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Working Paper

Replication of "How Threats of Exclusion Mobilize Palestinian Political Participation"

I4R Discussion Paper Series, No. 261

Provided in Cooperation with:

The Institute for Replication (I4R)

Suggested Citation: Bochkareva, Yana; Silagadze, Givi; Stephan, Meret (2025) : Replication of "How Threats of Exclusion Mobilize Palestinian Political Participation", I4R Discussion Paper Series, No. 261, Institute for Replication (I4R), s.l.

This Version is available at:

<https://hdl.handle.net/10419/326980>

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No. 261

DISCUSSION PAPER SERIES

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This paper received a response:

Weiss, C.M., A. Siegel, and D. Romney. 2025. How Threats of Exclusion Mobilize Palestinian Political Participation – A Response to Bochkareva, Silagadze and Stephan. *I4R Discussion Paper Series* No. 262. Institute for Replication

September 2025

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SEPTEMBER 2025

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Replication of “How Threats of Exclusion Mobilize Palestinian Political Participation”¹

Yana Bochkareva², Givi Silagadze³, Meret Stephan⁴

July 16, 2025

Abstract

Weiss et al. (2023) study the effect of Trump’s peace plan announcement on Palestinian political participation, arguing that a threat of exclusion mobilizes minorities. Their study includes: (1) social media analysis showing differential treatment effects, (2) a difference-in-differences (DiD) analysis of voter turnout, and (3) a DiD analysis of social movement registrations. First, we reproduce their analysis in R and find no substantial errors. Second, we check the parallel trends assumption more rigorously and find that it does not unequivocally hold. Third, we conduct a series of robustness and sensitivity checks, ultimately demonstrating that different definitions of the treatment group and an outcome variable alter the results significantly. Although the results generally support the authors’ main argument, our replication raises questions about expanding their grievance mechanism. Our findings indicate that ambiguous and weak threats are more likely to result in institutionalized political participation, while explicit and strong threats tend to mobilize non-institutionalized political participation.

¹This paper originates from the PhD course *Replicating Research* at the European University Institute (EUI), led by Prof. Simon Hix. We are grateful to Prof. Hix for his encouragement and instructive comments on earlier drafts, and to the course participants for their helpful suggestions. The author list is alphabetically ordered. All authors have contributed equally.

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1 Introduction

Weiss et al. (2023) examine the question of whether the threat of exclusionary policies can lead to increased political mobilization. They argue that actual exclusion is not necessary to create or resurface grievances among the threatened population, but merely the possibility of exclusion is sufficient. These grievances will then translate into increased political mobilization. Empirically, Weiss et al. (2023) examine the effect of Donald Trump’s announcement of a peace plan in January 2020 on minority mobilization in Israel. The plan stated a possibility of land swaps, effectively endangering the legal status of Palestinian Citizens of Israel (PCI) residing in the ‘Triangle area’. This area comprised 16 localities adjacent to the West Bank, ten of which were explicitly mentioned in the plan.

The analysis consists of three steps. First, Weiss et al. (2023) analyze over 170,000 Facebook posts, showing that the announcement increased attention to Trump and his plan among the mentioned localities. Second, using a difference-in-differences (DiD) design, they leverage the impact of the announcement on voter turnout. Among the ten explicitly mentioned localities, the effect of the announcement does not reach conventional levels of significance compared to non-Jewish/mixed areas, unless expanding the control group or considering all Triangle areas. Lastly, again using DiD, they examine whether the announcement mobilized Triangle residents to join a social movement aiming to unify Israelis and PCIs. Their findings show that the announcement increased sign-ups for the email list of the social movement in the Triangle areas across all models.

In the following, we first focus on the computational reproducibility of their results. In a second step, we conduct robustness replications of Weiss et al.’s (2023) models on voter turnout to find out how strong their parallel trends hold and whether the results are driven by specific localities. In the third step, we follow the same logic for their results on social movement participation and test alternative specifications of the outcome.

