# Omitted Variable Bias and Wartime Legacies

## A Reply to Malesky and Nguyen (*Journal of Comments and Replications in Economics, 2024)*

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*Abstract*
Malesky and Nguyen (2024, *Journal of Comments and Replications in Economics*, MN) reassess a study on the effects of wartime violence on civic engagement in Vietnam, attributing discrepancies in previous findings to coding and historical errors. They argue that prewar party strength, rather than wartime exposure, drives contemporary civic engagement. However, their reanalysis omits key geographic covariates, which are essential for the validity of the original study's quasi-experimental design and were included in both Barceló (2021) and Miguel and Roland (2011). MN claim that the control variables South and Latitude are collinear and therefore drop both from their models. This is an elementary mistake: collinearity between control variables does not affect the treatment effect (*Distance*) and should not impact its estimation. Even if collinearity were an issue, the appropriate response would be to remove only one of the collinear variables, not both. This misspecification leads to omitted variable bias and a violation of the exclusion restriction of the instrumental variable model. Once this omission is corrected and either *South* or *Latitude*, or both, are properly accounted for, the originally reported results withstand all of MN's other proposed modifications, including controlling for prewar party strength, excluding zone IV provinces, and adjusting standard error clustering, among others. This study confirms that wartime exposure enhanced long-term civic engagement in Vietnam and highlights how improper model specifications can produce biased estimates that may obscure, rather than clarify, our understanding of the legacy of historical events.

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## 1 Introduction

A burgeoning literature suggests that historical institutions and salient political events impact social and political attitudes over long periods of time. A prominent subset of these studies explicitly focuses on the long-term effects of exposure to wartime political violence, providing evidence on its effects on cooperation, risk attitudes, and identities (e.g., Charnysh et al., 2023; Simpser et al., 2018). However, determining the effects of violence on contemporary attitudes is particularly difficult. Exposure to violence is not random; violence is often targeted in areas with greater fighting activity or rebel capacity, meaning that exposed individuals and areas might differ from unexposed ones not due to violence but due to pre-existing conditions. To complicate matters further, legacies of violence often generate displacement and postwar migration patterns, introducing post-treatment sorting bias as individuals move across territories, making it difficult to reliably estimate a treatment effect based on where individuals live contemporaneously versus where they were exposed to conflict (Marbach, 2023).

Building on the previous measurement and identification strategy developed by Miguel & Ronald (2011); Miguel & Roland (2023), I took advantage of the natural experiment in the exposure to conflict caused by proximity to the 17th parallel (Barceló, 2021, 2023). This arbitrary border, established at the Geneva Conference in 1954, experienced the heaviest exposure to wartime conflict, while areas further away experienced less exposure, conditional on pre-existing province characteristics such as population density or geographic controls. Leveraging this unique context and data from a 2001 public opinion survey in Vietnam conducted as part of the World Values Survey, I used the difference in latitude between prewar provinces and the 17th parallel in a two-stage least squares analysis to examine the long-term effects of wartime violence on civic engagement. The findings were conclusive: People who lived in provinces significantly impacted by the conflict during the war are more actively involved in organizations and exhibit stronger expressive values, even 26 years after the war. Furthermore, an analysis of the mechanisms indicated that these behavioral changes were driven by both individual-level wartime experiences and experiences with the postwar context.

This paper made several key contributions to the literature on the legacy of political violence. First, while earlier work had found a positive effect of wartime violence on participation in social organizations (Bauer et al., 2016; Bellows & Miguel, 2006, 2009), the "as-if" random assignment of wartime exposure during the Vietnam War provided a distinctive case to strengthen causal claims about war exposure's impact on civic engagement. Second, previous studies on prosocial attitudes mainly focused on domestic wars in places like Burundi (Voors et al., 2012), Nepal (Gilligan et al., 2014), Sierra Leone (Bellows & Miguel, 2006, 2009), and Uganda (Luca & Verpoorten, 2015). The Vietnamese case allowed for generalization to conflicts involving international forces. Third, the World Values Survey included questions on contemporary residence, birthplace, and residence of individuals during the war, allowing an evaluation of transmission pathways and addressing post-treatment sorting bias (Marbach, 2023). Finally, unlike many previous studies, I used aggregate measures of exposure to violence, avoiding recall bias (Deaton, 2001).

MN revisit the findings of this article, attributing the differences in the results to supposedly coding and historical errors. Their reanalysis challenges Barceló's (2021) findings and interpretation, contending that preexisting communist involvement, rather than exposure to wartime violence itself, accounts for these areas' greater loyalty to the regime today, as evidenced by higher involvement in social and political organizations. MN's reanalysis, which finds a null effect of exposure to wartime violence on social and political participation, directly challenges 20 years of research on the legacy of violence in Vietnam, especially considering the widespread violence that impacted the Vietnamese population (for an early review, see Bauer et al., 2016). Therefore, clarifying whether the Vietnam case aligns with the general pattern observed in numerous settings, where exposure to wartime violence fosters civic engagement, or if it represents a rare exception to that rule, is fundamental for the literature on the legacies of violence.

In this response, I address the critique posed by MN (2024) by demonstrating that their reanalysis omits critical geographic covariates, *South* and *Latitude*, which are essential for the validity of the original study's quasi-experimental design and were included in the model specifications by Barceló (2021) and Miguel and Roland 2011. MN justified this omission by arguing that the control variables *South* and *Latitude* are collinear, leading them to drop both from their models. This decision is a significant error: collinearity affects the relationship between control variables, not the treatment of interest (*Distance*), making the inclusion of both variables acceptable for estimating the treatment effect. Even if one were concerned about collinearity, the proper approach would have been to remove one collinear variable, not both. By conflating the border distance treatment with the South-North distinction, this omission introduces substantial confounding bias into the estimate and violates the exclusion restriction of the instrumental variable model.

After rectifying this omission and properly accounting for either "South" or "Latitude," I demonstrate that the originally reported results remain robust across all of MN's modifications. Specifically, I show that the results are resilient to (a) controlling for their measures of prewar party strength, whether zone IV provinces or the birthplace of second committee members, (b) excluding zone IV provinces or provinces of the birthplace of second committee members, (c) reweighting the sample to reflect the distribution of party and non-party members in Vietnam, (d) controlling for respondents who are party members, (e) excluding party members, (f) removing organizational types linked to the VFF, (g) applying various clustering structures for the standard errors, (h) recoding the control variable of "South," and (j) controlling for ethnicity.

In the remainder of this manuscript, I review basic statistics to emphasize the importance of controlling for confounding variables in regression models to accurately estimate treatment effects, explaining how omitted variable bias (OVB) distorts results and violates OLS assumptions. I then discuss why prewar residence in South or North Vietnam, represented by either a dummy or continuous variable, is a critical confounder for the validity of the quasi-experimental design. First, North and South Vietnamese residents probably differed in civic engagement before the war due to historical institutional legacies affecting contemporary social and economic outcomes. Second, it demonstrates that quasi-experimental designs based on border distances must address confounding from differences across the border. In these designs, confounding arises from two main factors: (i) the presence of pre-existing differences between groups on either side of the border and (ii) the asymmetry in the distribution of units across the border, which increases the correlation between border location and distance. Both factors are present in Vietnam. Next, I propose two methods to address omitted variable bias in this context: controlling for the relevant pretreatment confounder or restricting the sample to ensure a symmetrical distribution of respondents across the border. Once either method is implemented, the main finding remains robust against all of MN's modifications and sensitivity analyses, with no evidence that prewar party strength undermines the causal effect of wartime violence exposure on civic engagement. Overall, MN's work has reinforced, rather than undermined, the evidence supporting the causal effect of bombing in Vietnam on contemporary civic engagement. Finally, I emphasize the importance of addressing complex modeling challenges to ensure that causal pathways are properly accounted for, highlighting that improper model specifications can hinder, rather than advance, scientific knowledge.

## 2 Importance of Controlling for Confounding Variables

In their reassessment of Barceló (2021), MN (2024) make several modifications and adjustments to the earlier-reported model specifications. While many of these adjustments are welcome robustness checks, MN commit a significant error that undermines their entire re-analysis: they omit a crucial geographic covariate–prewar residence of individuals in either North or South Vietnam. This omission introduces confounding bias and a violation of the exclusion restriction, invalidating all their OLS and IV models.

In this section, I revisit the fundamentals of econometrics by explaining confounding, or omitted variable bias, and discussing how the violation of OLS assumptions can affect the exclusion restriction in IV models. While this may seem basic to those familiar with quantitative methods, it underscores the critical nature of the error made by MN. This elaboration serves to clarify that an incorrectly specified regression model is likely to produce invalid results with biases of unknown direction and magnitude.[1]

### 2.1 Definition and Impact of Confounding or Omitted Variable Bias (OVB)

When conducting regression analysis, particularly in observational studies, it is crucial to adjust for control variables that precede the treatment and are associated with both the treatment and the outcome. This adjustment helps mitigate confounding bias and ensures an accurate estimation of causal effects.

Confounding bias occurs when an extraneous variable, known as a confounder, distorts the observed association between the treatment (independent variable) and the outcome (dependent variable). This distortion can either create a false association where none exists or obscure a true association. A confounder is characterized by its independent association with both the treatment and the outcome. Therefore, confounding factors, or control variables, are variables that must be accounted for in regression models to isolate the true effect of the treatment on the outcome. By adjusting for these control variables, we aim to remove the bias they introduce, ensuring that the estimated effect of the treatment is not confounded.

To formally understand the impact of omitting a confounding variable, consider the following regression model:

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 x_i + \epsilon_i$$

where:

- $y_i$ is the outcome for unit $i$

[1] For extended reviews covering this issue such as **?**, Hill & Gelman (2007), Llaudet & Imai (2022) or Wooldridge (2019).

- $T_i$ is the treatment indicator for unit $i$
- $x_i$ is the confounding covariate for unit $i$
- $\beta_0, \beta_1, \beta_2$ are the coefficients
- $\epsilon_i$ is the error term

If the confounding covariate $x_i$ is ignored, the model becomes:

$$y_i = \beta_0^* + \beta_1^* T_i + \epsilon_i^*$$

To understand the relationship between these models, consider a third regression:

$$x_i = \gamma_0 + \gamma_1 T_i + \nu_i$$

Substituting this representation of $x_i$ into the original equation and rearranging terms gives:
1. Substitute $x_i$ in the original model:

$$y_i = \beta_0 + \beta_1 T_i + \beta_2(\gamma_0 + \gamma_1 T_i + \nu_i) + \epsilon_i$$

2. Distribute $\beta_2$ across the terms inside the parenthesis:

$$y_i = \beta_0 + \beta_1 T_i + \beta_2\gamma_0 + \beta_2\gamma_1 T_i + \beta_2\nu_i + \epsilon_i$$

3. Combine like terms:

$$y_i = \beta_0 + \beta_2\gamma_0 + (\beta_1 + \beta_2\gamma_1)T_i + \epsilon_i + \beta_2\nu_i$$

Equating the coefficients of $T_i$ in the original and substituted models yields:

$$\beta_1^* = \beta_1 + \beta_2\gamma_1$$

This formula illustrates that the bias incurred by excluding a confounding covariate ($\beta_1^* - \beta_1$) is proportional to the product of the association between the treatment and the confounder ($\gamma_1$) and the association between the outcome and the confounder ($\beta_2$). If either of these associations is zero, there is no bias.

## 2.2 Violation of OLS Assumptions by Confounders

Confounders are variables that are then associated with both the treatment and the outcome. When these confounders are not included in a regression model, they violate key assumptions of Ordinary Least Squares (OLS) regression, leading to biased and inconsistent estimates.

The OLS regression model relies on several key assumptions for the estimated coefficients to be unbiased and consistent. One of these assumptions is that the error term is not correlated with the explanatory variables (i.e., $E(\epsilon_i|X_i) = 0$). However, when a confounder is omitted from the model, this assumption is violated.

