Author’s response to reviews


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Author’s response to reviews:

Response to Reviewer 1 Comments

Reviewer Comment:


This study uses country-level data from multiple databases to examine whether indicators of women's health (life expectancy and mortality rate) improve after a country ratifies the United Nations Convention on the Elimination of All Forms of Discrimination Against Women (CEDAW). Using interrupted time series methods that exploit variation in timing of ratification across countries, the authors find that ratification leads to improvements in women's health in low-income democratic countries and in high income countries, regardless of the level of 'political democracy.' The article is well-written, the analysis is rigorous, and the research question - do human rights declarations have measurable impacts on health and wellbeing? - is important. The manuscript could be strengthened by adding a conceptual framework (how might CEDAW ratification promote health?) and by adding a discussion of major threats to validity for the ITS method and how they are addressed in the analysis.
Authors’ Response:

We very much appreciate the reviewer’s efforts, and the very helpful comments that they have provided.

Reviewer comment:

Major comments:

1. In both the introduction and discussion, the article warrants more detail on proposed mechanisms by which CEDAW might impact health. Do the authors expect instrumental effects (e.g., gender parity in income and education), symbolic effects, or both?

Authors’ Response:

We very much agree with the reviewer. Based on public health theory, our expectation is that CEDAW may influence health through both instrumental and symbolic effects. Moreover, theory suggests that these effects may be related. In preparing our revisions, we also located a theoretical paper by Nancy Moss (Social Science & Medicine, 2002), which is more specific to the topic at hand than are general public health papers. This paper provides additional (and consistent) theoretical support for these notions. We have elaborated upon the proposed mechanisms in our Introduction and Discussion. Pages 4 and 5 now read as follows.

“In theoretical terms, because CEDAW was intended to improve the status of women, it might therefore be expected to improve women’s health. The general public health literature posits that such societal conditions are, effectively, the deepest of root causes of health. The idea is that they determine a broad range of economic and social circumstances, which then influence and trigger a variety of behavioral and biological mechanisms, which are ultimately expressed as health outcomes[2]. For example, income assistance policies are a ‘societal condition’ that are a significant determinant of the material circumstances of the poorest in society. In turn, material circumstances influence access to nutritional foods, experiences of stress, and so on, and these manifest in health outcomes as varied as hypertension and depression.

Further conceptual work by Moss (2002) has specifically explicated how and why a societal condition such as CEDAW should be expected to improve the health of women[8]. As she suggests, by addressing discrimination against women, gender equity initiatives improve the material conditions of women and, through improvement of material conditions, as well as
independent of these improvements, they increase the status of women in society. For example, CEDAW can lay the foundation for policies which access to education for girls and women. It can promote more equitable intrahousehold allocation of resources between husbands and wives and between girl- and boy- children. It can also change norms in society regarding the status of women. Their increased material well-being raises their status, but, independent of these changes, the sheer fact of a state’s commitment to addressing gender discrimination also sends a broad signal regarding the importance of fair treatment for women. In turn, these material and social gains can improve women’s access to health resources, such as nutrition and income. It can reduce the psychological and physiological stress that are triggered by discrimination and lower social status, and result in improved health outcomes for women[9, 10].”

Reviewer Comment:

a) If instrumental effects, have any studies examined whether CEDAW improved material circumstances for women? And/or are relevant variables available to test as mediators?

Authors’ Response:

We found this to be a very interesting question. We scanned the literature and were not able to locate any papers that have formally tested the influence of CEDAW on material conditions. As the political scientists on our author-team had suggested to us at the outset of our study, unfortunately, most of the literature on CEDAW (and other human rights treaties) has focused on the process of ratification, rather than on measuring subsequent effects on outcomes. Moreover, due to methodological constraints, it is difficult to test mediators that can be theoretically located (such as income). For example, in a country-level analysis such as ours, indicators of income are not disaggregated by gender (e.g., we know what national GDP is, but not gender-specific GDP).

Reviewer Comment:

b) Based on expected mechanisms, did the authors have hypotheses about post-ratification trends? e.g., did they expect to see changes in health emerge immediately, or after some lag? Do the observed trends support one mechanism over another?