While we managed to reproduce Weiss et al.’s (2023) code with only minor differences, our analysis suggests that their identification assumptions are partial, the effect is driven by particular localities, and the results are sensitive to alternative definitions of the outcome variable. Echoing research on social movements (e.g., Borbáth and Hutter 2024), which suggests that grievances alone do not always lead to mobilization, we argue that the theoretical argument might be more nuanced than the original paper’s grievance-based mechanism. Our findings suggest that the threat of exclusion does not affect all Triangle localities the same, and the type

of political mobilization depends on whether the threat of exclusion is ambiguous and weak or explicit and strong. When minorities are faced with a weak and ambiguous threat, they may choose to mobilize more strategically, channeling their grievances through traditional political institutions such as voting. Conversely, when the threat is strong and explicit, minorities may turn to mobilization outside of the political system, engaging in social movement activism because they do not feel represented by established political channels.

2 Computational Reproducibility

Replication materials are provided by the authors in full and are available here on the American Journal of Political Science Dataverse within the Harvard Dataverse Network. As Facebook data is not directly accessible, the authors provide an anonymized version. Using the provided materials, we successfully reproduced all the main results. Table 1 summarizes the data availability and the results of our reproduction.

Table 1: Computational reproduction results

	Fully	Partial	No
Raw data provided			X
Cleaning code provided			X
Analysis data provided	X		
Analysis code provided	X		
Reproducible from raw data			X
Reproducible from analysis data		X	

The only exception is their Table A16 in the supplementary material. This table shows the ten Triangle localities, which were part of Trump’s announcement, and ten non-Triangle localities that match each Triangle one the best regarding population characteristics and voting patterns. Between these matches, they then compare posts from public Facebook groups and show, descriptively, that the weekly relative salience of Trump and his announcement is higher among the Triangle localities than in the non-Triangle counterparts. Table 2) shows the authors’ matches, Table 3 ours. Finding different matches than Weiss et al. (2023) means that our table suggests comparing the salience of the announcement across a different set of localities than theirs. However, due to limited access to the full Facebook data, we could not test the robustness of this matching.

Table 2: Triangle and Matched Non-Triangle Localities (Original A16)

	Triangle	Non-Triangle (Match)
1	JALJULYE	KAFAR YASIF
2	KAFAR BARA	MAS'ADE
3	KAFAR QASEM	MUGHAR
4	AR'ARA	ARRABE
5	QALANSAWE	KAFAR KANNA
6	KAFAR QARA	REINE
7	UMM AL-FAHM	SHEFAR'AM
8	TIRE	HURA
9	TAYIBE	TAMRA
10	BAQA AL-GHARBIYYE	SAKHNIN

Table 3: Triangle and Matched Non-Triangle Localities (BSS)

	Triangle	Non-Triangle (Match)
1	JALJULYE	BI'NE
2	KAFAR BARA	EILABUN
3	KAFAR QASEM	KAFAR MANDA
4	AR'ARA	KUSEIFE
5	QALANSAWE	JUDEIDE-MAKER
6	KAFAR QARA	YAFI
7	UMM AL-FAHM	SHEFAR'AM
8	TIRE	ARRABE
9	TAYIBE	TAMRA
10	BAQA AL-GHARBIYYE	KAFAR KANNA

3 Robustness Replication

Since we do not have access to the original Facebook data, we were unable to replicate the matching process. Understanding that the Facebook analyses in the original paper are primarily descriptive, we decided to concentrate on the two main studies from the paper in our robustness replication: voter turnout and social movement participation.

3.1 Voter Turnout

The authors use OLS regressions to estimate treatment effects on voter turnout. Their Table 1 (p. 1089 in the original paper) presents one OLS model with population controls (Model 1) and three two-way fixed effects (2WFE) models with varying treatment/control groups (Models

2–4). Three of those models yield inconclusive results. Models 1 and 2 are not statistically significant. Model 4 is significant but relies on the full sample as a control group, violating the parallel trends assumption (as acknowledged in Figure A5).

3.1.1 Parallel Trends

The main claim that the threat of exclusion had an impact on electoral mobilization is, thus, largely based on Model 3. This model employs locality and election cycle fixed effects and considers the 16 localities as treated and all other non-Jewish localities as controls. However, the authors do not test the parallel trends assumption for this specification. In our Figure 1, we address this gap by plotting average turnout over time for the treated and control groups. Pre-treatment trends appear somewhat parallel but not perfectly so: the turnout gap narrows slightly after 2015 but widens again after 2019. This prompted us to test further the assumption that the treated (extended Triangle localities) and control (other non-Jewish localities) groups would have followed similar trends in the absence of treatment.