For example, consider the regression model:

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 x_i + \epsilon_i$$

If $x_i$ (a confounding variable) is omitted from the model, the error term $\epsilon_i$ will capture the effect of $x_i$. Since $x_i$ is correlated with both $T_i$ and $y_i$, the error term $\epsilon_i$ will be correlated with $T_i$. This correlation between the error term and the explanatory variable violates the OLS assumption $E(\epsilon_i|T_i) = 0$, leading to biased and inconsistent estimates of $\beta_1$.

A variable is considered a plausible confounder if it meets the following criteria:

1. **Associated with the Treatment:** The variable must be correlated with the treatment variable. For example, in a study examining the effect of campaign spending on election outcomes, socioeconomic status could be a confounder if wealthier candidates are more likely to spend more on their campaigns.

2. **Associated with the Outcome:** The variable must also be correlated with the outcome variable. In the same study, socioeconomic status could affect election outcomes independently of campaign spending.

3. **Not an Intermediary Variable:** The confounder should not be on the causal pathway between the treatment and the outcome. For example, if campaign spending influences voter turnout, which in turn affects election outcomes, controlling for voter turnout can lead to over-adjustment bias.

## *2.3 OVB And the Exclusion Restriction Assumption in 2SLS Models*

Two-Stage Least Squares (2SLS) regression models are used to address endogeneity issues by employing instrumental variables (IVs). However, omitted variable bias (OVB) in the first-stage regression can lead to a violation of the exclusion restriction assumption in 2SLS models. This assumption stipulates that the instrumental variable should affect the outcome only through the treatment variable and not via any other pathway. In other words, the instrumental variable must be uncorrelated with the error term in the second stage of the regression.

Consider the following 2SLS model:
First stage:

$$T_i = \pi_0 + \pi_1 Z_i + \pi_2 x_i + u_i$$

Second stage:

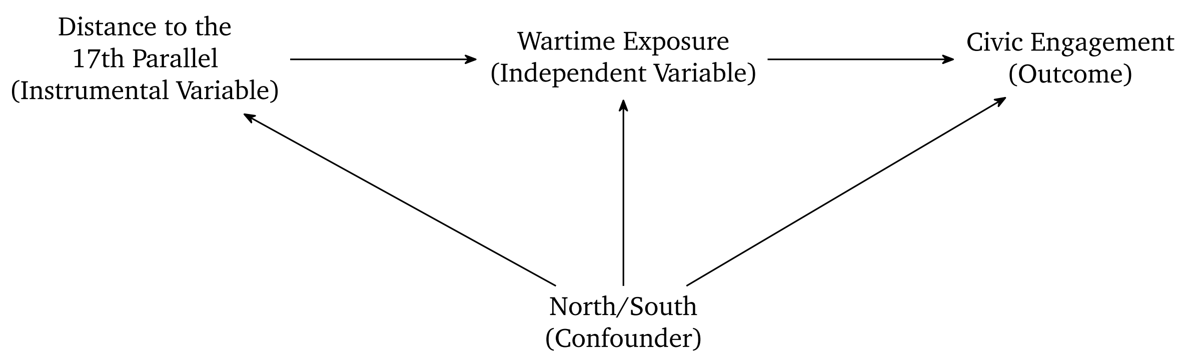
$$y_i = \beta_0 + \beta_1 \hat{T}_i + \beta_2 x_i + \epsilon_i$$

where $Z_i$ is the instrumental variable, $x_i$ is the confounding variable, and $\hat{T}_i$ is the predicted value of the treatment from the first stage.

If $x_i$ is omitted from both stages, the exclusion restriction is violated because $Z_i$ may be correlated with $x_i$, which in turn is correlated with the error term $\epsilon_i$. This correlation means that the instrument $Z_i$ affects the outcome $y_i$ not only through the treatment $T_i$ but also through the confounder $x_i$, violating the exclusion restriction.

To illustrate these concepts in a specific context, consider Figure 1, which depicts the diagram of Miguel & Ronald (2011) and Barceló (2021) research design in Vietnam. If $x_i$ represents prewar residence in North or South Vietnam, measured either as a dummy or latitude, and this variable affects both the treatment (e.g., exposure to wartime violence) and the outcome (e.g., civic engagement), omitting $x_i$ would result in an invalid IV model. The instrumental variable $Z_i$ (e.g., distance to the 17th parallel) would be correlated with the omitted confounder $x_i$, leading to biased estimates of $\beta_1$.

Figure 1: An Illustration of Barceló's Instrumental Variable Design




In the quasi-experimental design we employ, the aim is to gauge the impact of proximity to the border established at the 17th parallel by measuring the distance to it. In practice, this means that the variable of interest, distance, introduces a V-shaped pattern relative to latitude, where units at moderate latitudes register the smallest distances and those at extreme latitudes, whether high or low, show the greatest distances.

MN's difficulty in comprehending how south and latitude serve as crucial confounders in this setup led them to claim that "distance (measured in latitude) after removing its covariance with latitude and orientation" (11) is meaningless. However, I understand that some modeling approaches to identify causal processes can be challenging, and further clarity might be required. This misinterpretation reflects difficulties in understanding the fundamental principles of the research design shared by both Barcel'o (2021) and Miguel and Roland (2011), rather than indicating a flaw in the design itself.

To clarify, the instrumental variable's interpretation in the first stage is quite direct: a one-latitude unit increase away from the 17th parallel, either northward or southward, leads to a 57% reduction in the percentage of bombs dropped per capita, after controlling for the linear geographic trend (which simply accounts for proximity to the North increasing bombing likelihood) and systematic regional differences (where provinces in North Vietnam were more likely to be bombed). Essentially, this measurement captures the impact of proximity to the 17th parallel on bombing intensity, while adjusting for basic geographic trends and regional disparities.

To clarify this with another example, imagine we wanted to investigate the effect of being closer to the age of 30 on the likelihood of purchasing property. The variable, "distance to 30," would be measured as the absolute difference between a person's age and 30, creating a V-shaped pattern where those close to 30 have the smallest distances, and those much younger or older have larger distances. Now consider two factors: (i) older people tend to be wealthier, and (ii) there was a historical policy impacting purchases for those who in today's age they are over 30. To explore this question, one would collect survey data from individuals near the age of 30 as well as those further away from this age to ensure sufficient variation in the treatment. However, if the sample includes more older individuals than younger ones, this oversampling would generate a correlation between age (linear trend), the dummy variable for being over 30, and distance, as those far from 30 would likely be older. Under these conditions, it would be critical to control for linear age trends and pre-existing differences in properties for those benefited from the old policy with a dummy variable for those over 30 to ensure that the effect of distance is not conflated with the linear trend of age. One could only isolate the effect of proximity to age 30 on property purchasing behavior after controlling for these variables.

In summary, the presence of omitted variable bias in 2SLS models can lead to the violation of the exclusion restriction, resulting in biased and inconsistent estimates of the treatment effect. Therefore, it is crucial to identify all relevant confounders and address them to ensure the validity of the IV model. Using Barceló's instrumental design as an illustration, North/South, measured as either a dummy or a continuous latitude variable, is a crucial confounder for the design if it is correlated with distance, wartime exposure, and civic engagement. Excluding it introduces confounding bias. Unlike what MN argue, the first stage regression of these models produce a quantity of interest that is straightforward to interpret.

## 3 *Implications of OVB for MN's Re-analysis*

In the specific case of the legacies of wartime violence in Vietnam, there are both historical and methodological reasons why prewar residence in North or South Vietnam is a plausible confounder in explaining the association between exposure to wartime violence and civic engagement.

### 3.1 *Historical Reasons for Controlling for Prewar Residence in South and North Vietnam*

MN's critique that Barceló's study lacks a thorough understanding of Vietnamese history is contradicted by their own disregard for basic historical facts that underscore the necessity of controlling for fundamental prewar geographic and institutional factors. These factors are extensively documented in a wide range of historical sources, including scholarly articles, historians, and administrative documents, which emphasize the significant historical variations along the North-South divide in Vietnam. Such variations are critical to any rigorous analysis of causal inference, as they have profound implications for modeling choices, including the necessity of controlling for variables like the prewar place of residence, whether in South or North Vietnam. I will now elaborate on this historical evidence.

The experiences of northern and southern Vietnam have been dramatically different in historical terms, with distinct institutions shaping each region for over a millennium (Cœdès, 1966; Taylor, 2013). In the north, historically known as Đại Viêt, a strong centralized state was established, characterized by Chinese-influenced governance, Confucianism, and an agrarian-based economy (Lieberman, 2003; **?**). This region saw a series of dynasties such as the Ly, Tran, and Le, which consolidated control and developed distinct political and social institutions. In contrast, the central and southern regions were under the control of the Champa kingdom and later the Khmer Empire (Lieberman, 2003). Champa, influenced by Indian civilization, focused on maritime trade, Hinduism, and later Islam, while the Khmer Empire followed a patron-client model with weaker, more personalized power relations and no village intermediation. These historical differences had profound long-term impacts.

According to a study by Dell et al. (2018), areas historically under the strong state of Đại Việt have higher living standards today and have had better economic outcomes over the past 150 years. The strong historical state structure in Đại Việt facilitated better organization for public goods and redistribution through civil society and local government. In contrast, regions influenced by the Khmer Empire had weaker state structures and more personalized power relations, resulting in less effective collective action at the village level and long-term economic development.

While the historical borders between Đại Việt and Champa evolved significantly over the years (Lieberman, 2003), it is clear that provinces falling under North Vietnam after the 1954 separation had generally been under the influence of Đại Việt for a longer period—most since the beginning of the Ly dynasty in the 11th century. In contrast, provinces that became part of South Vietnam after 1954 were more likely to have been under Champa and later the Khmer Empire for longer periods and under Đại Việt for shorter periods. For instance, Đại Việt did not conquer Quang Tri and Hue, the northernmost provinces of South Vietnam, until the 14th century, and it was not until the 17th century that it reached Khánh Hòa Province at the 12th parallel. The process was not completed until the 19th century when Đại Việt conquered the territories that would ultimately resemble modern Vietnam (Dell et al., 2018; Lafont, 1985). This slow and gradual process of territorial expansion over the past millennium, combined with the very different institutions and forms of governance within and outside Đại Việt, and the known long-term legacy of these differential institutions on development and civic engagement, make geographic factors like the North-South divide a potential confounder for any association between wartime exposure and civic engagement.

Beyond the unequal historical trajectories between North and South Vietnam, it is also important to highlight the impact of the 1954 Geneva Accords, which established distinct institutions post-1954, before the beginning of the Vietnam War (Herring, 2001). The Accords divided Vietnam along the 17th parallel, creating the communist Democratic Republic of Vietnam in the North and the anti-communist Republic of Vietnam in the South. This division entrenched starkly different political, economic, and social systems on either side of the border (Library of Congress, 1987). The North pursued a path of socialist collectivization and central planning, implementing land reforms and promoting state control over resources. In contrast, the South adopted a market-oriented approach with support from Western allies, emphasizing private property, capitalist enterprise, and agricultural modernization. These divergent institutional developments further deepened the regional disparities that had historical roots, setting the stage for different developmental trajectories and influencing contemporary economic and social outcomes.

Once the war began, the nature of the conflict varied dramatically between the North and South. The North, heavily bombarded by U.S. airstrikes, suffered widespread destruction of infrastructure, industry, and civilian areas. This relentless bombing campaign, part of Operation Rolling Thunder, aimed to cripple the North's capacity to wage war and support the Viet Cong in the South (Nalty, 1998). In contrast, the South experienced a different kind of warfare, characterized by guerrilla tactics, widespread insurgency, and conventional battles. The Viet Cong, supported by the North, engaged in asymmetrical warfare, blending into the civilian population and launching unexpected attacks. In addition, the South faced significant political instability, with frequent changes in government and internal strife, complicating the war effort and affecting daily life. These distinct wartime experiences not only shaped the immediate hardships faced by the populations in each region but also left lasting legacies on their post-war recovery and development trajectories.