Authors’ Response:

Our hypothesis was that post-ratification trends would be more favorable than pre-ratification trends. We have added text on page 6, which now reads as follows.
“We hypothesize that, across countries, CEDAW will be associated with improvements in women’s health, as evidenced by a steeper rise in female life expectancy and a steeper decline in female mortality rates after, compared to before, its ratification.”

We approached our analysis as an initial attempt to determine whether CEDAW demonstrated any significant association with health trends. However, we did not have a clear sense of the degree of lag that might be observed. This is partly because lag effects are likely dictated by the magnitude of the impact of CEDAW on the lives of women. In a cross-national analysis using a binary indicator (ratification versus not), it is difficult to capture such nuances. It is also difficult to empirically assess lag with the limited data available at the country level. Page 11 now reads as follows.

“…. At this early stage of the literature, and with little theoretical or empirical guidance, we remain agnostic regarding the extent of lag impact. For this reason, and reasons of analytic constraints, we did not test lag effects…."

Reviewer Comment:

c) How and why were the effect modifiers (democratization and low/middle/high income) chosen? Are there differences across countries based on income/democratization that could explain why effects are observed in some and not others?

Authors’ Response:

The variables we used to stratify the analyses were chosen, given limited degrees of freedom, as major ways to characterize the variation in societal contexts in which CEDAW unfolds, and that may also influence women’s health. We conceptualized these factors as both potential effect modifiers and confounders, and have now clarified this in the manuscript. We have added text to our paper to better convey the rationale for, and roles of the variables we have included. Page 7 of the methods section now reads as follows.

“…We also sought to include measures of other country-level characteristics, which could either confound or modify the effects of CEDAW. Because of the limited sample size, we were only able to include key, summarizing characteristics, rather than a more exhaustive set of specific indicators. The characteristics we included were country-level income and level of political
development (whether a country is a democracy or not). The notion was that higher income and more democratized countries, with better strength of institutions and infrastructure, may have greater availability and accessibility of education, health care, and other important health resources. On the one hand, these are factors that have a positive impact on women’s health, independent of what is happening to discrimination against women, and thus the observed effect of CEDAW may be in part an effect of these other institutions and infrastructure (a confounding or selection bias effect). On the other hand, it could be that an environment in which there is greater strength of institutions and greater democracy better facilitates a state effort to reduce discrimination against women (an modification of the effect of CEDAW)…”

Reviewer Comment:

d) Could / should men be used as a sensitivity check? Or would the authors expect spillover effects for men as well?

Authors’ Response:

Thank you for this excellent suggestion. We would expect spillover effects for men, both through general improvements in the societal environment, and through increased instrumental spillover (e.g., greater household income). There is some relevant previous work on this (e.g., Kawachi, I., Kennedy, B.P., Gupta, V. and Prothrow-Stith, D., 1999. Women's status and the health of women and men: a view from the States. Social Science & Medicine, 48(1), pp.21-32.). Unfortunately, we are unable to include the results for men in this paper, because a student is examining them for another paper. However, for the sake of interest, we have included a supplementary file of tables and figures on men’s mortality and men’s life expectancy, attached at the end of this submission [N.B. THESE FILES ARE INTERNAL AND FOR REVIEWERS ONLY.]. These data indicate secular trends of declining male mortality rates and rising male life expectancy, with a significant number of regions and country-income groups demonstrating significant changes after ratification. Of course, these analyses don’t provide a direct indication of the association between CEDAW ratification and men’s health in the manner provided by interrupted time series analyses.

Reviewer Comment:

2. The authors should discuss major threats to validity and how they are addressed by the model and/or through sensitivity analyses.
Author Response:

Thank you. This comment is related to the reviewer’s earlier comment, regarding effect modifiers. We believe that the major threats to validity are differences in societal conditions, which confound the influence of CEDAW on women’s life expectancy and mortality rates. In other words, the threats come from erroneous attribution of improvements in women’s health outcomes to CEDAW, that should actually be attributed to other societal conditions that also improve women’s health, such as the general economic well-being of the society, the strength of its institutions and infrastructure, and so on.

