Author’s response to reviews

Title: Effects of the Convention on the Rights of the Child on child mortality and vaccination rates: a synthetic control analysis

Authors:

Gary Reinbold (grein3@uis.edu)

Version: 1 Date: 30 Nov 2018

Author’s response to reviews:

We are submitting a revised version of the article with changes marked from the initial version. We did not mark changes in the tables, because most numbers changed and the tables would be too difficult to read with the markings. We note that we could easily eliminate Tables 7 and 8 from the article and make those tables available online or upon request instead, if the editor would prefer for the article to be shorter. Following are the comments received from the editor and reviewers on the initial version and our responses to those comments.

Editor Comments:

1. As this is a secondary data analysis article, please state in the Methods and Declarations (in the section Ethics approval and consent to participate) whether the databases are publicly available or not. If they are not, please include a statement that all necessary permissions were obtained to access and use the data and who gave this permission.
Response: We have included a statement that all data are publicly available on page 24 of the marked copy.

2. The link in the “Availability of data and materials” of the Declarations does not work. Please provide a persistent web link.
Response: We have corrected the link to our dataset on page 24 of the marked copy.
Reviewer 1 comments:

1. There is no rationale given for why a dummy variable was "Islam dominant" the juxtaposition of this framing with that of civil war, polity and trade could be construed as discriminatory. This framing needs more careful explanation and justification - as it's unclear how religious differences would affect child immunization rates/child mortality.

2. Additionally, the "British legal origin" choice is also lacking justification. It suggests that there was no legal origin before the British colonized those countries. This also could be construed as discriminatory, and suggests that the authors have a clear bias toward Anglo/Christian ideals/views. What about the countries colonized by the Belgians, the French, Portuguese, and Spanish? I suggest the authors reconsider their methods and be careful of how white supremacy is infusing these methods and hypotheses.

Response to prior two comments: We included the religion and British legal origin dummy variables as controls mainly because Simmons (2009) included them in the only prior study of the effects of the CRC, although a few other researchers also included the British legal origin dummy variable in their studies of the effects of core human rights treaties. However, because none of these variables affected our results, because they were rarely significant at the 5% level, and because they were not included in most other studies of the effects of core human rights treaties, we dropped them from our analyses.

3. Throughout the authors use "adoption" of the CRC (without distinguishing signed vs. ratified). I think there should be a clarification that adopted signifies "ratified."

Response: There was a parenthetical on page 8 of the marked copy, indicating that "adopted" means ratified, accepted, acceded, or succeeded. We expanded that parenthetical to a full sentence on page 7 of the marked copy and added the suggested explanation that adoption does not include merely signing the treaty.

4. Throughout the authors infer that the CRC itself is an agent of change, whereas it's simply a piece of paper/document. The language throughout should be ratification of CRC or state/govt adoption of the CRC.

Response: We changed all references to the effects of the CRC to refer to the effects of CRC adoption instead.
5. The discussion begins with a sentence suggesting that scholars criticize the effectiveness of the treaties due to lack of enforcement. Please provide evidence/citation.

Response: We added citations to several of those studies after the first sentence of the Discussion section on page 18 of the marked copy.

6. The discussion merits more narrative about effectiveness of public health workforce & infrastructure, assistance from other countries, access to clean water, etc.

Response: We considered adding a general discussion of how the suggested factors contribute to efforts to increase vaccination rates and reduce child mortality. However, we expect that most readers are already familiar with those processes, in general. Instead, we added a paragraph on pages 20-21 of the marked copy discussing a specific example from Djibouti that illustrates the importance of many of the factors raised by the reviewer.

7. More is needed about the meaning of "momentum" that was identified in the methods.

Response: As a result of changes in the other variables included in the year fixed effect regressions, the lagged (year t – 5) value of the dependent variable was rarely significant. Therefore, we condensed our discussion of the lagged value and eliminated the mention of momentum on page 13 of the marked copy.

8. For child mortality, make it explicit that the rate is 10 per 1,000 throughout to ensure that all readers can understand the metric.

Response: We added the phrase "per 1,000 live births" to all references to specific mortality rates.

9. Table 1, consider adding some categorization or signal of treatment vs. control.

Response: We received a similar comment from the other reviewer, and we added a new Table 3 with descriptive statistics for the treatment and control groups in the synthetic control analyses.

10. Table 2. Clarify the meaning of the N. (Country years?)

Response: We changed N to "country-years" in all of the tables.
Reviewer 2 comments:

1. P. 9, the line below [Insert Table 2], "Our independent variable was a dummy variable coded 1 if the country had adopted the CRC and 0 otherwise." -- How could it be the case since the authors had declared earlier that the main objective of their study was to compare "the group that adopted in 1990 (the treatment group) with the group that adopted in 1993 or later (the control group)" (p. 8, In 2)? Stated differently, ALL sample countries should have adopted the CRC.