Finally, it is important to note that the differences between North and South Vietnam, as well as the variations observed along latitudes, are not primarily related to ethnic groups, contrary to what MN discussed (p. 9). Instead, these differences can be traced back to distinct institutional legacy that began more than a millennium ago, shaped by various institutional, political, and economic regimes, and further reinforced by events in the latter half of the twentieth century. Evidence indicates that these historical processes have led to the development of separate social and political cultures, largely independent of the ethnic communities living in contemporary Vietnam.

Despite the presence of 54 ethnic groups in Vietnam today, the society is relatively homogeneous in ethnic terms, with 85% of the population (and the majority of the 2001 survey) belonging to the Kinh ethnic group (General Statistics Office of Vietnam, 2009). However, attributing Vietnam's diversity solely to ethnic diversity oversimplifies the complexities of Vietnamese society, where latitude or the North-South divide has been a significant source of differentiation due to distinct historical trajectories. In other words, these historical and sociological differences suggest that a Kinh individual living in the North may experience a very different sociological and economic reality than a similar Kinh individual living in the South, due to these deep-rooted historical processes. Therefore, controlling for respondents' ethnic group, as MN did (p. 9), cannot substitute for the critical control of the South or latitude variables and does not address the omitted variable bias that arises from not accounting for the differences between the South and North.

### *3.2 Empirical Reasons for Controlling for Prewar Residence in South and North Vietnam*

As previously discussed, prewar residence in either North or South Vietnam is a plausible confounder in the association between wartime exposure and contemporary civic engagement if it is (a) associated with the treatment, (b) associated with the outcome, and (c) not an intermediary variable. Thus far, historical and contextual evidence supports points (b) and (c). First, the distinct institutional trajectories between the South and the North, linked to long-term effects on economic development and civic engagement, as documented by Dell et al. (2018), demonstrate this plausible association. Secondly, the origins of these divergent trajectories can be traced back to centuries of institutional development, making the prewar region of residence a non-intermediary factor. However, I have not yet provided any evidence explaining why prewar residence in either North or South Vietnam could be related to the distance to the 17th parallel (point a).

In this section, I will explore this issue in detail. First, I will demonstrate that the association does not arise mechanically through structural multicollinearity, as suggested by MN (p. 9), but rather due to the asymmetrical distribution of respondents across the 17th parallel in Vietnam—respondents in the North tend to live closer to the 17th parallel than those in the South. I will then show that models failing to account for this asymmetrical distribution essentially conflate the estimated effect of distance with that of a simple North-South divide, thereby incorporating a significant amount of confounding bias into their estimates, as evidenced by the strong association between the model residuals and prewar geographic controls. To address this concern, I will propose two methods to mitigate omitted variable bias in this context: (i) controlling for the relevant pretreatment confounder and (ii) restricting the sample to ensure a symmetrical distribution of respondents across the border. Finally, I demonstrate that applying either method produces an estimated effect that closely aligns with the originally reported results.

### *3.3 Examining the Association Between Prewar Residence and Distance to the 17th Parallel*

There are two potential reasons for observing a correlation between prewar residence in South or North Vietnam and distance to the dividing border: (i) structural multicollinearity resulting from mechanical processes in the creation of the variables (as suggested by MN, 2024) or (ii) the asymmetrical distribution of respondents across the 17th parallel border. I will demonstrate that the latter, rather than the former, explains the observed correlation.

**Mechanical processes or structural multicollinearity.** The first plausible reason for this association is that it could arise mechanically from the way the variables are created. According to MN (2024), distance is strongly correlated with both south and latitude due to structural multicollinearity, a supposed mathematical artifact caused by deriving new predictors from existing ones. This implies that the correlation should emerge when using south and latitude in a model where distance is computed from them, regardless of the actual values of the observations.

I demonstrate that this claim is factually incorrect using simulated data under a scenario of pure symmetry of respondents across the dividing line through a numerical simulation. First, 100,000 observations are generated using a uniform distribution along a one-dimensional space[2] between 9 and 23, roughly equivalent to the southernmost and northernmost parallels within Vietnam. A border is then placed at the midpoint, dividing the space so that 50% of the observations are below the midpoint and 50% are above it. In this scenario, I generate: (i) a dummy variable for South indicating whether an observation is below the midpoint; (ii) a latitude variable indicating its values in the uniform distribution; and (iii) a distance-to-the-midpoint variable, taking the absolute value of the difference between the latitude variable and the midpoint. This setup represents a simplified version of Barceló's research design under conditions of pure symmetry.

Figure 2 graphically represents this numerical exercise, showing the uniform distribution and reporting the correlations among these variables under a symmetrical scenario to observe their mechanical associations under perfect conditions. This exercise helps to visualize two striking features of the design:

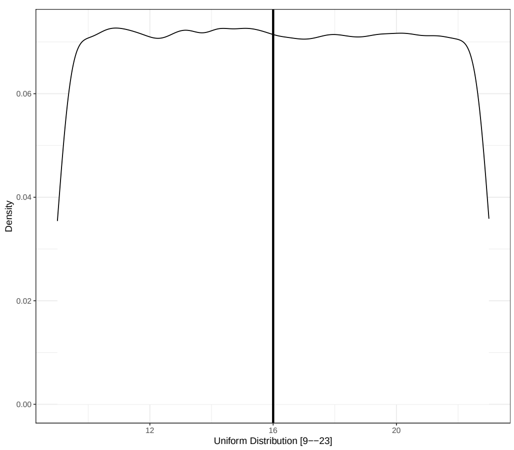
- South is *not* mechanically correlated with distance to the midpoint. The correlation between South and Distance in a symmetrical scenario is exactly 0.

[2]Results do not change if I use a two-dimensional space.

- Latitude is *not* mechanically correlated with distance to the midpoint. The correlation between Latitude and Distance in a symmetrical scenario is exactly 0.
- South and Latitude are strongly and mechanically associated, with a correlation of 0.87.

In conclusion, under a symmetrical scenario, the two control variables used in the regression models reported in Barcelo (2021), *South* and *Latitude*, are indeed mechanically associated by design. However, they are not associated with *Distance*, the treatment of interest, and therefore should not have an impact on the causal estimate of interest. It is crucial to remember that the primary concern in causal inference is whether control variables affect the relationship between the treatment and the outcome. If the control variables are correlated with each other but not with the treatment, they do not bias the causal estimate. Therefore, the strong association between South and Latitude does not impact the causal inference regarding the treatment of interest.

Figure 2: Distribution and Correlation of Variables Under a Symmetrical Scenario



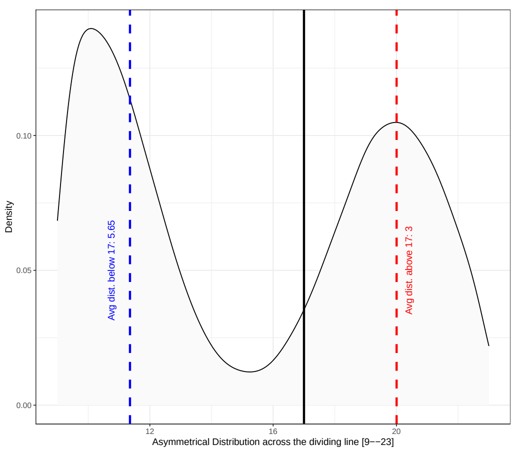

| | Latitude | South | Distance |
|---|---|---|---|
| Latitude | 1.00 | -0.87 | 0.00 |
| South | -0.87 | 1.00 | 0.00 |
| Distance | 0.00 | 0.00 | 1.00 |

**Asymmetrical distribution of respondents across the dividing line.** The second reason we might observe an association between South and Distance is due to an asymmetrical distribution of respondents across the 17th parallel. This asymmetry may occur if respondents do not live symmetrically across the border, with comparable distances in both the south and north. For instance, if people in the South live farther from the border than those in the North, then the variation in the border variable conflates distance with North-South differences.

To illustrate this point, I modified the numerical example with two specific changes: (i) moving the border from the midpoint to the 17th parallel, and (ii) adjusting the distribution of respondents so that people in the South live farther from the border than those in the North. Specifically, the distribution is bimodal, with a peak in the South at 10 and a peak in the North at 20, each with a standard deviation of 2, and observations truncated at the southern and northern borders. This results in an average distance to the border of 5.65 for those below the dividing line and 3 for those above it. Both the shift of the border and the unequal distribution of observations above and below the dividing line reinforce an asymmetrical scenario.

Figure 3 illustrates the empirical implications of the asymmetrical distribution of respondents across the border. Under these conditions, the design causes the correlations between South and Distance, as well as Latitude and Distance, to strengthen proportionally to the degree of deviation from a purely symmetrical scenario. This effect is clearly depicted in the figure, where the bimodal distribution of the respondents and the resulting correlation matrix demonstrate how these variables interact. *Latitude* and *South* remain strongly associated with each other ($r = -0.93$), as observed in the symmetrical scenario. The major difference now is the emergence of correlations of $-0.65$ and $0.61$ between *Latitude* and *Distance* and *South* and *Distance*, respectively. These correlations highlight how the asymmetrical distribution impacts the relationships between these variables, posing a significant threat to the validity of our estimates.

Figure 3: Distribution and Correlation of Variables Under an Asymmetrical Scenario



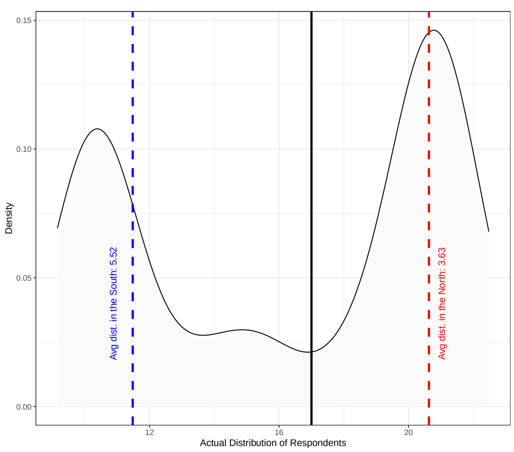

| | Latitude | South | Distance |
|---|---|---|---|
| Latitude | 1.00 | -0.93 | -0.66 |
| South | -0.93 | 1.00 | 0.62 |
| Distance | -0.66 | 0.62 | 1.00 |

**How Does the Actual Distribution of Respondents Look in Vietnam?** While the above discussion highlights the potential reasons for correlations between *South* and *Distance*, and *Latitude* and *Distance* under symmetrical and asymmetrical scenarios, it is important to examine the actual distribution of respondents in Vietnam. If respondents in Vietnam are relatively symmetrically distributed across the border, the bias introduced by omitting the variable of prewar residence is likely to be small. However, if respondents are highly asymmetrically distributed, the potential bias can be significant.

Figure 4 presents the actual distribution of respondents along latitude, along with the corresponding correlation matrix. It shows that the actual distribution of respondents in Vietnam closely resembles a scenario of strong asymmetry across the border, rather than one under conditions of pure symmetry. As a result of this asymmetry, significant correlations emerge between South and Latitude, as well as between *Latitude* and *Distance*.