Importantly, these threats can emerge in two principal ways. They can occur because the group of countries across which inferences being drawn are fundamentally very different in terms of such potentially confounding societal conditions. In our analyses, we stratified by what we consider to be two major indicators of such general societal conditions: income level and a political system that could be characterized, by an established indicator, as a democracy. There may be other factors that one could consider, such as poverty rates (which, for example, would further distinguish countries within each income strata), presence of a medical system, and women’s educational attainment, or but doing so creates very small sample sizes within each stratum. We thus chose what we felt were the major societal conditions which differentiate countries around the world, and could be readily measured, and still provided us with a reasonable sample size within each stratum.

Threats to validity can also emerge because societal conditions may change at the same time at which CEDAW is changing. For instance, we know that over the period during which CEDAW was being ratified, there was a general global increase in national income, though most countries did not move out of their income categories. There was also a decline in poverty rates, and a rise in level of income inequality. These ‘time-varying covariates’ could not be completely controlled for (beyond categorization of countries into income levels) because we did not have ‘control’ countries, which had undergone similar secular societal trends, but hadn’t undergone CEDAW ratification. The assumption of interrupted time series analysis is that such secular trends are accounted for by mathematically projecting out the pre-exogenous-shock trend in health outcome.

Another threat to validity is the extent to which CEDAW ratification actually manifests as change to societal conditions, which improve women’s health. Our study is predicated on the notion that CEDAW ratification is, to put it bluntly, meaningful for women’s lives. Because we do not have indicators of concrete change attributable to CEDAW, we don’t know what societal
conditions change as a result of CEDAW. We have now incorporated a discussion of this in our manuscript. This is available on page 7, as stated above. It is also now incorporated in the discussion. Page 18 and 19 read as follows.

“…The second possibility is that the effects of CEDAW are too weak, independent of other societal conditions, to register an effect on population-levels of women’s health. This is suggested, for example, by the fact that CEDAW appears to be associated with improvements with women’s health in the high-income strata, where other societal conditions provide essential preconditions. It is also suggested by the finding that CEDAW was not effective in low-income countries that are not democratized, but was effective in those which are, suggesting democratization may also be a highly influential precondition. On the other hand, it is difficult to make sense of the lack of effect across the middle-income stratum.

The third possibility is that confounders for which we were unable to account are complicating our ability to assess the ‘true’ effect of CEDAW. There are several sources of confounding that are of concern. Over the course of time that CEDAW was ratified, many other global changes took place. Perhaps most notably, globalization of markets and trade ramped up considerably. Specific secular changes had also occurred, which may or may not be account for by globalization[20]: decline in poverty rates, rise in income inequality, improvements in medical care. Because these factors may influence women’s health, and are possibly associated with CEDAW ratification, our study might be biased by not including them. As aforementioned, we are not entirely sure whether these should be considered confounders or effect modifiers. Moreover, for mortality rates, we were unable to locate age-adjusted rates, and therefore changes in the age distribution over time were also unaccounted for, and may have biased our results.

The time-varying nature of national income and, potentially, of democratization, also may have posted a problem. The implication is that countries may have moved in and out of strata over the period of analysis, which may mean that, by using the year of ratification as the year by which to categorize countries by income and democratization, some misclassification may have occurred. We believe, however, this was minimal.

Indeed, the main limitation of our study is our limited means for addressing threats to validity, namely those introduced by sources of unmeasured confounding. This was principally attributable to the fact that we were unable to incorporate a ‘true’ control group, which would tell us what happened to women’s life expectancy and mortality trends in similar countries that
did not undergo CEDAW ratification[6]. However, as discussed earlier, this was simply not possible, given that state commitment to CEDAW is an international norm – it has been broadly adopted - and thus there is a true lack of available control countries[19]. This paper thus raises interesting methodological questions regarding how to test the effects of norms…”

Reviewer Comment:

The fact that different countries ratified CEDAW in different years is an advantage. Although there is no unexposed comparison group, pre-ratification trends in late-adopting countries do serve as a sort of comparison group for early-adopters. (see, e.g., Goodman-Bacon, "DIFFERENCE-IN-DIFFERENCES WITH VARIATION IN TREATMENT TIMING")

Authors’ Response:

Thank you for this suggestion. This is a very interesting paper and approach. We consulted an economist colleague of ours, who specializes in econometrics, about the possibility of using this technique with our data. Given the time frame in which we were to submit our revisions, pursuing this technique does not seem feasible. This would require much more investigation of the suitability of countries to serve as controls for each other, and so on. Moreover, our initial sense is that it does not seem suitable for our study. The biggest issue is that ‘early adopter’ and ‘late adopter’ countries differ in many ways which require us to stratify our analyses (as we have done in the existing analyses). By the time those strata are established, sample sizes become very small.

