Author’s response to reviews

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

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Author’s response to reviews:

Dear Editor and Reviewers,

Thanks a lot for precise and comprehensive review of our manuscript. The comments notably improved our manuscript and enriched its content. In reply to your invaluable comments, the mentioned points and their corresponding point-by-point descriptions are listed as follows:

Reviewer #1

It was my pleasure to review this paper by ten Iran-based researchers attempting to learn if their nation’s investments in access to family medicine, rural insurance and what I take to be a form of community health workers (Behvarz) had resulted in reductions in three types of child mortality.
This type of study is important to conduct, as the global effort for universal health coverage relies on knowledge of successes and limitations of primary care access programs at the level of the nation state. That said, this paper has some problems that I hope the authors can solve.

Question 1. I’d like to see the introduction include a description of the Iranian health system, before and after the reforms, and some sense of the evolution of the system over the period of the study (1995 to 2011). The description should explain changes in the payment system and how coverage expanded or contracted. It should also explain how the family physician program operates in a practical sense and which of its features would be expected to reduce mortality. How many “thousands of health centers and health houses were launched,” and how quickly did it scale?

Response

Thank you for the comment. Couple of sentences are added to Introduction to address evolution of the health system in Iran, besides a more detailed description about impact of the reform on the system. Details about number of health centers are also added. Moreover, we added a diagram on Health System Structure to show how the health system in Iran is working.

Question 2. Since the paper describes a rural system, there should be a definition of rurality and a sense of gradients along the scale. Also, the 25 provinces in the study—were those all rural? How many non-rural provinces are there? There was no map or sense of geography provided.

Response

Thank you for the comment. Rural areas were defined as all villages and towns with populations of up to 20,000 inhabitants (corresponding descriptions are added to text), and although all provinces of Iran include both rural and urban areas, the program was launched only in rural parts of each province (so there is no categorization like rural provinces or urban provinces). We have added parts to the manuscript addressing this issue. Also a descriptive table is added to show changes of independent variables over time period of the study.

Question 3. This is a long period of time (16 years), and it includes some significant world events that doubtless had an effect on child mortality that should be considered (conflict with neighboring states, sanctions, political upheavals).

Response

Many thanks for this precise comment. In the time period of our work, 1995 to 2011, our country, Iran, fortunately was not involved in conflicts with other nations nor any wars (the most recent one, was the imposed war by our neighboring nation, Iraq which happened between 1980 and 1988). Moreover, over this time period there were not any lifting of sanctions for our nation, though it might have been aggravated in areas which does not seem to be effective on mortality rates (however, lifting of sanctions and their effects on child mortality rates is a well-established trend in our neighboring country, Iraq).
Question 4. Why were data from 2007 unreliable?

Response

Thanks for the comment. The main reason was the non-ignorable inconsistencies between the human resources data (Behvarz Density) for this year and the years before and after it. Figure A.1 was added to the appendix, reflecting this situation. Due to the fact that the Behvarzes are government employees for 30 years services, their number could not observe such a drop in a year. Thus, the data for this year is invalid and was put out of the analyses.

Question 5. What is the Behvarz program? Am I right that those are community health workers?

Response

Thanks for the comment. Yes, they are the same. National community health workers, or Behvarzes in Persian language, program had been existed in Iran since 1985 As we have mentioned in the text, Behvarzes are selected from native inhabitants of each community, and after passing 2 years of training, they staff health houses, the peripheral health facility in primary health care network of Iran, and provides inhabitants of each area with simple health care services. Pervious works reported that in 2007, 31,000 Behvarzes staffed 17,000 health houses in Iran to provide inhabitants of rural areas with simple cares, including child vaccination and treatments of simple diseases.

We have added some sentences to the “Introduction” part addressing this issue. [please see “Implementing family physician programme in rural Iran: exploring the role of an existing primary health care network” - http://fampra.oxfordjournals.org/content/30/5/551.long -for more details]

Question 6. The paper would benefit from a table of measures of the independent variables over time: wealth, schooling, measures of the family physician program, Behvarz density, sex ratio, and population size for each province.

Response

Thanks for the comment. Table A.1 was added to the appendix, which contains this information.

Question 7. I can’t speak to the statistical methods in any detail, but I do have some methodological questions:

1) Authors describe a wealth index, but not any details about it. How many values are in the scale? Was the measure of wealth at the provincial level an average, or a category, or what?