This asymmetrical scenario suggests that if respondents in the North differed from those in the South due to historical dynamics predating the treatment, and as observed, people in the North live closer to the border than those in the South, then distance might be correlated with an outcome

Figure 4: Actual Distribution and Correlation of Variables in Vietnam




| | Latitude | South | Distance |
|---|---|---|---|
| Latitude | 1.00 | -0.95 | -0.65 |
| South | -0.95 | 1.00 | 0.51 |
| Distance | -0.65 | 0.51 | 1.00 |

not because of the distance itself but because of pre-existing differences related to North residence. Consequently, this asymmetrical distribution of respondents across the border indicates that prewar residence is a crucial variable in the study design and its omission must be addressed in any carefully constructed regression model that aims to estimate a causal effect accurately. Unfortunately, this is precisely what MN omit in their re-analysis, introducing bias into their estimates proportional to the deviation from a purely symmetrical scenario in the real-world context of Vietnam.

## 3.4 *Does OVB Affect MN's Regression Estimates?*

Although I have thoroughly reviewed the reasons for adjusting for respondents' prewar residence, it is still important to empirically assess whether this adjustment is necessary using evidence from the dataset and the results of various model specifications. Specifically, I will examine whether information on prewar residence–whether measured as living in the south or north of Vietnam or by prewar latitude – is redundant in a regression model, as MN suggest, because it is already captured by the main predictor, distance to the border. If south, latitude, and distance provide the same information, as suggested by their presumed collinearity, the residuals from the regression models should be uncorrelated with any of the observed, yet excluded, variables.

Hence, the core essence of the empirical debate between Barceló (2021) and MN (2024) centers around differing claims about model specification. Barceló argues that excluding both *South* and *Latitude* from the model results in the error term $\epsilon_i$ being correlated with $T_i$, leading to biased and inconsistent estimates of $\beta_1$. In contrast, MN contend that due to the strong correlation between *South*, *Latitude*, and $T_i$, excluding the former two variables should not result in the error term $\epsilon_i$ being correlated with $T_i$, as they are deemed redundant. Given the empirical nature of this debate, I should turn to evidence from the regression analyses to shed light on the conflicting views.

Returning to the fundamentals, we should recall that when OLS assumptions are met, the Gauss-Markov theorem ensures that estimates will be unbiased and have minimum variance. However, omitted variable bias occurs when residuals correlate with a variable that precedes and is associated with both the treatment and the outcome. In a regression model with two significant independent variables, X1 and X2, which are correlated with each other and the dependent variable, removing X2, the confounding variable, from the model results in a poorer fit, increasing the residuals. These residuals then correlate with X2. Since X1 correlates with X2, X1 also correlates with the residuals, violating the OLS assumption that independent variables do not correlate with residuals, leading to biased estimates.

The key question is whether the residuals in MN's (2024) models are correlated with either *South* or *Latitude*. If the correlation is low, a model excluding both South and Latitude should still reveal a treatment effect of distance/bombing on civic engagement. However, if the residuals in their models show significant correlation with either of these excluded variables–South or Latitude–it indicates a critical misspecification of MN's models, violating a fundamental assumption of OLS and, by extension, IV.

Table 1, columns 1-3, reproduce the coefficients from MN's (2024) main models, which supposedly show the insignificance of Barcelo's findings. At the bottom of the table, I report the correlation between the model residuals and *South* and *Latitude* for both the OLS and IV models. The results show that the residuals in MN's models are strongly correlated with *South* (Pearson's correlation ranging from 0.32 to 0.39) and *Latitude* (Pearson's correlation ranging from 0.41 to 0.46).

This finding leads to two significant conclusions. First, MN did not perform basic regression diagnostics on their reanalysis models, nor on any other regression models reported in their paper. Second, as a result, their models are fundamentally flawed due to OVB, undermining any conclusions drawn from these specifications and affecting every regression estimate presented in their paper.

Table 1: A Re-analysis Addressing Omitted Variable Bias in MN by Controlling for Confounders

| | MN (2024) | | | Barcelo (2021) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | Appendix X | | | Appendix X | | | Main text | | |
| | First Stage | VIF | Second Stage | First Stage | VIF | Second Stage | First Stage | VIF | Second Stage | First Stage | VIF | Second Stage |
| Bombs, per km2 (log) | | | −0.72* | | | 0.22*** | | | 0.24*** | | | 0.23*** |
| (residence pre-1975) | | | (0.32) | | | (0.08) | | | (0.06) | | | (0.06) |
| Female | 0.06 | 1.06 | −0.02 | 0.03 | 1.06 | −0.07** | 0.02 | 1.06 | −0.07* | 0.02 | 1.06 | −0.07* |
| | (0.09) | | (0.07) | (0.06) | | (0.03) | (0.05) | | (0.03) | (0.04) | | (0.03) |
| Age | 0.00 | 1.09 | 0.00 | 0.00* | 1.09 | −0.00 | 0.00* | 1.09 | −0.00* | 0.00 | 1.10 | −0.00 |
| | (0.00) | | (0.00) | (0.00) | | (0.00) | (0.00) | | (0.00) | (0.00) | | (0.00) |
| Education | 0.06 | 1.12 | 0.07** | 0.04* | 1.13 | 0.02* | 0.03 | 1.13 | 0.02** | 0.02 | 1.14 | 0.02** |
| | (0.04) | | (0.03) | (0.03) | | (0.01) | (0.02) | | (0.01) | (0.02) | | (0.01) |
| Pop Density/1000 | 0.61*** | 1.13 | 0.44* | 0.38 | 1.23 | −0.10* | 0.38*** | 1.20 | −0.12*** | 0.40*** | 1.23 | −0.10*** |
| | (0.13) | | (0.20) | (0.13) | | (0.06) | (0.14) | | (0.05) | (0.14) | | (0.06) |
| Pre Avg/100 | 2.23*** | 1.46 | 1.54* | −0.72 | 2.99 | −0.18 | −0.47 | 2.35 | −0.32*** | −0.08 | 3.14 | −0.14 |
| | (0.52) | | (0.65) | (0.16) | | (0.16) | (0.49) | | (0.16) | (0.60) | | (0.21) |
| South | | Omitted | | 2.24*** | 2.90 | −0.27** | | Omitted | | −0.93*** | 15.0 | −0.45*** |
| | | | | (0.55) | | (0.11) | | | | (0.48) | | (0.14) |
| Latitude | | Omitted | | | Omitted | | −0.28*** | 2.91 | 0.03*** | −0.36*** | 15.1 | −0.02 |
| | | | | | | | (0.04) | | (0.01) | (0.05) | | (0.02) |
| Distance to Border | 0.07 | 1.43 | | −0.45*** | 3.56 | | −0.59*** | 3.83 | | −0.59*** | 3.86 | |
| (Instrument) | (0.10) | | | (0.14) | | | (0.10) | | | (0.09) | | |
| Intercept | −1.90 | | −1.29** | 4.06** | | −0.24 | 10.01*** | | −0.63* | 11.23*** | | 0.04 |
| | (1.06) | | (0.47) | (1.76) | | (0.27) | (1.94) | | (0.34) | (1.72) | | (0.48) |
| N Clusters | | 48 | | | 48 | | | 48 | | | 48 | |
| Observations | | 862 | | | 862 | | | 862 | | | 862 | |
| **Correlation of Residuals with Variables Omitted in MN (2014)** | | | | | | | | | | | | |
| r($\epsilon_i$, $Latitude$) | $r = 0.46$*** | | $r = -0.41$*** | $r = 0.15$*** | | $r = -0.01$ | $r = 0$ | | $r = 0$ | $r = 0$ | | $r = 0$ |
| r($\epsilon_i$, $South$) | $r = -0.39$*** | | $r = 0.32$*** | $r = 0$ | | $r = 0$ | $r = 0.05$ | | $r = -0.03$ | $r = 0$ | | $r = 0$ |

*Notes:* Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Province-level heteroskedastic clustered standard errors in parentheses. The dependent variable is the civic engagement log (in 2001).

### *3.5 What Is the Solution to Address Confounding Bias?*

The asymmetric distribution of respondents across the border fundamentally biases models like those presented by MN (2024) because they do not account for potential pre-existing differences between individuals living in South and North Vietnam before the war. By failing to adjust for this variable, substantial omitted variable bias (OVB) is introduced, undermining their conclusions. The critical question then becomes: Can we develop a model that avoids omitted variable bias?

#### *3.5.1 Solution #1: Controlling for confounders*

The standard approach for addressing omitted variable bias involves including the omitted variables directly in the model. While omitted variables are often unobserved or difficult to measure, making it challenging to resolve confounding bias, this case is different. Here, the variables are both observed and measurable, allowing for a straightforward solution: simply include these variables in the model to effectively address the bias.

Table 1, columns 4-6, corrects MN's model misspecification by adjusting for a dummy variable indicating whether a respondent lived in *South Vietnam* before the war. The correlation matrix at the bottom of the table shows that incorporating this variable substantially mitigates the OVB, as the residuals from this model are correlated with latitude to a much smaller extent in the OLS but not in the IV model (Pearson's correlation ranging from 0.01 in the IV model to 0.15 in the OLS model). The results from this modification show that the estimated treatment effect is significant and positive, with a coefficient of 0.22, which is roughly equivalent to the coefficient of 0.23 reported in the main model in Barceló (2021). Therefore, simply adjusting for the confounder recovers the main estimate of the model.[3]

An alternative specification is to use a more detailed measure of prewar geographic location: prewar *Latitude*. Unlike a simple North-South divide, prewar latitude can also account for differences within the northern and southern regions of Vietnam, providing a more granular measure of pre-existing geographic variations.[4] Table 1, columns 7-9, rectifies MN's model misspecification by simply controlling for the *Latitude* of the respondents' prewar place of residence. Once the model adjusts for this confounder, the estimated effect of exposure to wartime violence on civic engagement remains positive and significant, with a substantive effect even stronger than that reported in the main analysis of the paper. Reassuringly, the correlation matrix at the bottom of the table shows that incorporating this variable completely mitigates the OVB, as the residuals of such a model are no longer correlated with *South* (Pearson's correlation of the residuals with *South* of 0.05 and 0.03 for the OLS and IV models, respectively).

Finally, Table 1, columns 10-12, presents the results after controlling for both *South* and *Latitude*

[3] These models were originally presented in Appendix X of Barceló (2021). The only difference in this version is that the standard errors reported here are province-level heteroskedastic clustered standard errors in parentheses, rather than the originally-reported robust standard errors, yet the results are the same.

[4] Note that if pre-existing differences between the north and south of Vietnam are related to their distinct institutions and forms of governance within and outside Đại Việt, then the gradual and millennia-long process of Đại Viêt's expansion means that, for example, northern regions in South Vietnam might share more characteristics with the north due to their longer period under similar governance. Therefore, the variable *Latitude* may also serve as a sensitive indicator of the North-South divide, capturing the different historical and institutional prewar trajectories.

within the same model, originally reported in Barceló (2021). Although, as discussed, controlling for both variables is not strictly necessary to obtain an unbiased treatment effect, incorporating both *South* and *Latitude* together effectively captures the pre-existing differences between northern and southern regions of Vietnam. These regions have experienced different historical institutions, prewar conditions, and varying intensities of warfare. This model thus provides an unbiased estimate of the effect of wartime exposure on civic engagement, free from the omitted variable bias present in MN's models, and consistently demonstrates a significant positive effect of exposure to wartime violence on civic engagement in Vietnam.

**Is Collinearity a Problem?** It was stated that the originally-reported model in Barceló (2021) contains an error due to the inclusion of collinear covariates: *South* and *Latitude*. As shown in Table 1, the VIF for *South* and *Latitude* are 15.65 and 15.04, respectively, in the first stage model presented in column 10. MN's solution to the supposed multicollinearity issue is to drop both covariates from all their regression models.

Given that this decision significantly impacts the results of the paper, it is crucial to revisit the fundamentals and consider two related questions: (1) When does multicollinearity become a problem? (2) What are the appropriate solutions to multicollinearity, and what should be avoided?

