



SDI Review Form 1.6

Journal Name:	<u>British Journal of Economics, Management & Trade</u>
Manuscript Number:	2014_BJEMT_15560
Title of the Manuscript:	Improved Electricity Supply: What Factors Determine How Much Domestic Customers are willing to Pay in Tamale Urban and Peri-Urban Areas?
Type of the Article	Original Research Article

General guideline for Peer Review process:

This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty**', provided the manuscript is scientifically robust and technically sound.

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PART 1: Review Comments

	Reviewer's comment	Author's comment <i>(if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)</i>
<u>Compulsory</u> REVISION comments	Presumably, the reason that 17 observations were deleted is because the natural log of 0 is undefined. Thus, a double-log model could not be used if these observations were included. Yet, ignoring the observations will bias the estimated coefficients. Why not use a Tobit model (for example) or a transformation that will allow use of the observations associated with 0 WTP?	The researchers initially wanted to use choice modelling such as binary logit, or probit or a selection model (tobit) as suggested by the reviewer. As you are aware, choice models are suitable for data with nearly equal number of respondents choosing two categories of outcome (willing to pay and not willing to pay). In this research, 293 respondents were willing to pay whilst only 17 respondents were not willing to pay. Therefore, the data is skewed towards those who are willing to pay and this can lead to biasness of the estimation if choice models are used. Meanwhile, tobit model is a selection model. The first stage modelling will not produce estimates that are BLUE since 293 (willingness to pay respondents) far outweigh 17 (non-willingness to pay respondents). Transformation of the data with zero WTP observations is a welcoming suggestion and would be considered in future research of this nature.
<u>Minor</u> REVISION comments	The English needs some further proofreading. Clarify whether the amounts on p. 2, lines 44-45 are “per year” or total.	Proofreading has been done and all the minor grammatical errors corrected. It has been corrected Corrections have been made



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	There is a reference to B_0 on line 131 of p. 4, but that point is not identified in the Fig. II. The link between (the confusing) theory presented in Sec. 3.2 and the model which is estimated (Eq. 8) is not clear. Breusch-Pagan is a test for heteroscedasticity, not autocorrelation, as stated on p. 5, line 196.	The model is not all that confusing. One needs to read it and conceptualise the linkages. The reviewer's observation is true. It was a mistake. It has been corrected
<u>Optional/General</u> comments	Overall, this analysis is on the right track, and much better than most of the submissions that I have reviewed for this journal. Nonetheless, some "clean up" is needed.	