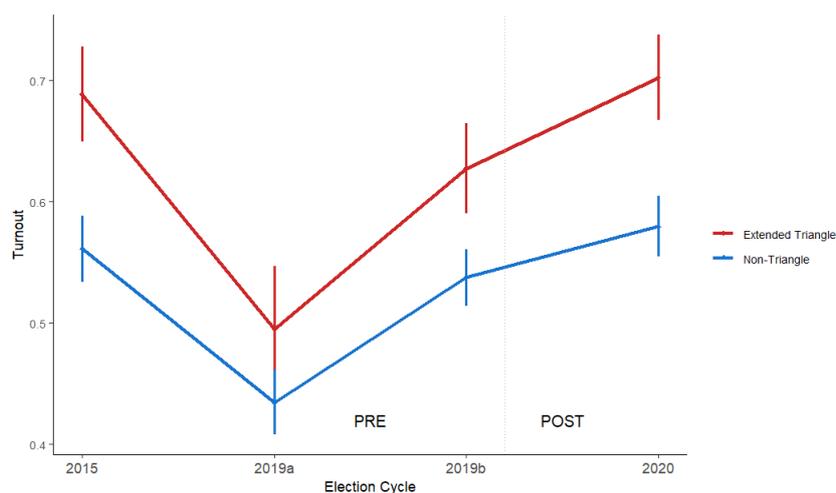


Figure 1: Average Voter Turnout Over Time

To test the parallel trends assumption more formally, we conducted an event study. If pre-treatment estimates for the (extended) Triangle localities are not significantly different from zero, parallel trends can be assumed. Figure 2 shows this is not the case: while the 2015 estimate is statistically significant, the 2019 (first cycle) estimate is marginally non-significant. These results suggest potential threats to validity, since non-parallel trends would be a fundamental problem for the identification strategy.

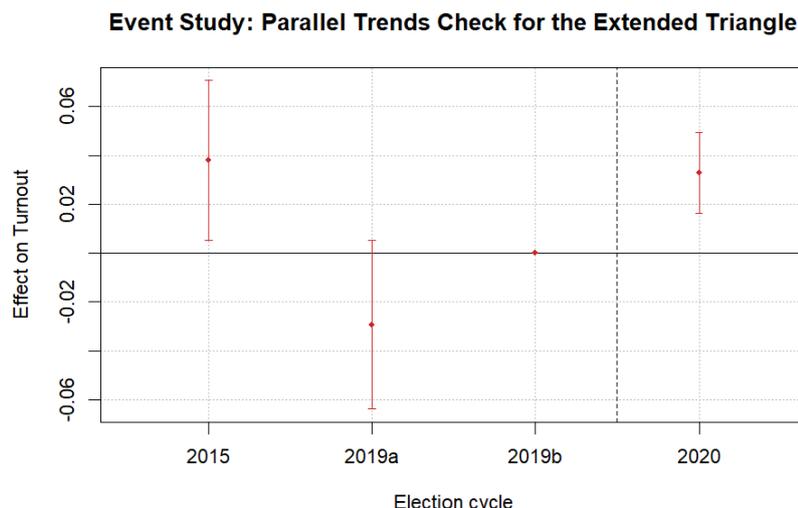


Figure 2: Leads and Lags Specification

3.1.2 Regressions Turnout

Weiss et al. (2023) find no significant effect of Trump’s announcement on voter turnout when focusing only on the ten localities named in the plan. However, when they include the six additional Triangle localities, their results become significant and positive. We reran the original models using only these six areas as the treatment group. Table 4 replicates Models 1, 2, and 3 from the original paper (our Models 1, 3, and 4, respectively), and adds three models (Models 2, 5, and 6). In Model 2, we rerun their OLS model with population controls but with all 16 Triangles as treated and find significant positive effects. This finding aligns with the authors’ model that uses fixed effects instead of population controls (Table 4, Model 4 in our replication and Table 1, Model 3 in the original paper). This is reasonable, considering that the authors use population control variables from before the treatment, which are likely to be quite similar to using fixed effects.

