Reviewer’s report

Title: Impact of rural family physician program on child mortality rates in Iran: a time-series study

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Reviewer: Nancy Fullman

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Overall:

This is a study of potentially high utility for understanding the impact of primary care and insurance programs in rural Iran. Conducting retrospective evaluation analyses are often challenging, though very important and worth-while pursuits; they require careful consideration of how outcomes (dependent variables), possible drivers of change (independent variables), and confounding factors should be measured and assessed over time. The study, as presently written up, provides a foundation for assessing the association between child health outcomes (mortality in this case) and programs implemented. At the same time, prior to consideration for acceptance, it requires, at minimum, much more information regarding its methodological steps - and possibly reconsideration of its analytical approach. Further, the indirect and direct implications of causality, as supported by this analysis, should be substantially tempered; a retrospective analysis can, at best, show the strong relationships between various and outcomes, not that x program has significantly reduced child mortality (in general or by y amount). This is really important, lest the study overstate its results.

Beyond this higher-level comments, I have three overarching concerns about this study as it is currently presented. My first one involves the assessment of the rural family physician program impact, as a standalone intervention, versus the combination of the family physician program and insurance program. The background and discussion emphasize evaluating the effects of both, yet the analysis seems primarily focused on the existence of the family physician program. It seems important to include some kind of data on the uptake of insurance status or other components of the insurance program beyond behvarz density to assess the effects of both programs (as currently emphasized by the manuscript). My second major concern involves the time-series analysis, as it is not very clear how such a time-series assessment occurred. Based on the current description in the Methods, it does not appear that a time series of mortality estimates were generated, which is of particular concern given that at least one year of data (2007) was missing during the time period following program implementation (post-2005). Figures 1A-C showing apparent trends in various child mortality measures are disjointed, especially for neonatal mortality, which suggests that these data were not synthesized into a time series of estimates. If they were - which is certainly possible - a description of the time-series estimation process is need. My third major concern pertains to the decision to use child mortality measures as the dependent variables for the impact of the rural family physician program. Changes in mortality would likely to be very downstream to the implementation of the family physician and insurance programs and overall cascade of care; thus parsing out the associations between mortality trends
and the implementation of these programs are likely to be near impossible if other primary drivers of reduced mortality have been properly accounted for (e.g., increases in maternal educational attainment, improved coverage of vaccination for highly-fatal diseases). Data on indicators that are more likely reflect the impact of a rural family physician and insurance program implementation also may be less widely available (e.g., care-seeking behavior, insurance uptake, percentage of children who saw a family physician in the last x months). At minimum, some kind of discussion about why such indicators weren't used should be included in the manuscript.

Specific major comments by section:

Background

(1) Page 6, lines 91-93: The different types of indicators used to measure effects of these programs should be further elaborated on here, and why mortality rates were determined to be the best indicator used in the present analysis. While child mortality rates are used as an indicator of "health status in countries," its utility may not be as great for assessing the effects of a rural family physician program (and/or insurance program). Previous studies of the effects of such programs primarily focus on the programmatic uptake and indicators that more directly reflect program effects (care-seeking behavior and health-care utilization; diagnosis and/or treatment rates of high-priority conditions; insurance affiliation - for example, see Gakidou et al, 2006: http://www.sciencedirect.com/science/article/pii/S0140673606695688). Please provide more information as to why child mortality rates should be used as the primary dependent variables in this study.

Methods

(1) Page 6-7, lines 110-111: Behvarz density is defined as average number of behvarzes per 1000 rural inhabitants in each province. Are the number of behvarzes also limited to rural areas within each province? It is not clear if the numerator only represents behvarzes in rural areas. To assist with interpretation, please include what constitutes a rural area within a province, as it is not necessarily clear at the moment.

(2) Page 7, lines 113-114: Since the study is focused on rural areas, I imagine there may other sources of "wealth" beyond expenditures and income (e.g., other owned household commodities, such as livestock) that may more accurately reflect the relative wealth of households in rural areas (and thus how relative wealth status may contribute child health outcomes). Were the inclusion of such indicators considered? If so, why were they ultimately excluded? If not, can this be added to the PCA for the wealth index?

(3) Page 7, lines 117-118: What was the issue with 2007 data? Was it limited to child mortality, behvarzes, or did it apply to all indicators covered by the vital horoscopes and/or the statistics centre?
(4) Page 7, lines 118-119: What kind of inconsistencies occurred at the district level? Missingness, miscalcategorization of outcomes or behvarzes? It is not clear how performing the analyses at the provincial level, if the data are aggregated up from the district level, would fully address data inconsistencies across provinces unless the data inconsistencies were systematically addressed. Please provide more information on what these data inconsistencies involved and how they were addressed in the aggregation of district-level data up to provincial analyses.

(5) Page 7, line 127-129: Please explain the difference between annual incremental effect of the rural family physician project (AIE) and the effect of family physician project (FP). It's not immediately clear why both of these indicators are needed. Further, given their likely correlation, including both may be methodologically unsound. Also do these differ by province? Or was was the program implemented at the same time (2005) for every province?

(6) Page 7, lines 129-131: Perhaps it is a limitation of my methodological knowledge, but it is not clear to me why adding the four socioeconomic variables would increase the efficiency and power of the model. Their inclusion may reflect the association of socioeconomic indicators with reductions in child mortality (which there is a strong association), but I don't believe that's the same as model efficiency or power.