**(1) Is multicollinearity a problem for Barceló's models?**

In Barceló's correction (2023), a paragraph was added to specifically address the issue of collinearity, stating: "I noticed that two control variables in the primary models, namely Latitude and South, are collinear. This is demonstrated by a Variance Inflation Factor (VIF) above 10 in the second-stage regression. However, it is important to note that this collinearity does not influence the instrumental variable, distance to the 17th parallel, or the primary predictor of interest, the log of the number of bombs per square meter. While collinearity between two control variables does not pose a significant issue for interpreting the main estimates."

However, MN (2024) disregarded this information in their re-analysis and incorrectly argued that a Variance Inflation Factor (VIF) above 10 for South and Latitude, two control variables, posed a threat to the validity of the causal estimate. It is well-known that multicollinearity between control variables does not affect the main estimates of a model as long as it does not involve the main predictor. Importantly, the VIF for the main treatment, *Distance to the Border*, is 3.86 in the model that includes both *South* and *Latitude* (see Table 1, column 10). Therefore, *collinearity does not affect the main models in Barceló's paper because it does not affect the main estimate from which causal inference is drawn*.

This distinction between "good controls" and "bad controls" is important for understanding why collinearity among control variables does not undermine the validity of causal estimates. As emphasized Angrist & Pischke (2009), "good controls" are variables that help to isolate the effect of the treatment by accounting for confounding factors, while "bad controls" are those that may themselves be affected by the treatment, thus introducing bias. The key point is that, as long as the controls used are appropriate ("good controls"), any multicollinearity among them does not compromise the causal inference derived from the main predictor.

While a proper formal justification for this point can be found in numerous econometric textbooks, (Voss, 2005, 765) made this point extremely clear:

> "Predictive models often contain a wide variety of explanatory variables, only some of which directly concern the researcher. The remainder, usually called control variables, appear in an analysis simply to increase the accuracy of the theoretically important coefficient estimates. The researcher need not worry whether coefficient estimates for control variables are close to the truth, nor does an analyst necessarily mind if these coefficients are accompanied by high standard errors (that is, whether they achieve statistical significance). What matters is that these rival explanations appeared in the analysis at all, thereby protecting other coefficient estimates from omitted variable bias.
>
> Partial multicollinearity among control variables is almost entirely harmless. It does not undercut their effectiveness in eliminating omitted variable bias. It does not produce any sort of bias. It does not reduce the fit of a regression. Coefficient standard errors properly report the uncertainty attached to each estimate; there should be no opportunity to place more stock in a given coefficient than it deserves. The only risk is if, aside from random noise, the control variables lack independent variation, overlapping so completely that they are redundant. This circumstance would create inefficiency in a regression model, which could be problematic in very small data sets. But such costs appear any time analysts include unnecessary variables in a model; they are not unique to cases of partial multicollinearity. Otherwise, unless the researcher places more confidence in coefficient estimates than is warranted by their level of uncertainty–an indefensible flaw–partial multicollinearity in the control variables does not disrupt an analysis."

**(2) What are the solutions to partial collinearity, and what should be avoided?**
Although I have demonstrated that partial collinearity between two control variables does not threaten the validity of a treatment effect, MN commit two compounded errors in this regard. Not only do they attempt to solve a nonexistent problem, but they also address it incorrectly.

Basic textbooks on model specifications advise that to address collinearity, scholars should either remove redundant variables or merge collinear variables. By carefully selecting and removing only redundant variables, researchers can avoid collinearity while also mitigating omitted variable bias. For instance, in a political science study examining the effect of a civic education program (treatment) on voter turnout, if both "Income" and "Social Class" (pre-treatment control variables) are included in the model and are highly collinear, one might choose to remove "Social Class" if "Income" sufficiently captures the necessary socioeconomic information. Both income and social class are related to program uptake and voter turnout, so controlling for them is crucial. This approach retains the relevant information while eliminating redundancy, thus addressing collinearity without introducing omitted variable bias. Without addressing the collinearity, the model could be specified as follows:

$$\text{Voter Turnout} = \beta_0 + \beta_1(\text{Civic Education Program}) + \beta_2(\text{Income}) + \beta_3(\text{Social Class}) + \epsilon$$

After addressing collinearity by removing the redundant variable "Social Class," the model would be:

$$\text{Voter Turnout} = \beta_0 + \beta_1(\text{Civic Education Program}) + \beta_2(\text{Income}) + \epsilon$$

This adjusted model avoids collinearity among control variables while, at the same time, reduces the risk of omitted variable bias.

Certainly, Barceló (2021, 2023) includes *South* and *Latitude* in the model to capture a similar source of confounding: the North-*South* differences in pre-existing characteristics. These differences are particularly problematic due to the asymmetric distribution of respondents across the border in Vietnam. Although these are control variables and their collinearity should not be a major concern, the standard approach to addressing collinearity while maintaining model specification is to remove redundant variables. In this case, this means removing either *South* or *Latitude*.

Table 1, columns 4-8, demonstrates this approach. Columns 4-6 show a model specification where *Latitude* is omitted, and columns 7-9 show a model where *South* is omitted instead. In both models, after excluding one of the control variables, we find that the Variance Inflation Factor (VIF) for all variables falls below the standard threshold of 10 used in econometric literature, indicating the absence of collinearity in both models. These models, which have no collinearity, also show minimal omitted variable bias arising from prewar geographic factors due to the significant reduction in model residuals associated with the corresponding omitted variable. As reported, both models recover a treatment effect that is nearly identical to the one found when both control variables are included. This approach–removing redundant variables to address collinearity–is standard practice in the literature.

In contrast, MN (2024) deviate from this established method by opting to eliminate all control variables affected by partial collinearity. While their approach does remove collinearity, it inadvertently introduces significant confounding bias into their analysis. Instead of merely addressing the issue of collinearity by removing redundant variables, their method disrupts the model's ability to account for important pre-treatment confounding factors, thereby invalidating their causal inferences.

Following the example above, if a researcher decides to remove not just the redundant variable but all control variables affected by the initial collinearity (in this case, both "Income" and "Social Class"), this would introduce significant problems. The resulting model might be specified as follows:

$$\text{Voter Turnout} = \beta_0 + \beta_1(\text{Civic Education Program}) + \epsilon$$

By removing both "Income" and "Social Class," the researcher fails to control for important pre-treatment factors that influence both program uptake and voter turnout. This omission can lead to omitted variable bias, where the estimated effect of the civic education program on voter turnout is confounded by uncontrolled socioeconomic differences among respondents. As a result, the treatment effect estimates would be biased and unreliable, capturing not only the effect of the program but also the underlying impact of the omitted variables, "Income" and "Social Class."

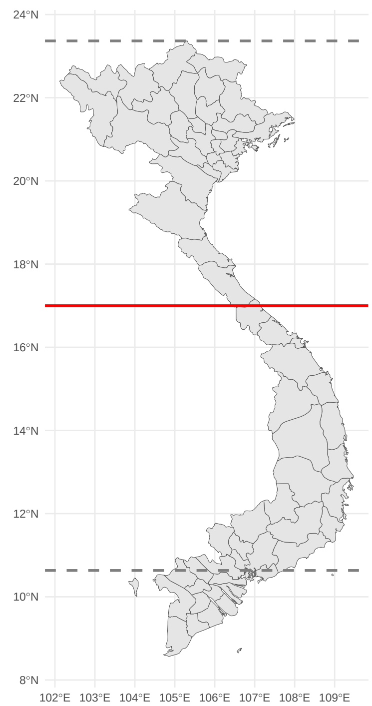
Returning to Barcelo's models and considering the importance of controlling for prewar factors in the Vietnamese context, it becomes evident that MN's approach undermines the validity of their causal inference by conflating the true effect with the spurious effects of the omitted geographic factors. In other words, MN's approach is akin to attempting to cure a disease that does not exist by eliminating the patient.

### *3.5.2 Solution #2: Restricting the Sample to Approach Symmetry Across the Border*

As previously demonstrated, the partial collinearity observed between South and Distance, and between Latitude and Distance, emerges from the asymmetrical distribution of respondents across the 17th parallel in Vietnam. Although the standard method to address this issue involves directly controlling for these factors, an alternative approach is to create a sample that is symmetric across the border. This can be achieved by excluding individuals in the South who live farther from the 17th parallel than the distance from the northernmost point in the North to that parallel. This means removing individuals in the South who do not have a comparable counterpart in the North regarding their distance from the border, thereby ensuring a balanced sample with common support across both sides of the border. Figure 5 displays the geographic restrictions of the sample to achieve this, whereby respondents to the south of $10.6337$ latitude – this is equivalent to $17$ minus the difference between 17 and $23.36628$, the latitude of the northernmost point of Vietnam.[5] After applying this restriction, the sample size decreases from 862 respondents in the fully specified model to 689 respondents, resulting in the exclusion of approximately 20% of the respondents who reside in the southernmost part of Vietnam. While this reduction in sample size may result in a less efficient model specification due to fewer observations and province clusters, making it not the ideal setup, it does ensure a symmetrical distribution of respondents across the border.

[5] $17 - (23.36628 - 17) = 10.6337$.

Figure 5: Map of Vietnam with the Sample Restrictions to Approach Symmetry Across the Border




The results reported in Table 2 reveal two notable findings. First, the estimated effect of exposure to wartime violence on civic engagement remains robustly positive and significant, even after omitting *South* and *Latitude* from the model specification in a sample that approaches symmetry across the border. The substantive effect size of 0.22 is comparable to the originally reported coefficient of 0.23 in Barceló (2021). Second, the robustness of the main effect after omitting South and Latitude comes along with a reduction of correlation between these geographic controls and distance when the sample is symmetrical across the border – changes in correlations from 0.65 to 0.35 with *Latitude* and from 0.51 to 0.35 with *South*. In other words, the robustness of the effect is maintained because adjusting for prewar geographic location is no longer necessary when the sample is symmetrical, resulting in a lower correlation between distance and *South* and distance and *Latitude*.

In summary, this alternative specification reinforces the central finding of this manuscript: MN's models are significantly affected by confounding bias. Once this bias is addressed–either by controlling for the confounding factors or by restricting the sample to avoid such bias–the results consistently reveal a robust, strong, and positive effect of exposure to wartime violence on civic engagement in Vietnam.

Table 2: Re-Analysis of MN (2024) After Restricting the Sample to Approach Symmetry Across the Border

| | First Stage | VIF | Second Stage |
|---|---|---|---|
| Bombs, per km2 (log) | | | 0.22*** |
| (residence pre-1975) | | | (0.06) |
| Female | −0.03 | | −0.05 |
| | (0.05) | | (0.03) |
| Age | −0.00 | | −0.00 |
| | (0.00) | | (0.00) |
| Education | 0.02 | | 0.02* |
| | (0.02) | | (0.01) |
| Pop Density/1000 | 1.24*** | | −0.19*** |
| | (0.22) | | (0.05) |
| Pre Avg/100 | 1.50** | | −0.47*** |
| | (0.64) | | (0.20) |
| South | | Omitted | |
| Latitude | | Omitted | |
| Distance to Border | −0.55** | | |
| | (0.21) | | |
| Intercept | 1.39 | | 0.14 |
| | (1.61) | | (0.30) |
| Observations | 689 | | 689 |
| N Clusters | 39 | | 39 |

Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variable is the civic engagement log (in 2001). Province-level heteroskedastic clustered standard errors are in parentheses.

## 4 Robustness to MN's Alternative Story

After thoroughly discussing the appropriate model specification for analyzing the main effect, I will now evaluate the robustness of the findings in relation to the arguments proposed by MN. First, I will examine whether the association between exposure to wartime violence and civic engagement is confounded by historical communist party strength, as MN suggested. Second, I will assess whether this association reflects broader societal changes as argued by Barceló, rather than being limited to the behavior of party members as argued by MN. Additionally, I will investigate whether the observed effects are predominantly present in organizations controlled by the communist party, indicating a potential partisan-oriented bias.

