SUPPLEMENTARY MATERIAL

NEVER AGAIN
The Holocaust and Political Legacies of Genocide

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Supplementary Material

A1 Sampling

We recruited Holocaust survivors through an email circulated by the United States Holocaust Memorial Museum or a regional Holocaust Museum,[1] and recruited descendants either through the survey firm PrimePanels or various "Children of Holocaust survivors" listservs. We recruited non-descendant Jews and non-Jews through PrimePanels. Table A1.2 reports the number of U.S.-based respondents sampled from each exposure group.

Table A1.2: Sample sizes by recruitment method.

Exposure	How recruited?	N
Survivors	listserv	200
Descendants	PrimePanel	202
Descendants	listserv	115
Non-descendants	PrimePanel	710
Non-descendants	listserv	8
Non-Jews	PrimePanel	517

A1.1 Selection Bias

Because ours is not a probability-based, nationally-representative sample — which is nearly impossible to achieve, given the advanced age of remaining survivors — our sampling procedures have several implications for generalizability and selection bias.

First, people who survived the Holocaust may be systematically different from those who perished. As Finkel (2017, p. 5) notes, "even if under impossible constraints, each and every Jewish person had to decide how to react to Nazi persecution." Survival strategies varied among survivors systematically, depending on geography, pre-war integration, and pre-war economic and education backgrounds. To the extent that some

1. We contacted all 17 regional Holocaust museums in the U.S. Of these, six – in Florida, Ohio, Illinois, Pennsylvania, Michigan, and New York – circulated our survey to their survivor and descendant listservs. Recruitment began in July 2017.

survival strategies had better success rates than others, some Jews became systematically more likely to survive than others. The semi-random nature of survival makes it impossible to rule out survivorship bias, in that those who survived may have developed different long-term attitudes than the dead might have adopted, had they survived. Of course, this is a challenge for *any* study in this area.

A second source of selection bias stems from the advanced age of Holocaust survivors who, by definition, were born in or before 1945. We cannot rule out the possibility that survivors who died young held systematically different attitudes from the survivors in our sample, or that survivors' attitudes have changed over their lifespan. Thus, our results can only point to a snapshot in time: how survivors' experience in the Holocaust have shaped their attitudes *in old age*. We see this not as a limitation of the study, but as a feature. It is precisely these truly long-term attitudes in which we are interested.

Third, survivors in the United States are different than those elsewhere. For example, it is possible that, after 1945, survivors who were more wary of political violence came to the U.S., whereas those who were more risk-acceptant immigrated to Israel. This is an important scope condition of our study – we restrict our conclusions regarding long-term effects of the Holocaust to individuals who immigrated to relatively safe countries.

Fourth, survivors who joined the USHMM listserv and have email addresses on file may be different than survivors who did not. As noted in the main text, the biggest concern here is that survivors on the listserv are politically more liberal than those who opted out. We find no evidence of a partisan skew in our sample: the partisan distribution of our survivor sample roughly matches that of other Jews in the U.S.

Although we cannot rule out the possibility that survivors with a different demographic makeup may hold different attitudes from those in our sample, the survivors who are in our sample did not significantly differ from other Jewish survey participants on socio-demographic metrics other than age. The average survivor in our sample was middle class (mean of 2.45 on a 5-point scale, compared to 2.39 for the average non-descendant), well-educated (mean of 5.34 on a 7-point scale, compared to 5.78) and about equally likely to be Republican as the non-descendant Jew (32% vs. 35%). That said, as Figure A1.2 confirms, the age distribution of survivors is quite distinct from that from other sub-samples, with common support limited to respondents in their 70's.