2) What measures of the family physician program were used? The authors simply say “annual incremental effect,” I assume at the provincial level. But then in the description of the model, maybe it’s simply a 0 or 1 (no or yes) in each year? Is that a granular enough measure?
How many provinces fell into which category over each year? Could provinces be in the program one year and not the next?

3) What measures of schooling were used, how many values on the scale (linear, categorical)?

4) What is the theory behind using “sex ratio” in the model, and what was the measure?

Response

Thanks for the comment.

1) The wealth index was obtained through a PCA over 14 assets, which were asked if the households owned them or not. The assets list was added in the Methods section. After calculating wealth at the household level, it was averaged at the provincial level, and its exact value, without any transformation or categorization, was used in the analyses.

2) In the segmented regression model, the expectation is that by accumulating the years after performing the intervention, the effect of this intervention on the response variable becomes more and more. In fact, the annual incremental effect could be regarded as the average annual effect, since the beginning of the intervention (2005) till the end year of the study (2011). Hence, the annual incremental effect takes value 0 for years before intervention, and values 1 through 7 for year 2005 to 2011. No, all of the provinces are in the program after 2004, and none of them are in the program before 2005.

3) Years of schooling is a discrete variables with values 0,1,…,30, registered for each person. No categorizations are applied on it.

4) Considering the fact that child mortality could be affected by the proportion of male in the population, the sex ratio was added to control any variation in the population regarding male/female proportion. It equals the number of male population divided by the number of female population.

Question 8. The first paragraph of the discussion section is quite confusing; I can’t tell what is being said.

Response

Thanks for the comment. We re-wrote this paragraph and rectified possible mistakes.

Question 9. Another feature of the discussion I found confusing is the comparison to findings of other studies (lines 228 through 241). These comparisons assumed too much knowledge of Iran on the part of the reader to make sense of the differences in findings.

Response
Thanks for the comment. We have rephrased couple of sentences addressing this issue. Besides, a more detail description of the Iranian health system is added to the “Introduction” part.

Question 10. An important discussion point would be the powerful effect of the wealth index, and what the authors make of that. Of course this is a worldwide phenomenon: wealthier people are healthier, but tell us more about income distribution in Iran and how this would play out in the findings. Further, the authors should explain why they think they found such a large range of coefficients for the effects of the rural family physician program on NMR.

Response

Thanks for the comment. We have added sentences to the paragraph disusing this point in the discussion.

Question 11. The measure on page 14, “has saved lives of 2550 children” was not named in results. Also, the paragraph on lines 167 to 170 should be in the methods section.

Response

Thanks for the comment. We have rectified problems of these two sentences.

Question 12. Table 1 would be better portrayed as a series of line graphs over time, overlaid. Data source should be named in the caption of the table. A Table 2 could show us the independent variables and how they distribute before showing us the regression coefficients in a new Table 3.

Response

Thanks for the comment. A new table was added in the appendix containing the independent variables distribution. The data source was added in the caption.

Question 13. The manuscript needs editing by a native speaker of the English language.

Response

Thank you for the comment. We really apologize for the errors. We performed a comprehensive language proofreading as well as asking a person native in English to help us proof-read the manuscript.

Reviewer #2

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.

Question 1. 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.

Response

Thanks for the comment. We reviewed the manuscript and corrected the sentences which sounded that there is a cause-and-effect relationship between our response variables and launch of the program. We again apologize for the problem.

Question 2. 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).

Response

Thanks for the comment. I think there might be a misinterpretation here. Family physician program and rural protestation scheme (insurance) were both launched simultaneously as a part of a single health care reform. They were interconnected, and whenever the family physician program was launched, the inhabitants were provided with the new type of the insurance coverage. So by adding AIE and effect of the program in the models we have counted effect of the whole program, though we did not have data on the willingness of inhabitants to use the program. We really question the existence of a way to compare effect of insurance part vs. family physician part of a single health reform in setup of our work.

We have corrected possibly parts which might have this misunderstanding. We apologize.

Question 3. 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.

Response

Thanks for the comment. No, this analysis was done based on an ecologic perspective. Our objective was obtaining an interpretable model. Due to this reason, we tried to capture the time effect inherited in the data, through adding the time effect and the annual incremental effect in the model. Moreover, only one year of data (2007) was missing and no more. The observed disjoint in the numbers is natural, especially for the case of neonatal death. Actually, the number of under 1 month death occurrences could be seen as the output of a combination of components, which cause the fluctuation and disjoint from one year to another.

Question 4. 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.

Response

Many thanks for the comment.