### 4.1 Historical Communist Party Strength

The central argument of MN is that the observed relationship between exposure to war during the Vietnam War and contemporary civic engagement is not driven by the effects of the exposure itself. Instead, they contend that the areas targeted by bombings were already strongholds of pre-war communist insurgency, which naturally continued to exhibit high levels of loyalty to the communist regime after the war, reflected in elevated participation in social and political organizations. Following this argument, one would hypothesize that once pre-war party strength is accounted for, the association between exposure to wartime violence and civic engagement would diminish, revealing the relationship to be spurious. Although MN compiled a dataset on pre-war party strength, they did not empirically evaluate its impact on the main findings in Barceló (2021). They simply demonstrated that pre-war party strength is correlated with both exposure to wartime violence and civic engagement, without assessing the extent to which these correlations challenge the validity of the causal estimate. I will now do what they chose not to: evaluate whether pre-war party strength is a confounder in the association between exposure to wartime violence and contemporary civic engagement.

Although historical communist party strength was unmeasured and therefore unobserved in the model specification reported in Barceló (2021), MN (2024) constructed two proxy variables for prewar party strength: Zone IV Provinces and the Birthplace of Second Committee Members. They argue that Zone IV, including the provinces of Thanh Hoa, Nghe An, and Ha Tinh, was a crucial hub for the Democratic Republic of Vietnam (DRV) during the First Indochina War, providing significant labor, recruits, and food supplies, particularly during the Battle of Dien Bien Phu. Thus, whether a province was part of Zone IV could serve as a proxy for the strength of the communist party before the war. Furthermore, despite limited historical data on party membership by region, MN suggested that the birthplace of Central Committee (CCOM) members could also indicate party strength. Historically, Zone IV provinces had significantly higher representation in the Central Committee, underscoring their enduring political influence and strong party presence both before and after the war.

To rigorously test for spuriousness, proxy variables for prewar party strength, such as Zone IV Provinces and the Birthplace of Second Committee Members, should be included in the regression model. Controlling for these factors allows us to assess if the estimated effect of wartime exposure on civic engagement remains significant. If including these controls renders the effect size statistically insignificant, it would suggest that the original association was indeed confounded by prewar party strength. Conversely, if the main effect persists even after accounting for these proxies, it would suggest that the relationship between wartime violence and civic engagement is not merely an artifact of prewar political conditions but reflects a causal association.

Table 3 outlines four model specifications to assess the validity of MN's argument. Column 1 presents the second-stage regression model, analyzing the impact of wartime violence on civic engagement while controlling for a dummy variable indicating whether respondents' wartime provinces were in Zone IV, as defined by MN (2024). Despite the significant correlation of Zone IV with both wartime violence and civic engagement, the effect of wartime exposure on civic engagement remains robust, even after accounting for this proxy for prewar party strength.

To further assess the robustness of the finding, column 2 presents models that exclude all respondents who lived in a province within Zone IV, which encompasses 19% of the sample. This analysis helps evaluate whether the main association between wartime exposure and civic engagement persists without the potential influence of these historically significant provinces. The results confirm that the effect of wartime exposure on civic engagement remains positive and statistically significant even when focusing on respondents who lived outside of Zone IV before the war.

Additionally, I employ MN's second proxy for historical communist party strength–the number of Second Central Committee members from 1951 born in each province–as an alternative control variable in the main models. Column 3 shows that controlling for this proxy does not alter the causal effect of wartime violence on civic engagement, which remains significantly positive at the 99% confidence level with an effect size identical to that originally reported in Barceló (2021). In column 4, I further assess the robustness of the main effect by restricting the sample to individuals from provinces with no Second Central Committee members born there. While this adjustment reduces the sample size by 40%, the main effect persists largely unchanged, continuing to demonstrate a positive association between exposure to wartime violence and current engagement in organizations.

Overall, these models confirm the robustness of the causal effect of wartime violence on civic engagement in Vietnam, even when considering historical communist party strength. While correlations between wartime exposure and civic engagement in historical communist strongholds are evident, they do not detract from the fundamental finding: individuals from areas heavily bombed during the Vietnam War continue to demonstrate higher levels of participation in social organizations, regardless of prewar communist party strength.

### *4.2 Party Membership and Participation in Social Organizations*

In their critique, MN also argued that the WVS data reports an inflated party membership rate compared to the national average.[6] Although it is true that the WVS oversamples party members, this does not necessarily pose a threat to the validity of the causal estimate.

[6] The party membership rate in the WVS is 23.8% compared to a national average of 4.6%. Note that MN incorrectly report a 3% national average because they do not take into account that the survey only includes respondents above the age of 18, and correspondingly, party membership should also be restricted to those above the age of 18. When the calculation is conducted with the national population in Vietnam in 2001 older than 18 years in the denominator, the national average of party membership is 4.6%.

Table 3: Robustness of the Wartime Violence Effect After Accounting for Historical Party Strength

| | Control Zone IV | Exclude Zone IV Provinces | Control for 2nd CCOM Provinces | Exclude 2nd CCOM Provinces |
|---|---|---|---|---|
| Bombs, per km2 (log) (residence pre-1975) | 0.21** (0.08) | 0.18*** (0.07) | 0.23*** (0.06) | 0.23*** (0.05) |
| Female | −0.07** (0.03) | −0.07** (0.03) | −0.07** (0.03) | −0.09** (0.04) |
| Age | −0.00 (0.00) | −0.00 (0.00) | −0.00 (0.00) | −0.00 (0.00) |
| Education | 0.02* (0.01) | 0.02* (0.01) | 0.02* (0.01) | 0.00 (0.02) |
| Pop Density/1000 | −0.09 (0.06) | −0.08 (0.06) | −0.07 (0.05) | −0.03 (0.33) |
| Pre Avg/100 | −0.14 (0.20) | −0.16 (0.24) | −0.12 (0.21) | −0.11 (0.27) |
| South | −0.38 (0.25) | −0.21 (0.43) | −0.48* (0.26) | −0.49** (0.38) |
| Latitude | −0.01 (0.03) | −0.00 (0.04) | −0.02 (0.03) | −0.02 (0.04) |
| Zone IV | 0.06 (0.11) | | | |
| # Members 2nd CCOM born in the province | | | −0.04 (0.03) | |
| Intercept | −0.02 (0.47) | −0.23 (0.65) | −0.02 (0.51) | 0.09 (0.57) |
| Observations | 862 | 730 | 862 | 570 |
| N Provinces | 48 | 42 | 48 | 33 |
| Model | IV | IV | IV | IV |
| First Stage | | | | |
| Distance to 17th Parallel | −0.58 (0.14) | −0.64 (0.17) | −0.58 (0.09) | −0.64 (0.09) |
| F-statistic | 16.4 | 14.7 | 41.2 | 49.3 |

Significance levels: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Dependent variable is civic engagement log (in 2001). Province-level heteroskedastic clustered standard errors in parentheses. VIF for the main predictor in the first stage, distance to the 17th parallel, is consistently below the threshold of 10 at: 6.8, 6.02, 3.9, and 3.5, for models in columns 1, 2, 3, and 4, respectively.

To begin with, model regressions can be adjusted by weighting according to the actual proportion of party members in the national population, ensuring the estimates more accurately mirror the population distribution. For example, weights can correct for the overrepresentation of party members, given that non-party members comprise 76.2% of the sample. Secondly, the central question is whether party membership confounds the relationship between wartime exposure and civic engagement. If party members are inherently more likely to engage in civic activities, irrespective of wartime exposure, and are also more likely to have been exposed to bombings, then the observed association might indeed be spurious. However, if the analysis demonstrates that the effect of wartime violence on civic engagement persists even when accounting for party membership, then the issue of oversampling does not compromise the findings. Moreover, if non-party members also exhibit increased civic engagement in response to wartime exposure, it reinforces the argument that the observed relationship is not solely a product of party dynamics. Thus, while the oversampling of party members requires methodological attention, it does not automatically invalidate the causal claims if properly addressed in the analysis.

In Table 4, I empirically investigate whether party membership influences the relationship between wartime exposure and civic engagement. To this end, I employed a weighted instrumental variable model, using individual partisanship as a weight to reflect the actual proportion of party members in the country in 2001 accurately. Column 1 of Table 4 reveals that the second-stage regression of this weighted model indicates that the estimated effect remains positive and statistically significant at the 99% confidence level. This approach helps to ensure that the results are representative of the national demographic distribution.

In Table 4, column 2, I re-analyze the main model by incorporating party membership as a control variable. The estimated treatment effect of exposure to wartime violence remains unchanged, suggesting that the relationship is not driven solely by party dynamics but reflects a broader social response to wartime experiences. Column 3 of the same table isolates the effect of wartime exposure by restricting the analysis to non-party members only, thus addressing any potential bias from the overrepresentation of party members in the WVS data. The findings reveal similar patterns of increased civic engagement among non-party members due to violent experiences, reinforcing the robustness of the results and indicating that the relationship extends beyond individuals affiliated with the party.[7]

Addressing a related point, MN argued that most social organizations in Vietnam are under the control of the Vietnamese Fatherland Front (VFF), an umbrella group aligned with the Communist Party of Vietnam, and that membership in these organizations consists primarily of party members. They suggested that the measures of civic engagement from the World Values Survey (WVS) merely serve as proxies for party strength, reflecting participation in politically-controlled organizations. However, it is overly simplistic to assert that all organizational membership and engagement were exclusively restricted to party members. Participation in social organizations indeed extends beyond

[7] It is important to consider that weighting for party membership, including it as a control variable, or excluding party members might introduce post-treatment bias, as party membership could itself be influenced by wartime exposure. As noted in various studies (**?**Montgomery et al., 2018), adjusting for post-treatment variables can lead to biased estimates because these variables may absorb part of the treatment effect, which is why this adjustment is not incorporated in the main analysis. Despite this potential concern, the results remain robust, suggesting that the effect of wartime violence on civic engagement is not entirely driven by party dynamics.

Table 4: Statistical models

| | Weighted IV Regression | Control for Party Membership | Exclude Party Members |
|---|---|---|---|
| Bombs, per km2 (log) | $0.17^{***}$ | $0.15^{***}$ | $0.15^{***}$ |
| (residence pre-1975) | (0.05) | (0.05) | (0.05) |
| Female | $-0.04$ | $-0.02$ | $-0.04$ |
| | (0.04) | (0.03) | (0.04) |
| Age | $-0.00^{**}$ | $-0.00$ | $-0.00^{*}$ |
| | (0.00) | (0.00) | (0.00) |
| Education | 0.01 | $-0.00$ | 0.00 |
| | (0.01) | (0.01) | (0.01) |
| Pop Density/1000 | $-0.05$ | $-0.05$ | $-0.03$ |
| | (0.05) | (0.04) | (0.05) |
| Pre Avg/100 | $-0.05$ | $-0.05$ | $-0.07$ |
| | (0.18) | (0.16) | (0.17) |
| South | $-0.46^{*}$ | $-0.30$ | $-0.28$ |
| | (0.24) | (0.23) | (0.25) |
| Latitude | $-0.02$ | $-0.02$ | $-0.01$ |
| | (0.03) | (0.03) | (0.03) |
| Party member | | $0.52^{***}$ | |
| | | (0.04) | |
| Intercept | 0.14 | $-0.12$ | $-0.13$ |
| | (0.43) | (0.40) | (0.43) |
| Observations | 862 | 862 | 608 |
| N of provinces | 862 | 862 | 38 |

Significance levels: $^{***}\ p < 0.01$; $^{**}\ p < 0.05$; $^{*}\ p < 0.1$. The dependent variable is civic engagement log (in 2001). Province-level heteroskedastic clustered standard errors in parentheses.

party-controlled circles. Once again, MN did not empirically substantiate their claim, so I turn to this here.