Response: The countries that adopted the CRC after 1990 would be coded 0 until the year in which they adopted it. We added language on page 10 of the marked copy to clarify that point.

2. P. 10: The contents of the first paragraph of the Results section belong to Methods, indeed. And, there is a similar suggestion regarding the first part of the text under the Synthetic control analyses subsection (p. 12). The authors should recognize that it is improper to include any reference citation in the Results section; as such, they could avoid the aforementioned mistakes they had made.

Response: We agreed with the reviewer's suggestion and relocated those paragraphs from the results section to the methods section on pages 8-9 and 11-12 of the marked copy. (We are unaware, however, of any general prohibition on citing sources in the results section and note that doing so is a fairly common practice in this journal.)

3. Table 1 is quite perplexing. In 1991, there were 42 countries adopting CRC, but only 39 of them existed in 1990. Hence, it means that there had been 3 new countries founded in 1991 around the world. By the same token, there were 3 newly established countries in 1992, 6 in 1993, 5 in 1994, and so on? Is it really the fact?

Response: It is true that many new countries were formed in Eastern Europe during the early 1990s after the breakup of the Soviet Union, Yugoslavia, and Czechoslovakia. The reviewer's interpretation of the table is not quite correct, however, as the differences between the first two columns indicate the number of countries that newly adopted the CRC each year AND were not in existence in 1990. Thus, there were 3 countries that adopted the CRC in 1991 that did not exist in 1990, 3 countries that adopted the CRC in 1992 that did not exist in 1990, and so on.

4. Table 2: (a). "Table 2 shows descriptive statistics for the variables in our analyses; it includes all countries with under-5 mortality rates greater than 10 in 1990, regardless of their CRC adoption years, because we included all of these countries in our year fixed effect regressions." (p. 8, last para) -- It is difficult for me to accept this rationale since the authors declared that they intended to compare "the group that adopted in 1990 (the treatment group) with the group that adopted in 1993 or later (the control group)" (p. 8, In 2). In view of that, it would be better for the authors to present comparative statistics between the two groups at baseline. (b). The second column heading is "N", but Table 2 contains infant mortality "rate" (with the data of 984, etc.),
and other kinds of rates. Obviously, "N" is not equal to "rate", and thus infant mortality rate of 984 is rather horrific, for instance. (c). Following up on my previous comment, the "N" values in Table 3 are 598, 509, 548, and 547. But, NONE of the N values could be located in Table 2. Put another way, a quite confusing use of the two Ns in Tables 2 and 3 by the authors. (d). Finally, why did the authors perform logarithmic transformation on "population" only, as many other variables (such as GDP per capita) were apparently not normally distributed, either?

Response: First (addressing point (a)), we added a new Table 3 with descriptive statistics for the treatment and control groups in our synthetic control analyses. We felt that it was important to retain Table 2 because it presents descriptive data for many variables that are included in the year-fixed effect regression analyses and instrumental variable analyses, but not in the synthetic control analyses. Second (addressing points (b) and (c)), we followed the other reviewer's suggestion to change N to "country-years" throughout the tables. Third (addressing point (d)), we revised all of our analyses to use the natural log of GDP per capita as suggested, because that transformation was used in most of the prior studies and the interpretation is just as easy with the transformed variable. We did not transform any of the other variables because prior studies have not transformed them and the interpretation is easier with the raw variables. And, of course, there is no requirement for independent or dependent variables to be normally distributed in any of our analyses.

5. Table 3: The first column is supposed to be the names of those independent variables used in regression models. Hence, how could it conceivably have an independent variable (the fourth to the last) named as "Dependent variable" and another one (the third to the last) as "Dependent variable in year t-5"? The same confusing presentations appear in Table 6, too.

Response: We appreciate the reviewer pointing out the confusion associated with including the current year and lagged dependent variables in the regression. We changed the first column heading of Tables 4 and 7 (formerly Tables 3 and 6) to describe "variables included in the regression" rather than "independent variables."

6. In Table 3 and other tables of inferential analysis results, "*" was indeed referring to "p < 0.10" (pointed out in the footnotes). I would argue that it is sort of misleading usage since commonly the Type-I error threshold is set at 0.05.

Response: We are aware that the selection of an alpha varies by discipline. Most prior studies of the effects of core human rights treaties have used an alpha of 10 percent (Clark 2010; Cole 2012, 2015; Dancy & Sikkink 2012; Englehart & Miller 2014; Hafner-Burton & Tsutsui 2005, 2007; Hill 2010; Lupu 2013; Neumayer 2005; Vreeland 2008), which seems appropriate to us in light of the small sample sizes in these studies. However, many of those studies did use alternate symbols to indicate the lowest significance level, so we changed our table notes to indicate p < .10 significance with a plus sign instead of an asterisk.