

We would like to thank the reviewer for his/her apt and constructive criticisms. We agree without exception with all of the criticisms made and revised our paper along the lines suggested. This would definitely add value to our paper.

Main comments:

· **_Endogeneity problem:** one of the main problems of the paper is that the reader suspects almost immediately an endogeneity problem between the two variables of interest. Hence, it can be the case that energy consumption will determine the size of the informal sector. This effect will certainly go through the quality of energy production and delivery. I am aware of the difficulty to tackle this endogeneity problem; however, the author should at least discuss this very important issue.

Response: Arguably one could expect a certain degree of endogeneity, but the fact that various different specifications lead to similar outcomes might somewhat alleviate this problem. Moreover, we also have run different regressions using the GMM estimator a la Arellano and Bond (1991) where one period lagged values of the independent variables are used as instruments for their levels and ended up with qualitatively similar result. We do not report these regressions in the paper but mention in a footnote that interested readers might obtain them from the corresponding author of our paper.

Determinant of energy use: what is quite is astonishing to me is that, according to the author, energy consumption is only a function of the size of the informal economy. The authors invite potential readers to interpret their results "ceteris paribus". I deeply suspect that there are other important variables, which drive energy consumption. I believe that very high R squares presented in various regressions are a direct consequence of important missing explanatory variables in the econometric models. The authors should at least take into account some control variables.

Response: First we agree with the reviewer that using the term *ceteris paribus* is unnecessarily strong in our case, hence we removed this term from the revised paper. However we disagree with the reviewer that very high R squares presented in various regressions are a direct consequence of important missing explanatory variables in the econometric models. Missing explanatory variables might have lead to low R squared values not the other way around as adding more variables to the RHS of a regression (even not significant ones) always increases the R-square values. Moreover, the fact that we use lagged values of the informal sector and energy intensity on the RHS might further alleviate this problem as current values of different potential factors that might affect energy intensity might already be (if not fully partially) explained by lagged values of informal sector and energy intensity. Nevertheless for the linear specification we have added results of one more regression with (For all countries) GDP per-capita on the right hand side, as well. Notice that results are qualitatively similar in this case. We also have experimented some regressions with growth rate of GDP as well as some institutional quality variables but ended up with strikingly similar results. This might be due to the fact that the already controlled lagged informal sector size and energy use intensity variables capture the variation coming from the variation in GDP per-capita (or growth and institutional quality). In a footnote we mention that these additional results are available upon request from the corresponding author.

· **_Theoretical model:** Considering that the findings of this paper are strongly predictable, the authors should present or at least discuss some theoretical model. The authors should add a theoretical section to this paper. They should model the energy consumption and the possible relationship with the size of the informal economy. Then the authors should present the main findings of their

theoretical model (the negative relationship) and test them empirically.

Response: We now have a separate chapter in which we discuss a theoretical construct.

Particular Remarks (still important):

· Definition of variables of interest: the author should be much more rigorous and explain in detail how the two variables of interest are constructed. For example, IS is sometime described as a share of the formal sector size and sometime as a share of GDP. The reader continuously questioned himself what the variables of interests really are. Is informal economy taken into account in the GDP? Is informal energy use taken in total energy consumption? The authors should define very clearly these variables.

Response: We now are clearer on the names and definition of the variables used in the empirical analysis.

· Policy implications must be strongly strengthened. Especially considering the implications of the non-linearity and asymmetry, which I believe are the main contribution of the paper. More precisely (page 19), the comparison with the link between informality and pollution is very interesting but should not be presented in the conclusion. Discussing the potential deregulation and scale effects should become an important contribution of the paper and not simply relegated in the conclusion.

Policy discussion section has been revised and strengthened.

· Descriptive statistics on the size of informal sector: The authors should present some descriptive statistics concerning informal sector size in various countries known for their specificity toward shadow economy. For example Switzerland, Sweden ... or Italy, Romania ... This should convince the reader of the quality of the measurement of the informal sector.

In a footnote we now refer to a forthcoming paper by Elgin and Schneider that exactly addresses this specific issue raised by the reviewer.

· Informal energy use: The author should discuss deeper this issue. Hence, informal sector often use energy illegally. How the paper takes into account this dimension?

We now discuss how to take informal energy usage into account. Actually, this fact highly supports our arguments.

· Asymmetry vs non linearity: the authors try to convince us that both asymmetry and non-linearity are observed between energy consumption and informal sector size. What is quite puzzling is that Asymmetry is only significant for a sub group of countries whereas non-linearity is only significant at the aggregate level. The authors must be very cautious when they interpret both results.

We have revised our interpretation of the non-linearity and asymmetry results following the suggestion of the reviewer.

· Elasticity interpretation: in table 4 the parameter estimate for IS_{t-1} in emerging countries is -0.13. If I understood well the econometric specification both explained and explanatory variables are expressed in log. Therefore, the associated parameter must interpret as an elasticity. Therefore, a one percent increase in the size of the informal sector generates a 0.13 percent decrease in energy intensity. And not 13% ! This mistake should be corrected. Hence, this mistake is typically an

undergraduate student error and has no room in a research paper.

- We corrected this mistake in the revision.