We had not claimed that child nor maternal mortalities are the “best” measure in this regard. However, we believe on a less extreme insight: child and maternal mortality show health system performance to some extent, which enables us to study possible effects of health reform programs on health system using these measures, instead of uncommon (as you said “less widely available”) and error-prone measure like “child who have been visited by physicians in past X month, or care-seeking behavior”. Moreover, we agree with the reviewer that these measure might show short-term effects of health reform programs, but in term of long lasting health outcome, you cannot find a stand-alone measure which represent all aspect of a health program. Furthermore, the data which the reviewer had suggested to be used instead aren’t not only in our
hand, but also we really doubt if there would be a source with those data in a developing country like ours. Keeping in mind the background of our country, we are really sure that using child and maternal mortality is among the most appropriate measure which can be used in such studies. [Please check “Measuring the health-related Sustainable Development Goals in 188 countries: a baseline analysis from the Global Burden of Disease Study 2015” (http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(16)31467-2/fulltext), to see the strong correlation of child and maternal mortality measures with the position of the countries health system in the path to achieving SDG (as a index of their health system performance).]

Specific major comments by section:

Background

Question 5. (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.

Response

Many thanks for the comment. we have rephrased the text here.

However, as mentioned earlier we had not claimed that child nor maternal mortalities are the “best” measure in this regard. However, we believe on a less extreme insight: child and maternal mortality show health system performance to some extent, which enables us to study possible effects of health reform programs on health system using these measures, instead of uncommon and error-prone measure like “child who have been visited by physicians in past X month”.

Please check “Measuring the health-related Sustainable Development Goals in 188 countries: a baseline analysis from the Global Burden of Disease Study 2015” (http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(16)31467-2/fulltext), to see the strong correlation of child and maternal mortality measures with the position of the countries health system in the path to achieving SDG (as a index of their health system performance).

Methods

Question 6. (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.

Response

Thanks for the comment. Yes, the behvarzes are community health workers who are working only in rural areas, in the health system in Iran (we don’t have Behvarzes in the urban areas). By rural, we mean those areas within each province who are seen in contrast with urban and metropolitan areas.

Question 7. (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?

Response

Thanks for the comment. It might be a misinterpretation. We actually estimated wealth index, using 14 household assets and we used them in the PCA. We never used self-reported income or wealth in the model.

Question 8. (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 center?

Response

Thanks for the comment. No, the main reason was the non-ignorable inconsistencies between the human resources data (Behvarz Density) for this year and the years before and after it. Figure A.1 was added to the appendix, reflecting this situation. Due to the fact that the Behvarzes are government employees for 30 years services, their number could not observe such a drop in a year. Thus, the data for this year is invalid and was put out of the analyses.

Question 9. (4) Page 7, lines 118-119: What kind of inconsistencies occurred at the district level? Missingness, miscategorization 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.
Response
Thanks for the comment. No, the main problem was the fluctuations in the mortality level for districts through the 16 year time period. The reason behind it was that due to small number of births that may occur in a small town in each year, the child mortalities as dependent variables on the number of births also change with this fluctuations and it makes noise in the trends. Aggregating the data at the provincial level solved this problem to a large extent, because the scarcity issue was resolved.

Question 10. (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 the program implemented at the same time (2005) for every province?

Response
Thanks for the comment. Based on a segmented regression model, the annual incremental effect takes value 0 for years before intervention and values 1 through 7 for years 2005 to 2011. The effect of family physician project takes value 0 for years before 2005 and value 1 for years after 2004. FP captures the effect of the intervention, while AIE contains the aggregated effect of performing the program, through passing the years. Yes, there is some correlation between them, but due to their functionality in capturing the intervention and aggregated affects, we accept putting them at the same time in the model.

Question 11. (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.

Response
Thank you for the comment. We agree with your point that socioeconomic changes affect the child mortality. By power and efficiency of the model, we mean the strength of the model in recognizing this association.

Question 12. (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?

Response

Thanks for the comment. This model was used due to the intervention implemented in 2005, and based on the hypotheses that through passing the years, the program effect will change, so the behavior of the regression line would also change. This was the reason for using this model. Other benefit of this model was its interpretability which makes it a pleasant choice for offering to policy makers. A few other alternatives were also checked, such as propensity score matching model, but none of them outperformed the present model.

Question 13. (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.

Response

Thanks for the comment. The fixed effects were added to the model to capture any effects specific to any provinces, which may not be included in the modelling procedure.

Results (based on current Tables and Figures)

Question 14. (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.