Table 4: Deal of the Century Effect on Turnout

	(1)	(2)	(3)	(4)	(5)	(6)
Triangle	0.068*** (0.028)	0.043* (0.024)				
Post	0.097*** (0.006)	0.093*** (0.006)				
Triangle * Post	0.024 (0.016)	0.048*** (0.014)	0.024 (0.016)	0.048*** (0.013)	0.080*** (0.014)	0.082*** (0.014)
Pop Control	Yes	Yes	No	No	No	No
Cycle FE	No	No	Yes	Yes	Yes	Yes
Locality FE	No	No	Yes	Yes	Yes	Yes
Cluster	Locality	Locality	Locality	Locality	Locality	Locality
Sample	Non-Jew.	Non-Jew.	Non-Jew.	Non-Jew.	Non-Jew.	No 10
Treatment	10 Loc.	16 Loc.	10 Loc.	16 Loc.	6 Loc.	6 Loc.
Pre-Register	Yes	Yes	No	No	No	No
<i>N</i>	405	405	465	465	465	435

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Models 1, 3, and 4 replicate Weiss et al. (2023); Models 2, 5, and 6 are our additional models.

Weiss et al. (2023) report only the coefficient for the interaction *Triangle x Post*, as the fixed effects make the individual effects of *Triangle* and *Post* perfectly collinear with those fixed ones. As a result, the main effects are omitted from the model when fixed effects are included. We do the same in our additional models.

Notably, when considering only the six non-mentioned Triangle localities as treated, we still find significant positive effects (see Table 4, Models 5 and 6). The effect is nearly twice as large as when all 16 are treated, suggesting these six drive the results. In Model 6, excluding the 10 named localities entirely (they are part of the control group in Model 5) increases the effect further. Only these six localities are likely impacted by the announcement. Theoretically, this implies that minorities that are explicitly threatened might mobilize differently as opposed to minorities that are threatened in a more ambiguous way.

3.1.3 Regressions Vote Shares

We also analyzed vote shares, as the authors do in their appendix. Our results show that the positive effect on Blue-White's vote share is not driven solely by the six unmentioned Triangle localities, as opposed to the findings on the turnout. Table 5, which corresponds to Table A8 in their appendix, shows that, regardless of the specific combination, the plan had a positive and

significant impact on vote shares for the Blue-White party. As also Weiss et al. (2023) state in their appendix, these findings mean that, even though the Blue-White vote share decreased in both treated and control localities, the decrease was less steep in the treated localities.

Table 5: Deal of the Century Effect on Blue-White Vote Share

	Blue-White			
	(1)	(2)	(3)	(4)
Triangle				
Post				
Triangle * Post	0.025*** (0.006)	0.024*** (0.007)	0.019*** (0.007)	0.020*** (0.007)
Cycle FE	Yes	Yes	Yes	Yes
Locality FE	Yes	Yes	Yes	Yes
Cluster	Locality	Locality	Locality	Locality
Sample	Non-Jew.	Non-Jew.	Non-Jew.	No 10
Treatment	10 Loc.	16 Loc.	6 Loc.	6 Loc.
Pre-Register	No	No	No	No
<i>N</i>	465	465	465	435

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Model 1 replicates Weiss et al. (2023); Model 2, 3, and 4 are our additional models.

Again, Weiss et al. (2023) report only the coefficient for the interaction *Triangle x Post*, as the fixed effects make the individual effects of *Triangle* and *Post* perfectly collinear with those fixed ones. As a result, the main effects are omitted from the model when fixed effects are included. We do the same in our additional models.

These findings are not exclusively driven by either the ten or the six localities. Neither does it matter substantially or significantly whether the ten mentioned localities are part of the control group or are excluded from the analyses when looking at the effect of the six not-mentioned ones. We find no significant effects for other parties, consistent with the authors' results. As the authors noted in their Appendix, one explanation for the Blue-White effect, but not for the Joint Arab List, which opposed the plan more clearly, is that Blue-White was the main competitor against the incumbent. Another explanation—that aligns with theirs—could be strategic voting: the threat of exclusion may not only mobilize voters but also lead them to support the party most likely to defeat the incumbent, even if it does not fully represent their grievances.