Reviewer Comment:

b) The authors should address the potential for confounding by other, concurrent events (at the national, regional, or worldwide level). What are major potential confounders that vary over time and are related to women's health? The authors should think through this and conduct sensitivity analyses controlling for possible confounders. Different timing of ratification helps with this potential source of bias, but doesn't eliminate it.

Authors’ Response:

We very much agree with the reviewer. We have now included a more extensive discussion of potential confounding in our paper. As mentioned above, we have now included a discussion of this on pages 7, 18 and 19.
Reviewer Comment:

c) Related to (b), I am not sure I understand the claim on pg. 9 (l. 148-149) that longer pre-ratification trends would be more likely to be "contaminated by other societal changes." This implies that the pre- and post-ratification periods used aren't "contaminated," which they likely are (an unavoidable limitation of most natural experiments).

Authors’ Response:

Thank you. Our point was not intended to be binary, as in contaminated versus not contaminated. Rather, we were pointing to the increased likelihood of contamination by including additional years of data. We have now revised our wording. Page 11 now reads as follows.

“…We chose 5 years as a pre-ratification window because a longer pre-ratification period might increase the likelihood of contamination by other societal changes…..”

Reviewer Comment:

Minor comments:

3. Which, if any, variables are time-varying? Did countries move between categories of income and democratization over time?

Authors’ Response:

Income is time varying. Democratization is time varying for some countries. We used the year of ratification to measure both income level and democratization status. Some countries (but not many) may have shifted categories, but not many. The year of ratification appears to be the most reasonable time point to categorize countries, given our analytic constraints. However, we have now added text in the threats to validity section (as stated above) regarding the potential effect of countries shifting categories.

Reviewer Comment:

4. Is mortality age-adjusted?
Authors’ Response:

Mortality rates are not age-adjusted. Unfortunately, age-adjusted mortality rates are not available across all countries. We have now noted changes in population age distribution as a potential additional threat to validity in the discussion section.

Reviewer Comment:

5. I would like more details about the distribution of ratification year (either number of countries ratifying each year or mean (standard deviation) of ratification year) and whether timing of ratification differs significantly across the groups for stratified analyses.

Authors’ Response:

We have now added two supplementary tables, which provide these analyses, and the results section now includes a paragraph on these tables. Page 12 now reads as follows.

“...Supplementary Table 1 provides more detailed analysis of the distribution of ratification across countries in our sample, and Supplementary Table 2 provides the countries in each stratum. Across income strata, there was no significant difference in mean ratification year, which ranged from 1989 in high-income countries to 1990 in low-income countries. The standard deviation in each income stratum was roughly eight years. Across strata of democratization, there was a small but significant difference in mean year of ratification (1987 among democratic countries and 1990 among non-democratic countries). Across joint income*democratization strata, there were no significant differences in year of ratification. The earliest mean ratification year (1986, SD: 4.7 years) belonged to democratic, high-income countries, while the latest (1990) belonged to democratic, high-income (1990, SD: 4.7 years) and non-democratic low-income (1990, SD: 8.9 years) and non-democratic middle-income (1990, SD: 7.3 years) countries…”

Reviewer Comment:

6. A bit more detail on joinpoint analysis would be helpful. How is time modeled (a series of linear trends? Quadratic / cubic / etc. transformations?)? Does the analyst pre-specify the number (or a maximum number) of inflection points?
Authors’ Response:

We have now added additional text to describe joinpoint analysis. Pages 9 and 10 now read as follows.

“Next, we characterized the raw trends in life expectancy and mortality rates. We did so first by graphing trends in female life expectancy and mortality rates. Next, we performed paired t-tests to measure differences in life expectancy and mortality rates at the year of CEDAW ratification and 5-years and 10-years after CEDAW ratification. We also conducted joinpoint regression analysis, which assesses ‘inflection points’; years at which trends in these health outcomes had significantly changed. More specifically, time is modeled as a series of linear trends between meaningful inflection points. The number of inflection points is dictated by trends in the data. Inflection points are fit where the model determines there is a significant change in the linear trend over time since the previous inflection point.”