Response

Thanks for the comment. Routinely, there are 3 three standard components in a segmented regression model. The first one shows the time variable with values 1, 2, … . The second one takes values 0 and 1 for years before and after the segmentation, respectively. The third takes value 0 for years before the segmentation, and values 1, 2, … for the years after the segmentation. Here, we called the second one FP (Family Physician effect), because there was
no family physician program in the before years. In standard settings of segmented regression, FP is called “time of intervention”. In a segmented regression, the third element is the focus of interest, i.e. the annual incremental effect.

Question 15. (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.

Response

Thanks for the comment. In the standard regression model, what is considered as the effect of intervention is not the trend that goes down or up. It is about the degree of slope which each section of the trend does have. Thus, in Figure 1, the break point of the trend has happened exactly in 2005 and the slope remains different than before 2005 which will be considered as the effect of intervention at that point.

Discussion

Question 16. (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.

Response

Thank you for the comment. We have rephrased the text here. We would like to mention again here, although we did not have data on uptake of the insurance of performance of the program in hand, since the program was launched in a single package, inhabitants used both part of the reform program, family physician and health insurance coverage, at the same time. So we have entered “existence” of the social protection scheme (as part of the whole program) in our models.

Question 17. (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 Behvarz density has increased over time and across provinces.

Response

Thank you for the comment. we have rephrased the text here. Also we have added a table to show trend of other study variables (including behvarz density) to the manuscript.

Question 18. (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).

Response

Thanks for the comment. These were added in the appendix table A.1.

Question 19. (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.

Response

Thank you for the comment. we have rephrased the text here.

Question 20. (5) Page 13, Lines 248-251: "In other words...play an important role in this regard." This section implies overly strong causality. Please temper in accordance with the results at hand.

Response

Thank you for the comment. we have rephrased the text here.

Question 21. (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?

Response
Thank you for the comment. Yes, all of our analysis were performed at the provincial level. We really apologize for the mistakes here. We have rectified them.

Question 22. (7) Page 14, Lines 273-281: 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.

Response

Thank you for the comment. We have added some of the points you have mentioned. However, as we mentioned earlier, we do not claim that child mortality measures are the “best” possible indicators to study performance of any health system. But we believe (as it is cited in literature) child mortality (especially neonate or infant mortality rates) to some extent represent function of health cares in societies.

Please check “Measuring the health-related Sustainable Development Goals in 188 countries: a baseline analysis from the Global Burden of Disease Study 2015” (http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(16)31467-2/fulltext): both the U5MR and NMR rates are highly associated with the new SDG index (which shows ranking of countries in the path to achieve sustainable development in their health care systems). Furthermore, as we said earlier-and added corresponding description to the text, rural family physician program and social protection scheme was launched as a health service package simultaneously in all rural areas of Iran (we the definition we have mentioned earlier), so we are counting effect of the program (which includes the social insurance) both in AIE and effect of the program.

Question 23. (8) Page 14, Line 277-278: "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).

Response

Thanks for the comment. Here, by noise we meant fluctuations in the data over the time periods. These noises were not so serious that could affect the results, majorly. Moreover, these issues were found and resolved through manipulating the data. This concern was resolved in the text.

Question 24. (9) Page 14, Lines 282-284: 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 saves, when the study does not support such statements.

Response

Thank you for the comment. we have omitted this sentence, which was incorrectly mentioned in the manuscript.

Question 25. (10) Page 14, Lines 289-292: 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.

Response

Thank you for the comment. we have rectified problems with this sentences, and revised it. Again we apologize for the mistakes.

Tables and Figures

Question 26. (1) 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.

Response

Thanks for the comment. These were added in the appendix table A.1.

Question 27. (2) Figure 1: there was no mention of how the confidence intervals were calculated. Please include this in the Methods section.

Response

Thanks for the comment. The confidence intervals were calculated through routine parametric procedures based on the normal distribution.

Specific minor comments

Question 28. (1) Please have a thorough review of this manuscript for typos.

Response
Thank you for the comment. We really apologize for the errors. We performed a comprehensive language proofreading as well as asking a person native in English to help us proof-read the manuscript.

Finally, it is worth mentioning that few minor changes was applied as well in the format of some sentences to not only conform to the instructions of the journal but also to add the invaluable comments of the reviewers.

Once again, thank you so much for considering our work at your journal and reviewing our manuscript. Should there is any further changes to be applied, please let us know about it.

Yours sincerely,

Corresponding author