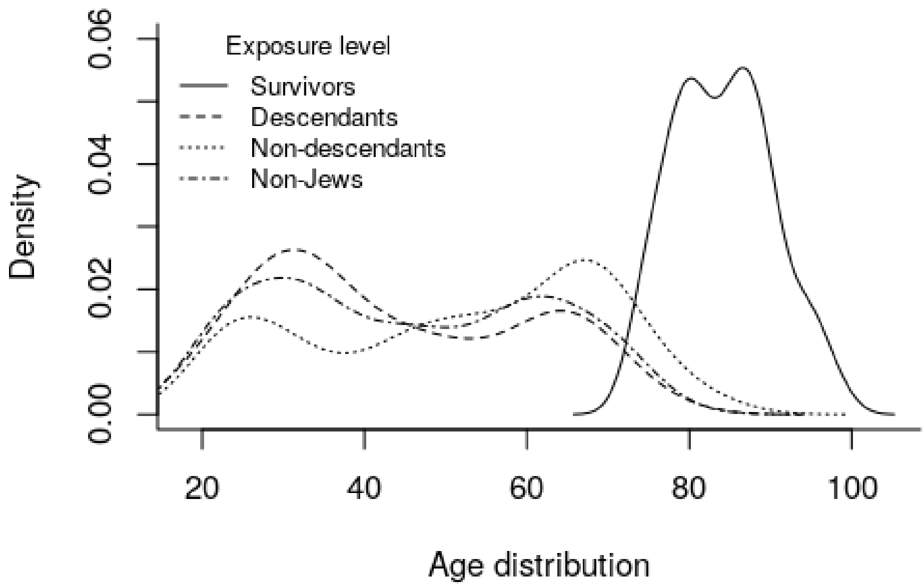


Figure A1.2: Age distribution of survey respondents.

Taken together, these sampling considerations lead to the following scope conditions for our results. Our findings apply to survivors of political violence who 1) suffered victimization as children, 2) emigrated to the United States following the violence, 3) live in relative comfort and security in the present, and 4) were sufficiently healthy to reach advanced age. While the intrinsic importance of this hard-to-reach population does not negate these limitations, we believe that the unique nature of this subject pool and the unprecedented size of our sample are sufficiently compelling to justify our analysis.

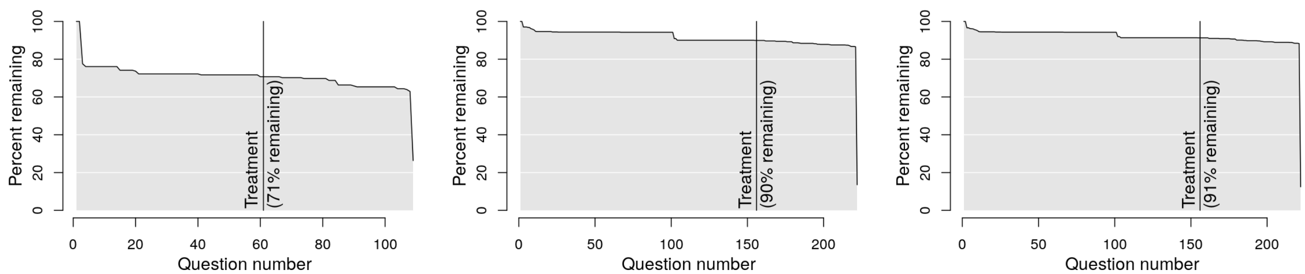
A1.2 Demand Effects

An additional concern with our recruitment method is that, because survivors and descendants recognize they were recruited *because of* their survivor or descendant status, there may be demand effects, where they feel obligated to respond to questions in a more inclusive way. If that were true, we would expect that, when presented with an explicitly outgroup-protective "never again" frame, survivors and descendants should express significantly more support for Syrian refugees. However, we find that survivors' and descendants' views were not significantly affected by either frame, whereas the less exposed populations (Jews and non-Jews) were.

A1.3 Sample Attrition

Finally, it is possible that survivors who chose to fill out the entire survey may be different from those who quit part-way. Figure A1.3 reports the proportion of respondents that reached each survey question, including the proportion remaining at the time of experimental treatment. Because the survey flow differed slightly across subgroups to accommodate specialized questions about personal and family background, we report these patterns separately for each sample. Survivors had the highest attrition, with 71% of respondents reaching treatment, compared to 90-91% for other groups.

