economic freedom (a measure of the quality of economic institutions) plays a particularly important role in the causal relationship between economic development and environmental quality. Failing to account for it in economic models of pollution can lead to spurious results.” (Page 5 of Wood and Herzog (2014))

This problem occurs again on Page 6 where the authors’ write:

“...refers to how freer trade increases output, which in turn increases pollution. More trade also changes the composition of industry, which can have either a positive or negative effect on pollution, depending on factor endowments. Antweiler et al. (2001) showed that freer trade can lead to an overall cleaner environment for some pollutants.”

These three sentences share too much in common with sentences in paragraph 2 and 3 on page 7 of Wood and Herzog (2014):

“...refers to how freer trade leads to increased output, which in turn leads to more pollution, all else equal.”

“...more trade also leads to a change in the composition of industry, which, depending on factor endowments, can have a positive or negative effect on pollution.”

“Freer trade can lead to an overall cleaner environment for some pollutants...”

I would recommend that the authors review guidelines on how to properly paraphrase. These are just the specific instances that I noticed, but it may be an issue throughout the paper.

2. On page 7, paragraph 3, the authors' state: "While these studies acknowledge that corruption, political institutions, or social structure are instrumental in accurately measuring the connection between economic activity and environmental quality, they do not fully account for those factors in their analysis (see, for example, Panayotou, 1997; Barret and Grady, 2000; Bhattarai and Hammig, 2001; Bernauer and Koubi, 2009; Leita0, 2010; Lin and Liscow, 2013). As such, our study contributes to the literature by more explicitly incorporating governance into the empirical analysis."

First: This paragraph bears many similarities to sentences in paragraph 3 on page 1 of Wood and Herzog (2014).

Second: The claim is false and indicates that the authors' did not read the papers cited in sufficient detail. For example, Bernauer and Koubi (2009) control for the quality of governance using the Polity index. Wood and Herzog (2014) include both the Economic Freedom of the World index and the Polity index in their regression specifications.

Third: The authors' of the paper under review then decide on page 10, without reference to what was done in the previous literature, that they choose the "Index of Economic Freedom as a proxy for the governance"; if anything, it is the current authors' who "do not fully account for" corruption, political institutions, or social structure. This also highlights that their contribution is purely adding an indicator of vulnerability to climate change to the analysis.

3. Page 10: The author's state "The novelty of this structure is to explicitly consider the possible impact of the level of energy consumption on the relationship between governance, vulnerability to climate change and air quality". There are 1000s of EKC studies, I find it difficult

to believe that this is the first study that included adding energy consumption to the regressions.

4. Page 16: The authors' mention Granger causality, however, their regression model on page 9 is not a dynamic panel data model, so they cannot test for Granger "causality".
5. Wood and Herzog (2014) highlight potential endogeneity between variables and estimate both an OLS model and a GMM-IV model; whereas, the authors' of the present study do not address the issue other than through testing for multi-collinearity. Furthermore, the authors' of the current study ignore the possibility of a non-linear relationship between income and PM2.5, despite the fact that Wood and Herzog (2014) find a non-linear relationship between income and PM10.
6. Page 19: The effect of the Governance variable is only statistically significant for the high-income sub-sample. The failure to reject the null that the coefficients are zero in the other sub-samples of countries raises questions about the robustness of your conclusions. Basically you are unable to identify an effect for changes in economic freedom on changes in concentrations for the other sub-samples of countries. This suggests to me that it is not economic freedom that matters, but maybe something else that only the high income countries have? Though it could just be a product of restricting the sample too much or of looking at changes in economic freedom rather than levels.
7. Table 3 on page 20 and 21: This table is extremely difficult to read. The authors' need to reformat them: All cells should not have borders.