(7) Page 8, lines 134-144: Is the primary reason why a segmented regression model is used is because of the 2007 data missingness? Or was it applied for the 2005 implementation of the rural family physician program? (at minimum, this needs to be clarified). From what I understand of this model choice, it's primarily used when there are explicitly different relationships for an independent variable before and after a breakpoint. Conceptually, I'm not sure if this method should be used in this case, particularly when unified trends of each indicator could be synthesized over time through a model like spatiotemporal Gaussian process regression and then analyzed together to quantify their relationships over time (see Ng et al 2016: http://www.ajtmh.org/content/early/2016/02/04/ajtmh.15-0315.long). Further, trends in child mortality do not appear to have a clear breakpoint; if interpolated over time, trends would follow a very consistent trajectory before and after 2007. Have alternative modeling approaches been tested here, and if so, why was the segmented regression model preferred over other methods whereby full time series could be generated and analyzed?

(8) Page 8, lines 143-144: How come a fixed effect is used for provinces rather than a random effect? It seems like the latter would provide a better absorption of unmeasured confounders.

Results (based on current Tables and Figures)

(1) Table 2: The regression coefficients do not align with what is emphasized in the results, particularly given the strength of the wealth index and time effects (which generally exceed the effects of indicators on the rural physician program). The differences in coefficients for the "Effect of rural family physician program" and the AIE further questions why both indicators are included. At minimum, perhaps the effect indicator and then an interaction term should be considered. What is the sex ratio meant to capture? I'm not sure if its inclusion has a conceptual basis.
(2) Figure 1: The trends shown here, particularly if a full time series was produced (and these trends would strengthen the argument for modeling these), would not necessarily support the claim that the rural family physician program, on its own, has reduced child mortality rates. For each measure, mortality rates were already falling, particularly for infant and under-5

Discussion

(1) Page 10, Lines 193-196: At minimum, phrasing "we showed that implementation of the second health system reform in 2005, known as the family physician program and social protection scheme for rural inhabitants, had reduced NMR and IMR significantly" implies much stronger causality than the study can actual show. Measures of the program, which were limited to the existence of the family physician program in a given year, were related to significant declines (based on the current model), but suggesting that the implementation of the reform reduced NMR and IMR, when no measure of program implementation of either program was assessed, is problematic. Further, no assessment of the social protection scheme was actually included in this study, so its effects on child mortality should not be implied.

(2) Page 11, Lines 207-208: "...our study did not support the hypothesis that increasing behvarz density improves child mortality rates." Be cautious with phrasing here - based on what is currently shown and described in this study, the phrasing should be that "our study did not find a significant association between changes in behvarz density and child mortality rates." Further, based on the current results, it is not possible to understand if behavrz density has increased over time and across provinces.

(3) Page 12, Lines 235-238: Provincial level mortality rates, since the study is conducted at the provincial level, need to be shown/reported on in one way or another in this manuscript (see commentary under Tables and Figures).

(4) Page 12, Lines 238-249: "Moreover, it is questionable not to show significant reductions in health indicators after implementing [a] family physician program..." This section seems problematic in its interpretation: not finding significant reductions in health indicators, when they are defined child mortality measures, is not actually that questionable given that child mortality rates may not be the best indicator of program impact. Further, areas with higher child mortality rates might be specifically targeted for family physician programs, so the issue of endogeneity here is not small. Please revise.

(5) Page 13, Lines 248-251: "In other words...play an important roles in this regard." This section implies overly strong causality. Please temper in accordance with the results at hand.

(6) Page 13, Lines 267-268: "Our work also showed that lower children mortality rates are seen in high-income families." Unless I missed this in the results section, I don't believe there was any reporting of this kind of finding. Further, it was my understanding that analyses occurred at the provincial level due to data inconsistencies, not at the family level. If this is the case, how is such a result supported by the present study?
At least a few limitations appear to be missing here: [1] it's likely that other potential drivers of trends in child mortality were missing, and thus all potential factors and confounders were not accounted for; [2] child mortality rates are likely to not be the most optimal measure of family physician program impact as other dependent variables, including xyz; [3] we focused on variables that measured the existence of family physician programs rather than its implementation and intensity of uptake; [4] we could not disentangle potential effects of the insurance program v. the family physician program; etc.

"Slight noises in these data were detected..." This should be expounded upon, as this could certainly affect the results (especially when a full time series for mortality, behvarz worker density, etc. has not been generated).

This claim - that over 2,550 children under the age of 1 have been saved by the family physician program - is completely counter to the previous sentence in the limitations section (we cannot establish cause-and-effect results with this study. Further no analysis was described in the Methods or Results sections about estimating potential lives saved. Please be more cautious about asserting causality, particularly regarding the potential of lives saved, when the study does not support such statements.

Again, this kind of statement is too strong for what the current study provides in terms of results. Revise to more accurately reflect the implications of the study.

Can you add one or more descriptive figures and/or tables on child mortality measures, behvarz density, average years of schooling, etc. at the provincial level and over time. At present, it's hard, if not impossible, to understand how the independent variables are changing over time (a key component of this analysis); currently only child mortality measure estimates are provided over time in Table 1 and Figure 1.

Figure 1: there was no mention of how the confidence intervals were calculated. Please include this in the Methods section.

Specific minor comments:

Please have a thorough review of this manuscript for typos.

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