Reviewer's report

Title: Operating efficiency of public hospitals in Zambia: implications for the success of global health initiatives

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Reviewer: Dan Friesner

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General
This paper examines the technical, scale and congestion efficiency of 30 hospitals in Zambia. The authors find significant evidence of inefficiency, and based on their findings discuss some potential policy changes to improve the effectiveness of health care provision in that country.

I think the general goal of this paper is quite unique, and has the potential to be quite useful for policy-makers and academic researchers. However, the paper in its current form is not ready for publication. I have some serious concerns about the data collection process, the empirical methodology, and the general structure of the paper. I think all of these concerns can be overcome with an extremely thorough revision; however, the resulting paper will look very different than the one currently under review.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The introduction spends a significant amount of time discussing the major global public health initiatives, and how they are affected by hospital efficiency. However, this discussion is somewhat misleading, because there is no direct link between the efficiency analysis conducted in the paper and these public health initiatives. I understand the general motivation behind the discussion, but the analysis itself does not use measures such as AIDS tests, services to AIDS patients, malaria treatments, etc. as the inputs or outputs in the analysis. As such, there is no understanding gained about how hospital efficiency leads to increased effectiveness of these policies. Certainly there will be a correlation; that is obvious. However, the important policy issue in my mind is some quantitative measure of the magnitude of the correlation between hospital efficiency and program effectiveness. And your paper does not address this issue, thus the discussion in the intro should be downplayed.

2. The introduction and specification of the DEA model needs to be completely re-written and expanded, for several reasons. First, the presentation of DEA would not satisfy any reader, whether experienced with DEA or not. A reader well versed in DEA (who knows the algorithm in explicit detail) will not like to see the actual LP formulation in the body of the paper, and this can instead be removed and placed in an Appendix if need be. These readers are more interested in the assumptions about the setup of the LP program. For example, why do you choose an input oriented program? While commonly used in health care, the fact that managers have a set budget which they try to exhaust implies that an input oriented technical efficiency measure might not be appropriate. Instead, one might wish to employ an indirect distance function measure a la Fare and Primont (1995). How are the slacks calculated? Are they calculated residually, or do they use some sort of two-step or multi-step algorithm?

Your discussion doesn’t satisfy someone unfamiliar with DEA either. For these readers, it is common to add a generic discussion to the text with a couple of figures that depict the input requirement set (and motivate better why it is an input requirement set and how that corresponds to an input-oriented LP formulation), and the radial distances calculated by the LP algorithm. A similar figure and discussion may also be used to illustrate the scale efficiency calculation. Lastly, for non-DEA readers, you need to motivate the assumption of weak versus strong disposability of outputs.

Again, to strike a good balance I suggest adding 2-3 figures and verbal discussion in the text, with an appendix displaying the actual LP formulation.

3. I have concerns about the data set used. In particular, are the 30 hospitals used representative of the population of 100 hospitals? Is the mix of government and church hospitals similar to that of the 100? What about the mix of services provided? It may be that the data collected is truly representative, but without much more additional information in section 4.1, it is impossible for the reader to tell. And this is important,
especially if one is attempting to generalize the efficiency of hospitals (and the corresponding policy implications, as you do) to the entire country.

4. The selection of inputs and outputs is troublesome for two reasons. Moreover, these two problems work at cross-purposes. It will be difficult to address them both with the available data, and failure to do so significantly reduces the usefulness of the paper. First, there are a lot of inputs and outputs relative to the sample size. This implies substantial bias due to the “curse of dimensionality”. That is, all of the efficiency estimates will be significantly inflated, both for efficient and inefficient firms. As such, it is tough to trust your results. At the same time, unless these hospitals perform a small handful of services, the complexity of outputs produced by a hospital, in general, requires a large number of inputs and outputs to be specified (more outputs are required, in particular, for your analysis) to appropriately capture the mix of services provided by each hospital. Otherwise, firms may be artificially measured as efficient or inefficient, when in fact the opposite is true. It is just that your data are too aggregated to capture the complexity of services.

5. You have no empirical measures of the quality of care in your analysis. If you look at the academic journals publishing most of the studies in this area over the past 5 years (for example, Health Economics and Health Care Management Science, among others) you will see a large number of articles that at least make an attempt to control for the quality of care. Many of these studies attempt to control for quality (its process and outcome aspects, anyway) through the use of casemix measures, and thus argue exactly the opposite of what you claim. (i.e., there is no need to control for casemix). Really, most of these studies try to adjust for casemix-based quality to partially address comment 4; that is, to account for the complexity of services, which in turn has a link to the quality of care. In any case, the only way you don’t need to control for case-mix and/or quality is if all hospitals in sample and population provide only a small handful of basic services, and do so in similar ways. But without an expanded discussion of the nature of Zambian hospitals, it is impossible to tell is whether this is the case.

6. There is no discussion of the outputs (in terms of justification, or how the relate to total hospital production) in section 4.2. Please add this to the text. Also, Section 4.3 consists only of 2 sentences. Please discuss your descriptive statistics in much more detail (3-4 paragraphs). I would also like to see a breakdown of hospitals based on discrete characteristics. For example, how many are church-owned versus government-owned?

7. Section 5.1: presentation of efficiency estimates. It is important to understand not just what the efficiency scores are for each hospital, but also what the individual characteristics of each hospital are that might drive the empirical results. For example, why is Thompson hospital inefficient relative to Kitwe Central? Is there a difference in ownership, or location (urban versus rural) or does one hospital face disproportionately more severe epidemiological conditions (more malaria outbreaks, higher incidences of HIV/AIDS infection, etc.)?

8. Continuing with 7, DEA is a rank-order procedure, so I don’t think your Hotelling test on page 15 is useful. Instead, one might want to conduct mean and median tests (and/or possibly cross-tabs based chi-square tests) to determine what other factors not controlled for in the DEA analysis impact efficiency (all three types of efficiency, not just VRS technical efficiency). For example, are church-owned hospitals more or less efficient than other hospitals? Depending on the information available, one could also test the issues mentioned (and others not mentioned) in 7. Please keep in mind that DEA results will not likely be normally distributed, so one should use nonparametric tests.

9. In the conclusions, please expand the limitations section of your study, especially as they relate to the small sample size issues (not just the paucity of inputs and outputs) discussed above. Also, please provide some recommendations for future work that are important extensions of your study. You might also want to address how your study’s findings and implications could be applied to other countries (possibly in sub-Saharan Africa) facing similar problems to those in Zambia.

Citation:

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
1. There are quite a few grammatical mistakes, especially at the beginning of the paper. The abstract itself has three typos, on line 6 (there is an incomplete sentence), line 7 (missing the word “not”) and 22 (“wastage” is not, to my knowledge, a word). There are others scattered throughout the first 10 pages of the paper. To me, this is not a crucial issue, but a good proof-reading should be conducted by the authors to be
on the safe side.

2. On page 5 (both full paragraphs) the authors should add a few citations to support their arguments. More citations should also be employed in the last paragraph on page 8.

3. On page 6, please change the discussion involving allocative inefficiency. You aren’t conducting a cost efficiency (and thus, an allocative efficiency) analysis because you don’t have input price data. You don’t mention the lack of input price data until much later in the paper, and this should be noted on page 6. As it is, the discussion is misleading and confusing. By defining allocative inefficiency, and given the wording of the paragraph, the reader is led to believe that an allocative inefficiency analysis might be conducted after the VRS technical efficiency analysis.

Discretionary Revisions (which the author can choose to ignore)

None.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests.