

Although I find the research question of the submitted manuscript highly interesting, I was unfortunately not convinced that the empirical analysis performed by the authors and their interpretation of the results can contribute significantly to our understanding of corruption and its consequences. Thus, I cannot recommend this article for publication in Economics. There is a range of problems with this article, but I will try to summarize the more important ones in order to justify my conclusion:

First, the theoretical model the authors have in mind does not become clear. Why and how should corruption in the home country exactly lead to different behavior and what exactly is that behavior?

The authors seem to equate apprehension and criminal activity. This is simply not true. There is tons of empirical evidence, particularly for the US, showing (racial) discrimination in law enforcement and the justice system. It is totally unclear that more apprehensions mean more violations of the law. It is an additional step to argue that these (assumed) violations of the law have to do with corruption. I don't believe that this is true for 99% of these cases. If the authors only want to link home country corruption to crime, they have to rewrite the article. Here we come back to the missing clearly spelled out theoretical model.

Are the authors really suggesting that the immigrants to the US are a representative or at least a comparable sample of the population of their home countries? Immigrants from corrupt countries might simply be more likely to be refugees or fleeing from poverty and discrimination than immigrants say from OECD countries. It is not surprising that they might be more likely to violate laws (or at least be apprehended), but that does not imply that people from these countries generally are. This criticism would also be in line with some of the results for the control variables.

The empirical analysis is not convincing. Without going through all the problems, here are some of the more easily avoidable ones:

- Extreme cases should not be excluded without further explanation of why or in what sense they constitute outliers. (What test statistic exactly did identify these countries?) Alternatively, one should maybe show results from robust regression, which are not easily manipulated to produce specific outcomes.
- The exact choice of independent variables needs motivation. Why these? Why not others? What were these decisions based on?
- It is highly questionable to eliminate multicollinearity by transforming (dichotomizing) independent variables. If the transformed variable is part of the data generating process in its original form, the author is misattributing some of its association with the dependent variable to other independent variables. This is at best hiding the multicollinearity problem, but not solving it.

Are the authors really suggesting to screen immigrants for "immoral attitudes" and base this test on the level of perceived corruption in their home countries??

Minor problems:

I think the authors are not very precise regarding facts. To give just two examples: Economists do not agree that corruption is "one of the main factors" explaining differences in income. See, e.g.,

Gundlach and Paldam (2009) in Economics Letters. Then, the data by Alesina et al. is in contrast to what the authors are claiming not collected in 2003 (the year the article was published) but sometimes even decades earlier. These things look unprofessional and should not happen.

The language and style of the manuscript need to be significantly improved, although one can admittedly understand what the authors are saying.

Citations have to be correct. Dimant (2013) is Dimant et al. (2013). Robert W. Fairlie is cited as W.F. Robert...

Links to published articles should not be listed in the references.

The literature on corruption and growth in the first section of the article is largely outdated.

What is “multi-front regression analysis”?