The authors aim to estimate the effect of health workforce availability on under-five mortality (U5M) across counties in China. The article is overall well-written and easy to follow, although some specific areas could benefit from some tempering of language used or additional details (see below). In general, this manuscript could benefit from more attention in two substantive areas:

1. The authors motivate the paper with citing achievements in China's U5M reductions, alluding to health reforms that have been enacted to realize these successes. This is indeed a compelling case. However, there remains very little information on what kinds of health reforms were undertaken that would have influenced health workforce numbers. This additional background information in the introduction would help to understand the greater context for where these data come from, how these patterns in the data were generated through health systems strengthening efforts, and for interpreting the results for greater generalizability to other countries/contests where workforce policies are similarly being considered. In reading both the introduction and discussion, the relation of the paper's analyses to the context of reform was lacking, and thus, felt somewhat incomplete in terms of being able to understand what the real policy implications are of the paper's findings.

2. While the general approach to model estimate sounds reasonable, some additional details and clarifications would help to additionally gauge how the sample and final full model was derived.
   a. How are the health workforce data reported? What do these numbers reflect (e.g., any trained professional? Only employed professionals?). What are the limitations of these data (this should be discussed) and how do they affect your results? How reliable, in general, are these data?
   b. Why was health workforce density used as a continuous linear variable as opposed to also being log-transformed? What about a sensitivity analysis?
   c. You have excluded counties with missing or not enough data. How does this potential selection bias affect your estimates? What can you say about the excluded counties?
d. You imputed some data for GDP and female literacy. How much data was imputed? Are the results sensitive to these missing data?

e. Are there other county characteristics that are available, but that you chose not to include in your model? I suspect many readers are not as familiar with Chinese administrative data. It is unclear whether the variables you have used are the only ones available or rather if they were the only ones you used among the set available.

f. For our sensitivity analysis using a dynamic specification, can you provide more information on how you did this? Why did you choose the lags you did? How many lags did you choose? Why those time periods? Do they accord with other studies?

g. To what extent are there other time-varying variables that could affect both U5M and HRH availability that you have not controlled for? You only mention capability in your limitations, yet, you spend a lot of time in the introduction mentioning all the various drivers of U5M (e.g., child characteristics, environmental factors, household income/wealth, ecological setting). Are county and year fixed effects able to account for all of these? Which omitted variables might be the most important sources for potential bias (e.g., disease environment/burden, sanitation and hygiene infrastructure)?

Other comments

* Page 5, lines 45-60: It seems that past studies have found opposite effects of HRH on U5M in China, such as that cited by Anand et al. and Feng et al. Why are there opposing effects estimated? What does this say about how the relationship between HRH and U5M should be modeled? While you say on Page 6 that the magnitude of different associations vary by context, what about the direction of the association?

* Please be careful when asserting that you have estimated the "impact" of HRH on U5M. Since HRH is not randomly assigned, it is a stretch to say that you have isolated an impact estimate. While you have gone through and conscientiously built your model to control for various sources of confounding, you rightly say in your limitations that there are still possible sources of biases that you cannot account for.

* Page 10, line 57: I think you mean to say "contemporaneous."

* Page 11, line 6: "This study confirms..." seems like a rather strong statement, especially since you do not have a source of exogenous variation that could arguably be better for isolating the causal effect between HRH and U5M.
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Please indicate how interesting you found the manuscript:

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Quality of written English
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