3.2 Activism Mobilization

The second analysis is concerned with the participation of PCI in the Jewish-Arab social movement, operationalized through the number of people from a specific locality joining the organisation’s listserv (mailing system).

3.2.1 Parallel Trends

As with the first DiD analysis, we test the parallel trends assumption using an event study. The authors show monthly registration counts in their Figure 5 and claim an immediate post-treatment increase. While their figure uses monthly counts, their analysis uses a binary locality-day outcome measuring whether registration occurred at the locality-day level. We ran an event study using both daily and monthly data. Figure 3 presents a leads-and-lags model over ± 60 days around the announcement. It suggests: (1) parallel trends are held imperfectly—six pre-treatment days show significant differences from zero, and (2) contrary to the authors’ claim, the effect appears only about 40 days after the announcement, around the March 2020 elections.

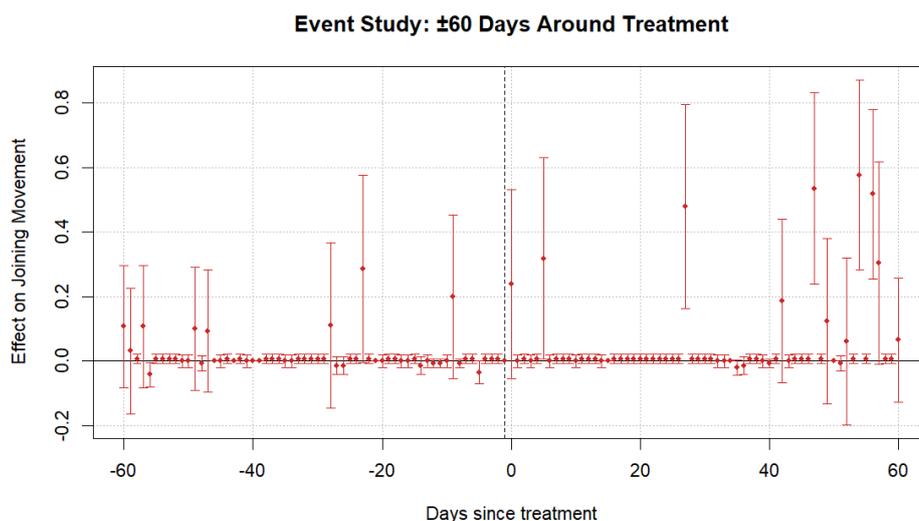


Figure 3: Leads and Lags Specification - Daily Registrations

Figure 4, using monthly registrations as the outcome, tells a similar story: parallel trends do not hold perfectly, and the effect only becomes visible in March 2020. While this challenges the authors’ claim of an immediate response, it still supports the broader conclusion that the announcement had a differential impact on political mobilization during the election period.

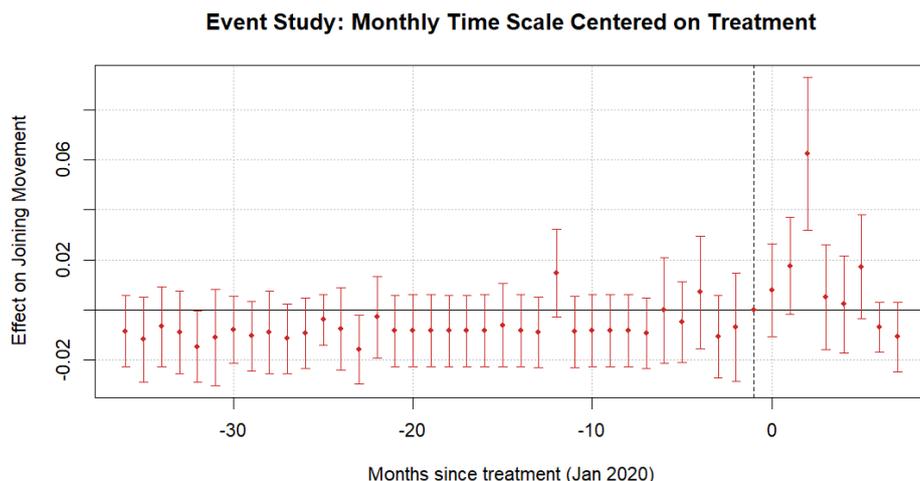


Figure 4: Leads and Lags Specification - Monthly Registrations

3.2.2 Regressions

The effects on mobilization contrast with the ones on turnout. Our findings indicate that the positive and significant effects on political activism are solely driven by the ten mentioned localities. In Models 2-4 of their original Table 2, the authors use week, month, and year FEs simultaneously. To assess the potential issue of overcontrolling, we reran their models with simplified combinations of fixed effects (e.g., using only month and locality fixed effects). The results remain largely the same.