Table 5 details the percentage of non-party members who are members and actively engaged in each type of organization. These percentages illustrate that civic engagement is not exclusive to party members but also prevalent among non-party members. On average, the membership and participation rates for non-party members exceeded 10%. This contradicts claims that non-party member participation was negligible and demonstrates that measures of civic engagement are not simply proxies for party strength. Rather, they represent widespread participation in social activities, with active involvement from both party and non-party members; this evidence directly contradicts MN's assertions.

Table 5: Participation Rates in Social Organizations Among Non-Party Members in Vietnam

| | | % Non-Party Members | |
|---|---|---|---|
| | Organization | Membership | Engagement |
| 1 | Social Welfare Services | 19.4% | 22.1% |
| 2 | Religious Organizations | 9.9% | 9.4% |
| 3 | Education, Arts, Music, Cultural Activities | 11.0% | 9.8% |
| 4 | Political Parties | 0% | 1.4% |
| 5 | Labor Unions | 4.9% | 4.1% |
| 6 | Local Community Actions | 15.5% | 16.2% |
| 7 | Human Rights/Third World Development | 0.7% | 0.6% |
| 8 | Conservation, Environment, Animal Rights | 2.8% | 3.5% |
| 9 | Professional Associations | 9.5% | 7.6% |
| 10 | Youth Work | 10.1% | 9.8% |
| 11 | Sports or Recreation | 12.6% | 12% |
| 12 | Women's Groups | 26.9% | 25.7% |
| 13 | Peace Movements | 4.5% | 2.9% |
| 14 | Health-Related Voluntary Organizations | 8.4% | 9.9% |
| | **Average rate**[a] | **10.5%** | **10.3%** |

The average rates of membership and engagement among non-party members are calculated without including political parties.

While it is clear that Vietnamese citizens could engage in social organizations irrespective of their affiliation with the communist party, MN critique the selection of organizations in the WVS, highlighting that many could be linked to the VFF, except for two categories: Third World development or human rights ("HR", column 7) and Sports or recreation ("Sports", column 11). MN found no VFF-linked organizations in these categories. If MN's premise–that associations are driven primarily by VFF-linked organizations–were accurate, we would not expect to see a significant association between wartime exposure and civic engagement in these two categories. To investigate this, Table 6 details the estimated effects of wartime exposure on civic engagement for each organizational type separately, instead of aggregating into a civic engagement index. The results displayed in Table 6 indicate consistent effects across various organizations, with the magnitude and significance of these coefficients largely independent of the prevalence of VFF-associated organizations within each category. Crucially, significant positive effects are observed for both

"Third World development or human rights" and "Sports or recreation". This finding challenges MN's assertion that the observed broader association between wartime exposure and civic engagement is predominantly influenced by the presence of VFF-linked organizations.

## 5 *Minor points*

MN highlight four additional critiques in their analysis that have not been directly addressed in this manuscript. They claim that: (1) the way standard errors are clustered affects the significance of the main findings; (2) there may be a potential miscoding of the variable *South*; (3) the original results are only significant for North Vietnam, not for South Vietnam; and (4) ethnicity fixed effects could confound the main relationship. I will address each of these points in the following sections.

### 5.1 *Clustering of the Standard errors*

The first point concerns the clustering of standard errors. MN stress the significance of using province-level heteroskedastic clustered standard errors instead of robust standard errors, as utilized in Barceló (2023), arguing that this adjustment could significantly alter the results. However, MN's Table 2 purports to show that the results become insignificant when applying clustered standard errors. Unfortunately, their analysis does not employ the main model from Barceló; rather, it is based on a modified model that omits a subset of control variables crucial for avoiding omitted variable bias, which we have extensively discussed. Although they aim to assess the impact of clustering standard errors, they instead present a different model specification from the one Barceló reported, thereby failing to provide a valid test of how the clustering of standard errors affects the main findings.

To address this issue and provide a proper test of the impact of different clustering structures on the standard errors in Barceló's main specification, Table 7 presents it using various forms of specifying the standard errors: (1) non-robust standard errors; (2) robust standard errors; (3) Clustered SEs (HC0); (4) Clustered SEs (HC1/STATA); (5) Clustered SEs (HC2); and (6) Clustered SEs (HC3). Once the omission of the models reported by MN is addressed, the results reveal that Barceló's fully-specified model is robust against any clustering choice of the standard errors, whether non-robust, robust, or clustering across its several corrections.[8]

### 5.2 *South Recoding*

Finally, the authors discuss an alleged miscoding of the control variable *South*. MN incorrectly claim that the regression tables in Barceló (2021) were affected by this miscoding, but they do not empirically demonstrate its impact. It is important to note that the change in coding for this variable affects only two provinces–Quang Tri and Thua Thien-Hue–and involves just 7 respondents (1 from Quang Tri and 6 from Thua Thien-Hue), representing only 0.08% of the sample. Table 8 shows that recoding these 7 observations in the control variable *South* produces exactly the same set of coefficients, except for the control variable *South* itself, which does not affect the main findings. The significant discrepancy between MN's portrayal of this as a major issue in Barcelo's analysis and

[8]All models reported in this manuscript, with the obvious exception of Table 7, employ province-level heteroskedastic clustered standard errors.

Table 6: Estimated Impact of Wartime Violence Exposure on Civic Engagement (Individual Items)

with

| | 1<br>Social | 2<br>Religious | 3<br>Cultural | 4<br>Parties | 5<br>Labor | 6<br>Local | 7<br>HR |
|---|---|---|---|---|---|---|---|
| Bombs, per km2 (log) | $0.15^{**}$ | 0.09 | $0.08^{**}$ | $0.13^{**}$ | $0.06^{***}$ | $0.16^{***}$ | $0.01^{**}$ |
| (residence pre-1975) | (0.06) | (0.05) | (0.03) | (0.05) | (0.02) | (0.05) | (0.01) |
| Female | −0.02 | −0.01 | 0.01 | $-0.09^{*}$ | 0.02 | −0.02 | −0.00 |
| | (0.03) | (0.03) | (0.02) | (0.04) | (0.02) | (0.03) | (0.01) |
| Age | $0.00^{**}$ | −0.00 | $-0.00^{**}$ | 0.00 | −0.00 | −0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Education | −0.01 | $-0.02^{*}$ | 0.01 | $0.03^{***}$ | $0.03^{***}$ | −0.01 | 0.00 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.00) |
| Pop Density/1000 | −0.02 | −0.02 | −0.04 | −0.10 | −0.03 | −0.07 | $-0.01^{**}$ |
| | (0.06) | (0.05) | (0.03) | (0.03) | (0.02) | (0.05) | (0.00) |
| Pre Avg/100 | 0.01 | −0.16 | −0.07 | −0.14 | $-0.10^{*}$ | −0.03 | −0.00 |
| | (0.21) | (0.21) | (0.12) | (0.14) | (0.06) | (0.19) | (0.03) |
| South | −0.20 | 0.17 | −0.15 | $-0.38^{***}$ | −0.01 | −0.38 | −0.02 |
| | (0.31) | (0.29) | (0.18) | (0.12) | (0.08) | (0.25) | (0.04) |
| Latitude | −0.01 | 0.02 | −0.01 | −0.02 | 0.01 | −0.02 | −0.00 |
| | (0.03) | (0.03) | (0.02) | (0.02) | (0.01) | (0.03) | (0.00) |
| (Intercept) | −0.00 | −0.21 | 0.37 | 0.54 | −0.11 | 0.52 | −0.01 |
| | (0.55) | (0.37) | (0.30) | (0.26) | (0.16) | (0.40) | (0.08) |
| Observations | 862 | 862 | 862 | 862 | 862 | 862 | 862 |
| # of VFF (MN, 2024) | 5 | 3 | 2 | 1 | 1 | 1 | 0 |

| | 8<br>Conserv. | 9<br>Prof. | 10<br>Youth | 11<br>Sports | 12<br>Women | 13<br>Peace | 14<br>Health |
|---|---|---|---|---|---|---|---|
| Bombs, per km2 (log) | $0.06^{***}$ | 0.01 | $0.03^{*}$ | $0.08^{**}$ | 0.03 | $0.05^{***}$ | 0.03 |
| (residence pre-1975) | (0.01) | (0.03) | (0.02) | (0.03) | (0.03) | (0.01) | (0.03) |
| Female | −0.03 | $-0.04^{**}$ | $-0.06^{***}$ | $-0.13^{**}$ | $0.48^{***}$ | −0.00 | −0.01 |
| | (0.02) | (0.02) | (0.02) | (0.03) | (0.05) | (0.02) | (0.02) |
| Age | −0.00 | −0.00 | $-0.00^{***}$ | −0.00 | $-0.00^{***}$ | −0.00 | $0.00^{*}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Education | 0.00 | 0.01 | 0.01 | $0.02^{**}$ | 0.01 | 0.00 | $0.02^{***}$ |
| | (0.01) | (0.00) | (0.01) | (0.01) | (0.01) | (0.00) | (0.00) |
| Pop Density/1000 | $-0.03^{***}$ | −0.01 | 0.01 | −0.01 | −0.01 | $-0.03^{***}$ | −0.01 |
| | (0.01) | (0.02) | (0.01) | (0.02) | (0.02) | (0.01) | (0.03) |
| Pre Avg/100 | $0.11^{*}$ | −0.07 | 0.01 | −0.11 | −0.05 | −0.03 | −0.01 |
| | (0.06) | (0.10) | (0.08) | (0.11) | (0.09) | (0.04) | (0.10) |
| South | $-0.32^{***}$ | 0.04 | −0.12 | −0.04 | −0.16 | $-0.11^{***}$ | −0.12 |
| | (0.05) | (0.12) | (0.09) | (0.15) | (0.13) | (0.04) | (0.11) |
| Latitude | $-0.02^{***}$ | 0.01 | −0.01 | 0.00 | 0.00 | −0.01 | −0.01 |
| | (0.01) | (0.01) | (0.01) | (0.02) | (0.01) | (0.00) | (0.01) |
| (Intercept) | 0.31 | 0.03 | 0.32 | 0.15 | 0.18 | 0.18 | 0.19 |
| | (0.15) | (0.17) | (0.19) | (0.24) | (0.24) | (0.12) | (0.21) |
| Observations | 862 | 862 | 862 | 862 | 862 | 862 | 862 |
| # of VFF (MN, 2024) | 1 | 10+ | 2 | 0 | 1 | 1 | 1 |

Significance levels: $^{***}$ $p < 0.01$; $^{**}$ $p < 0.05$; $^{*}$ $p < 0.1$. Province-level heteroskedastic clustered standard errors in parentheses. The complete names of the organizations are provided in Table 5. # of VFF reports the number of VFF organizations linked to a specific organizational type in MN (2024).

