

Review of: "Evaluation of Expenditure Control Measures on State Spending in Nigeria: An Empirical Approach with Internal Auditors of Ministries, Departments, and Agencies"

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Potential competing interests: No potential competing interests to declare.

The study covers an interesting topic of expenditure control, and I am thankful for the opportunity to review this paper. However, I have a few comments.

1. The introduction, literature review, and hypotheses development sections are too stretched. They can be summarized. For example, in the eighth paragraph of the introduction section, the budgetary policy is a part of fiscal policy, and the meaning of fiscal policy seems unnecessary. In the ninth paragraph, the 1st sentence and last sentence carry the same meaning and look repetitive. In the tenth paragraph, the need/demand for accountability and transparency is mentioned twice.
2. In the tenth paragraph, the following sentence is unclear- *"the primary role of auditing in expenditure control is not to detect fraud and error but to examine the books of account so as to determine whether or not they give/show a true and fair view of the state of affairs of the economy during the period under review"* If the books of accounts do not show a true and fair view, that will mean that there is some error or fraud. Right? So, detecting fraud or error is an internal part of the true and fair view.
3. The study is based on a single state named Katsina, which is located in Nigeria, and not on all the states of Nigeria. Hence, the title of the study is misleading.
4. Coming to the methodology, under the Population and Sample of the study section, the authors claim that they have used stratified sampling. However, in the abstract, they mentioned that they used purposive sampling. So, there is a mismatch among their claims on the sampling technique.
5. The study uses accounting controls, audit controls, and budget controls as independent variables. These three variables are endogenous in nature. However, the authors haven't tested for the endogeneity problem.
6. The study has neither presented the collinearity table nor the VIF values. Hence, a collinearity test is also expected.
7. Since the authors claim that they used stratified sampling, the responses may also be similar for a particular group and different for others. In other words, the model may suffer from heteroskedasticity and may need to be tested as well.
8. Since the authors are considering expenditure controls, the deviation between the budget and actual expenditure would be a better **dependent variable** than the actual expenditure. This may also be a reason for finding no relationship between the dependent and independent variables.

I hope these comments will help the authors in re-developing the paper for publication.



Best wishes.