Reviewer Comment:

7. p. 11, l. 184 - 185: the sentence lists two different values for "low-income countries;" one must be mislabeled. Figure 2: graphs for middle-income countries are presented twice, high-income countries aren't shown.

Authors’ Response:

Thank you. We have now corrected these errors.

Response to Reviewer 2 Comments

Reviewer Comment:

Marleen Temmerman (Reviewer 2): This paper is well written, reads well and addresses a very important topic in population health. The authors tried to assess the impact on women's health (life expectancy and mortality) using valid methods that study causality, controlling for poverty and for democracy indexes, and I would like to congratulate them for taking on this challenge and looking for ways to control for the fact that 187 out of 193 countries have ratified CEDAW.

I have however problems with the methodologies used and with the conclusions:
Authors’ Response:

We would like to express our sincere thanks to the reviewer for their time and efforts. Our study has very much benefited from their comments.

Reviewer Comment:

The authors discuss ratification of CEDAW which does not imply implementation: some countries translate UN resolutions and guidelines to national policies and practices, others ratify and do not implement at all. Did the authors try to look at implementation rate?

Author Response:

The reviewer raises an important point. We could not find data to indicate anything like an “implementation rate.” This is a limitation of an analysis of ratification, rather than implementation. On the one hand, our analyses tell us, on average, the association between CEDAW ratification and women’s health outcomes. On the other hand, it doesn’t give us information about the ensuing actions of states, which would provide a better indication of the specific policies and processes that are associated with women’s health. We have now added text about this in our discussion section. Page 17 now reads as follows.

“…The first possibility is that CEDAW ratification does not necessarily translate into improved social and economic conditions for women. In other words, there may be a disconnect between ratification and the ‘real’ implementation of policies and programs that have the potential to foster the elimination (or even reduction) in discrimination against women. Our binary indicator of ratification did not assess the post-ratification actions (or inactions) of countries…”

Reviewer Comment:

2. Line 119-142: authors state that there are no valid control methods and therefore they used an interrupted-time-series analysis (ITSA). My questions are the following

-CEDAW has been ratified over more than 20 years; hence could the authors analyse countries that ratified early with as control group late ratifiers in the same political democracy classes?

-perhaps even better would be to compare male life expectancy and mortality as well in relation to CEDAW ratification. In case the better rates are similar for females and males, the effect of CEDAW on these indicators is probably low/non-existent
Authors’ Response:

The reviewer brings up some interesting points. Regarding the first matter, using early and late ratifying countries as comparisons, there are two problems. The first is sample size. The second is that it is unclear if early versus late ratification will actually control for the main competing explanations, which are other macro-level changes, such as changes to poverty rates, income inequality, and other economic and social variables.

Regarding the second point, it is unclear that an association between CEDAW and male life expectancy should be expected to be low/non-existent. There are indications in the literature that, in fact, increases to women’s status do have substantial spillover effects to men. For example, this can occur because men may benefit from improvements in the human and financial capital of society (because society becomes more developed and richer). This may also be replicated at the household level. Unfortunately, we are not able to include analyses for men’s health in our paper because they are already being used by a student for another paper. However, we have provided the reviewers with some descriptive findings for the sake of interest.

Reviewer Comment:

3. Conclusions: the authors conclude that the impact of CEDAW ratification is mixed; I would rather state that no conclusions can be made based on this study.

Authors’ Response:

We have now modified our conclusions. Page 19 now reads as follows.

“…Our findings yielded mixed effects for CEDAW, indicating either that CEDAW’s impact on women’s health is highly dependent on other societal conditions, such as income level, democratization, or a host of other variables, for which we did not account or, that CEDAW does not have an especially large independent impact on women’s health. Our study provides a way to assess the effects of norms, and also how to examine if the effects of norms, even ostensibly widely-held ones, vary in the ways in which they manifest in different countries. Future studies should continue to investigate the associations between international human rights treaties and population health outcomes…”
Reviewer Comment:

In conclusion: the rationale of the study is really interesting, and I hope that the paper get published after additional analyses as suggested above, or if impossible, after addressing this in the discussion that is rather short now.

Authors’ Response:

Again, we are very grateful for this most helpful exchange.