Table 7: Robustness of the Effect of Exposure to Violence on Civic Engagement (Main Model) Using Clustered Standard Errors

with

| | Non-robust SEs | Robust SEs | Clustered SEs (HC0) | Clustered SEs (HC1/STATA) | Clustered SEs (HC2) | Clustered SEs (HC3) |
|---|---|---|---|---|---|---|
| Bombs, per km2 (log) | $0.23^{***}$ | $0.23^{***}$ | $0.23^{***}$ | $0.23^{***}$ | $0.23^{***}$ | $0.23^{***}$ |
| (residence pre-1975) | (0.03) | (0.03) | (0.06) | (0.06) | (0.07) | (0.08) |
| Female | $-0.07^{*}$ | $-0.07^{*}$ | $-0.07^{**}$ | $-0.07^{**}$ | $-0.07^{*}$ | $-0.07^{*}$ |
| | (0.04) | (0.04) | (0.03) | (0.03) | (0.03) | (0.04) |
| Age | $-0.00$ | $-0.00$ | $-0.00$ | $-0.00$ | $-0.00$ | $-0.00$ |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Education | $0.02^{**}$ | $0.02^{*}$ | $0.02^{*}$ | $0.02^{*}$ | $0.02^{*}$ | 0.02 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Pop Density/1000 | $-0.10^{***}$ | $-0.10^{***}$ | $-0.10^{*}$ | $-0.10^{*}$ | $-0.10$ | $-0.10$ |
| | (0.03) | (0.03) | (0.06) | (0.06) | (0.08) | (0.17) |
| Pre Avg/100 | $-0.14$ | $-0.14$ | $-0.14$ | $-0.14$ | $-0.14$ | $-0.14$ |
| | (0.11) | (0.12) | (0.21) | (0.21) | (0.24) | (0.28) |
| South | $-0.45^{***}$ | $-0.45^{**}$ | $-0.45^{*}$ | $-0.45^{*}$ | $-0.45$ | $-0.45$ |
| | (0.14) | (0.17) | (0.26) | (0.26) | (0.30) | (0.36) |
| Latitude | $-0.02$ | $-0.02$ | $-0.02$ | $-0.02$ | $-0.02$ | $-0.02$ |
| | (0.02) | (0.02) | (0.03) | (0.03) | (0.03) | (0.04) |
| Intercept | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 |
| | (0.27) | (0.30) | (0.48) | (0.49) | (0.56) | (0.67) |
| Observations | 862 | 862 | 862 | 862 | 862 | 862 |
| Model | IV | IV | IV | IV | IV | IV |

Significance levels: $^{***}$ $p < 0.01$; $^{**}$ $p < 0.05$; $^{*}$ $p < 0.1$. Dependent variable is civic engagement log (in 2001).

Table 8: Comparison of the Estimates Effects Using the Two Codings of the Variable *South*

| | South (original) | South (recoded) |
|---|---|---|
| Bombs, per km2 (log) | $0.23^{***}$ | $0.23^{***}$ |
| (residence pre-1975) | (0.07) | (0.06) |
| Female | $-0.07^{**}$ | $-0.07^{**}$ |
| | (0.03) | (0.03) |
| Age | $-0.00$ | $-0.00$ |
| | (0.00) | (0.00) |
| Education | $0.02^{*}$ | $0.02^{*}$ |
| | (0.01) | (0.01) |
| Pop Density/1000 | $-0.11^{*}$ | $-0.10^{*}$ |
| | (0.06) | (0.06) |
| Pre Avg/100 | $-0.22$ | $-0.14$ |
| | (0.21) | (0.21) |
| South | $-0.29$ | |
| | (0.28) | |
| South | | $-0.45^{*}$ |
| (recoded) | | (0.26) |
| Latitude | $-0.00$ | $-0.02$ |
| | (0.03) | (0.03) |
| Intercept | $-0.15$ | 0.04 |
| | (0.55) | (0.49) |
| Observations | 862 | 862 |
| Model | IV | IV |

Significance levels: $^{***}$ $p < 0.01$; $^{**}$ $p < 0.05$; $^{*}$ $p < 0.1$. Dependent variable is civic engagement log (in 2001). Province-level heteroskedastic clustered standard errors in parentheses.

the negligible empirical impact underscores their attempts to misrepresent the original work and mislead readers about the robustness of the results.[9]

## 5.3 *Effect in both North and South Vietnam*

MN argue that the observed effects of wartime exposure on civic engagement, which are significant across Vietnam as a whole, are driven exclusively by effects in North Vietnam and are absent in South Vietnam. They suggest that South Vietnam presents a challenging case because the significant results in the North might be influenced by the stronger historical presence of the communist party, which was more pronounced in the North. While we have already addressed and dismissed concerns that prewar communist party strength might threaten the validity of the main estimates, MN's observation that the effects are not found in the South remains puzzling.

[9] All models reported in this manuscript, with the obvious exception of Table 8, employ the updated control of *South*.

To test this hypothesis, they used split sample analysis, running separate models for the North and South. However, as William et al. (2006) argue, this approach can lead to a loss of efficiency due to the reduced sample sizes in each subgroup, without offering any improvement in interpretability compared to models that include interaction terms. This loss of efficiency is especially problematic in instrumental variable (IV) approaches, where it can negatively impact the F-test in the first-stage regression. Indeed, in this case, the F-test value for the South Vietnam sample is 1.8, which falls well below the conventional threshold of 10 for a reliable IV estimation.

Therefore, the conclusion that wartime exposure has no effect on civic engagement in the South might be misleading–not because there is no effect, but because the reduced efficiency in the split-sample analysis prevents a test of this hypothesis in the South using this approach.

The most robust method to assess the heterogeneous effects hypothesized by MN is to employ a multiplicative interaction model that combines the samples while distinguishing the effects in the North and South. This approach retains the efficiency of the analysis and provides direct estimates of the regional differences in the impact of wartime exposure on civic engagement. Figure 6 displays the estimated treatment effects of exposure to wartime violence for individuals who lived in South Vietnam and North Vietnam before the war. These findings significantly challenge the results provided by MN as the interaction model reveals that the effect of wartime exposure on civic engagement is positively significant in both regions. More specifically, the average marginal effect (AME) of wartime exposure on civic engagement is $0.23$ for North Vietnam ($p < 0.01$) and $0.09$ for South Vietnam ($p < 0.05$). Thus, MN's assertion that "in South Vietnam, there is no observed relationship between bombing and the measure of civic engagement" is, once again, an unfortunate misrepresentation of Barceló's original findings.

### 5.4 Controlling for Ethnicity Fixed Effects

Finally, MN also control for ethnicity fixed effects. Table 9 presents the regression results when including fixed effects for various ethnic groups. The estimated treatment effect of exposure to wartime violence remains positive and statistically significant at the 99% confidence level, with a coefficient of 0.23, indicating that the original findings are robust also after accounting for ethnicity.

## 6 Conclusion

This manuscript addresses the critiques raised by MN (2024) regarding the findings in Barceló (2021) on the long-term effects of wartime violence on civic engagement in Vietnam. I highlight the importance of tackling complex modeling challenges to ensure that causal pathways are properly accounted for. Specifically, omitting key pre-treatment covariates, such as prewar residence in North or South Vietnam, which were critical in the original specifications used in Miguel & Ronald (2011) and Barceló (2021), leads to misleading conclusions. After adequately adjusting the model for these confounders, this manuscript confirms that people who lived in provinces significantly impacted by the conflict during the war were more actively involved in organizations, even 26 years after the war. Furthermore, this study illustrates the broader methodological lesson that even well-intentioned critiques must ensure rigorous model specification in contexts where the risk of confounding bias is significant.

Figure 6: Estimated Effects of Exposure to Wartime Violence on Civic Engagement Separately by Region

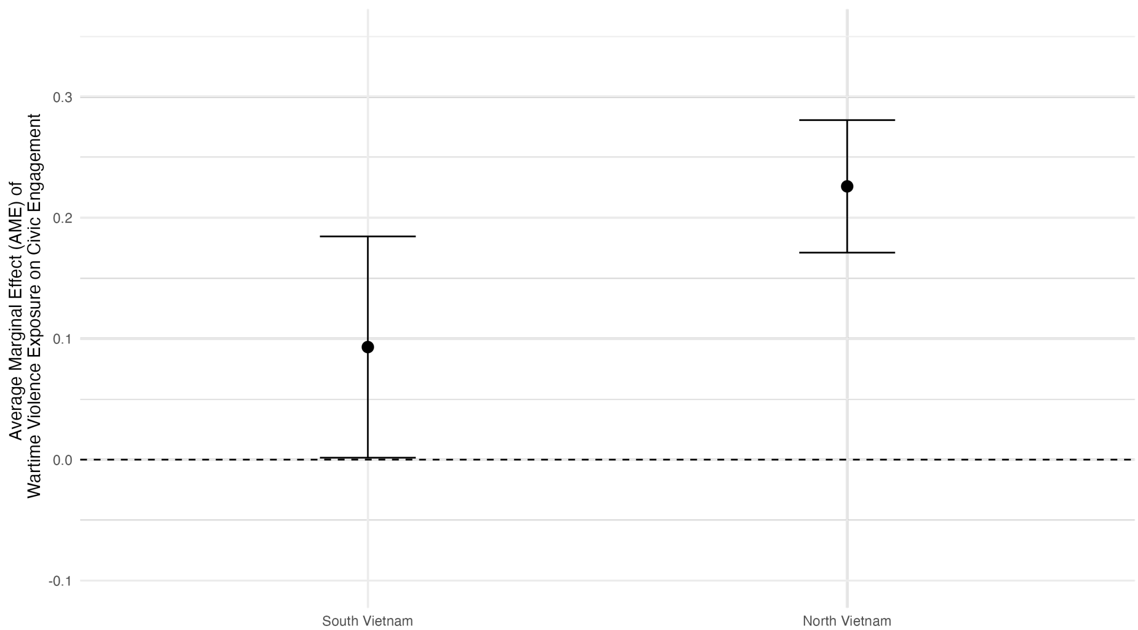


Table 9: Estimated Effects After Accounting for Ethnicity Fixed Effects

| | With Ethnic Group Fixed Effects |
|---|---|
| Bombs, per km2 (log) | 0.23*** |
| (residence pre-1975) | (0.06) |
| Female | −0.06* |
| | (0.03) |
| Age | −0.00 |
| | (0.00) |
| Education | 0.02* |
| | (0.01) |
| Pop Density/1000 | −0.10* |
| | (0.06) |
| Pre Avg/100 | −0.14 |
| | (0.21) |
| South | −0.45* |
| | (0.26) |
| Latitude | −0.02 |
| | (0.03) |
| Ethnic group = Kinh | −0.07 |
| | (0.18) |
| Ethnic group = Muong | −0.24 |
| | (0.24) |
| Ethnic group = Mong | 0.10 |
| | (0.26) |
| Ethnic group = Dao | −0.30 |
| | (0.19) |
| Ethnic group = Ede | 0.09 |
| | (0.23) |
| Ethnic group = Ray | 0.23 |
| | (0.33) |
| Intercept | 0.11 |
| | (0.46) |
| First Stage | |
| Distance to 17th Parallel | −0.58 |
| | (0.10) |
| F-statistic | 37.1 |
| Observations | 856 |
| Model | *IV* |

Province-level heteroskedastic clustered standard errors are reported in parentheses.

Additionally, this interaction with MN provides important lessons about replication practices. One key lesson is the critical role of transparency in ensuring high standards. As demonstrated here, the change in Barceló's specification–omitting key pre-treatment variables from the models–significantly alters the results. However, once these covariates are included, all models remain robust to MN's suggested modifications, which MN's reanalysis fails to mention. Therefore, it is essential for future replications that researchers clearly communicate what changes in the original specification bring about specific consequences for the results, ensuring that readers do not obtain misleading perceptions about the robustness of the findings.

Finally, another important consideration is that those who work on replicating other scholars' work, while essential for scientific advancement, must be meticulous in distinguishing between actual errors and principled decisions in the original research. Misinterpreting a deliberate methodological choice as a mistake can lead to misguided critiques and potentially undermine the credibility of valid findings. For instance, what may appear to be an "error" in a model specification could be a well-justified decision based on theoretical reasoning or empirical evidence. In the case of Barceló's and Miguel and Roland's work, the inclusion of pre-treatment covariates such as prewar residence in North or South Vietnam was a deliberate and theoretically grounded choice to address confounding bias. Mislabeling such decisions as errors not only misrepresents the original study but also misleads subsequent readers and researchers. It is crucial for replicators to thoroughly understand the rationale behind methodological choices and to communicate any modifications transparently, ensuring that the integrity and reliability of scientific research are maintained.

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