Our Models 1 and 2 (replicating the authors' Models 2 and 3) in Table 6 confirm their findings: the ten localities show significant increases in activism, though the effect is smaller when including the six not-mentioned ones. In contrast, when focusing only on the six not-mentioned localities (Model 3) or excluding the ten mentioned entirely (Model 4), the effects are negative and not significant. This suggests the plan had opposite effects on different forms of mobilization: the six not-mentioned localities were more likely to turnout to vote (institutionalized), while the ten mentioned localities were more likely to engage in activism (non-institutionalized). Seemingly, whether or not a threat of exclusion is explicit impacts strategic calculations of minorities in terms of expressing their grievances in institutional channels as opposed to non-institutionalized channels.

Table 6: Deal of the Century Effect on Mobilization

	Mobilization			
	(1)	(2)	(3)	(4)
Triangle				
Post	0.017*** (0.002)	0.017*** (0.002)	0.018*** (0.002)	0.017*** (0.0004)
Triangle * Post	0.022*** (0.004)	0.013*** (0.004)	-0.003 (0.004)	-0.002 (0.004)
Population Control	No	No	No	No
Week, Month, Year FEs	Yes	Yes	Yes	Yes
Locality FE	Yes	Yes	Yes	Yes
Sample	Non-Jewish	Non-Jewish	Non-Jewish	No 10
Treatment	10 Loc.	16 Loc.	6 Loc.	6 Loc.
<i>N</i>	203,980	203,980	203,980	190,820

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Models 1 and 2 replicate Weiss et al. (2023); Models 3 and 4 are our additional models.

Weiss et al. (2023) drop the coefficients for *Triangle* as the fixed effects make them perfectly collinear with the locality fixed effects. Since the *Week*, *Month*, *Year* fixed effects are not the same as a single unified *Post* indicator, they remain in the models. We do the same in our additional models.

3.2.3 Sensitivity to Time Aggregation

The original Figure 5 presents the rates of registrations to the social movement's listserv. This outcome variable is aggregated by month for visualisation, whereas OLS models use days. Thus, we reconstructed the figure using a more granular operationalization of weekly counts rather than monthly aggregates to assess the stability of an outcome.

Our recoded figure suggests more instability within the outcome variable, both before and after Trump's announcement, and the treatment effect is measured in relation to this unstable baseline. One such instance is that non-Triangle localities exhibit a resurgence in registrations shortly after February, closely following trends in Triangle areas. However, the trend in subscriptions from the latter is indeed very stable throughout periods: most days, no new registrations happen. The post-announcement effect is constituted by 240 counts for the period of 8 months.

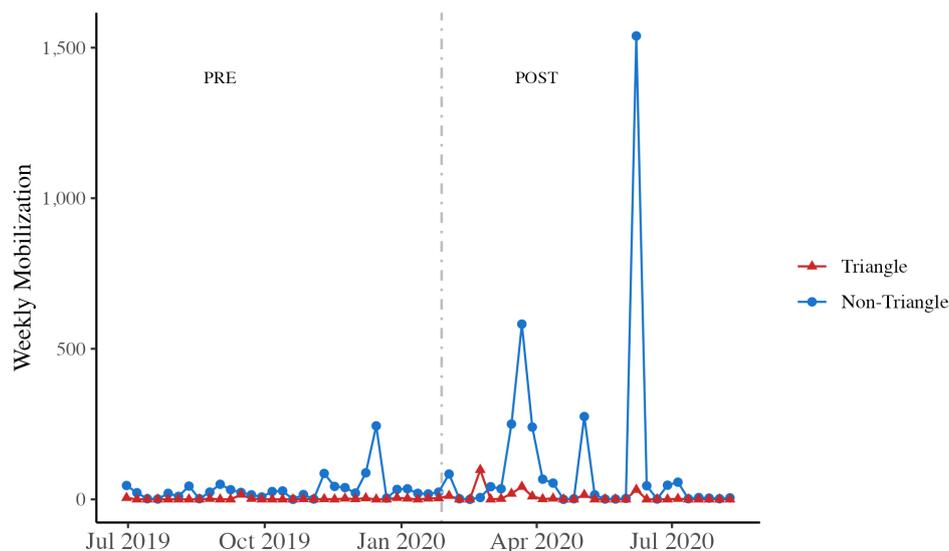


Figure 5: Listserv Registrations Over Weeks in Triangle and Non-Triangle Localities

3.2.4 Robustness to Alternative Outcome Definitions

The original estimation strategy defines the outcome variable as a binary indicator that is equal to 1 if at least one resident from the Triangle locality registered for the social movement’s listserv on a particular day, and 0 otherwise. Original robustness checks presented in the paper cover different model specifications. For example, the effect is consistent when estimated using negative binomial regression, which allows for the use of continuous variables with skewed distributions.

To test the sensitivity of the effect to alternative outcome definitions, we recoded it to be more conservative. We adjust this indicator by treating a day as 1 only if three or more individuals registered⁵, proposing a higher threshold for substantive mobilization activity. When using this new definition, the effect does not meet any conventional levels of significance in three out of four models presented in Table 7, the exception being the last model that uses the full sample. Thus, the effect of the announcement of Trump’s plan on the mobilisation of PCI is smaller and is sensitive to alternative operationalizations.

Under this more conservative threshold, the estimated treatment effect decreases in magnitude, though the direction remains positive. The primary difference across most models using this recoded outcome variable is that the results are not reaching conventional significance levels. The reduced magnitude and precision of the effect suggest that while Trump’s plan has

⁵The results hold when the dependent variable is coded as 1 if two or more individuals registered.

Table 7: The Effect of Trump’s Plan When Mobilization is ≥ 3 People per Day

	Mobilization ≥ 3 joins			
	(1)	(2)	(3)	(4)
Triangle	-0.0001 (0.001)			
Post	0.003 (0.001)	0.008*** (0.001)	0.008*** (0.001)	0.002*** (0.0002)
Triangle * Post	0.004 (0.003)	0.004 (0.003)	0.002 (0.002)	0.006* (0.003)
Population Control	Yes	No	No	No
Week, Month, Year FEs	No	Yes	Yes	Yes
Locality FE	No	Yes	Yes	Yes
Sample	Non-Jewish	Non-Jewish	Non-Jewish	Full
Treatment	10 Localities	10 Localities	16 Localities	10 Localities
<i>N</i>	177,660	203,980	203,980	1,597,624

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models are clustered at a locality level

influenced P CI mobilization, its impact may be more modest. However, the persistence of a positive sign despite not meeting conventional thresholds of significance means that the general direction of the effect is consistent, even under stricter outcome conditions.

4 Conclusion

In this replication report, we assess the computational reproducibility and robustness of Weiss et al. (2023). The data and analysis code are provided by the authors, and the analyses reproduce computationally without issues, except for the matching results in the first part of the analysis. Although all analyses point in the same direction as the authors’ main argument, robustness checks and sensitivity analyses suggest that the parallel trends assumptions in both sets of DiD estimations are questionable, that different localities drive the main effect in each of the two main analyses, and that social mobilization findings are sensitive to alternative definitions of the outcome variable. As a result, we argue that there is some evidence that the threat of exclusion affects minority mobilization, but the empirical results suggest there are more nuances to the theoretical mechanism. Namely, the mechanisms might differ for institutionalized and non-institutionalized political participation. Explicit and stronger threats are associated

with non-institutionalized political participation, such as activism. More ambiguous and weaker threats are linked to institutionalized participation, such as voting instead of activism. Taken together, this suggests threat of exclusion not only affects minority political mobilization through emotions but also affects their strategic calculations on whether to channel their grievances within the political system or not.

5 Funding

The research has received financial support from the French Ministry of Higher Education and Research.

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