A potential concern is that survivors' high attrition rate may reflect discomfort with the "never again" prime and its implicit comparison between the Holocaust and the plight of Syrian refugees. If this is true, then the observed effect of the outgroup-protective prime may be due to less "empathetic" survivors leaving the survey before measurements were made. There is little evidence of such a pattern. Most attrition among survivors occurred at the very beginning of the survey, following the informed consent form and screener questions, long before the treatment was administered. There is no evidence that survivors (or any other group) left the survey en masse shortly after treatment.



(a) Survivors (b) Desc. and non-desc. Jews (c) Non-Jews

Figure A1.3: Patterns of sample attrition among survey respondents.

We can use a simple calculation to assess how severe bias due to attrition would need to be to account for attitudinal differences between survivors and other respondents. Suppose that "true" group means in each exposure category are weighted sums of the attitudes of respondents in sample and those who dropped out:

$$\mathbb{E}[Y|E=k] = \mathbb{E}[Y|E=k, s=1]\pi_k + \mathbb{E}[Y|E=k, s=0](1-\pi_k)$$

where s is an indicator equal to 1 for individuals in sample, E represents one's exposure category, and π_k is the proportion of respondents with exposure $k \in \{$Survivor, Descendant, Non-descendant, Non-Jew$\}$ who completed the survey. $\mathbb{E}[Y|E=k, s=1]$ represents the observed in-sample group means in Figure 1; $\mathbb{E}[Y|E=k, s=0]$ is unobserved. If there is no bias due to attrition, then $\mathbb{E}[Y|E] = \mathbb{E}[Y|E, s=1] = \mathbb{E}[Y|E, s=0]$.

In order for sample attrition to fully explain differences between survivors and other exposure groups in Figure 1, average support for refugees in the incomplete survivor surveys would need to be lower than those in sample. To account for observed differences between survivors and descendants, $\mathbb{E}[Y|E = \text{Survivor}, s = 0]$ must be no higher than 4.7. To explain differences from non-descendants and non-Jews, the numbers are 3.7 and 1 — well outside the 95% confidence region of survivors' in-sample mean of 4.8. With the exception of the already-small differences between survivors and descendants, it is highly unlikely that sample attrition alone can account for the patterns we observe.

A1.4 Summary Statistics

Table A1.3: Summary statistics: respondent socio-demographics by exposure group. S: survivors; D: descendants; ND: non-descendants; NJ: Non-Jews.

Variable	Range				Mean				SD			
	(S)	(D)	(ND)	(NJ)	(S)	(D)	(ND)	(NJ)	(S)	(D)	(ND)	(NJ)
Age	(72,99)	(18,78)	(18,86)	(18,81)	84.12	43.63	51.68	45.32	6.14	16.37	18.10	16.89
Party ID (Republican)	(0,1)	(0,1)	(0,1)	(0,1)	0.30	0.35	0.33	0.54	0.32	0.35	0.35	0.35
Sex (male)	(0,1)	(0,1)	(0,1)	(0,1)	0.57	0.39	0.38	0.38	0.50	0.49	0.49	0.49
Education	(1,7)	(1,7)	(2,7)	(1,7)	5.31	5.88	5.79	4.58	1.91	1.34	1.25	1.41
Income	(1,5)	(1,5)	(1,5)	(1,5)	2.43	2.63	2.40	1.73	1.24	1.22	1.19	0.92
Pre-WWII: E. Europe	(0,1)	(0,1)	(0,1)	(0,1)	0.27	0.59	0.03	0.01	0.45	0.49	0.17	0.08
Pre-WWII: Primary	(0,1)	(0,1)	(0,1)	(0,1)	0.07	0.11	0.00	0.01	0.26	0.31	0.06	0.10
Pre-WWII: Manufacturing	(0,1)	(0,1)	(0,1)	(0,1)	0.15	0.13	0.01	0.03	0.36	0.33	0.11	0.17
Pre-WWII: Services	(0,1)	(0,1)	(0,1)	(0,1)	0.25	0.35	0.12	0.13	0.43	0.48	0.32	0.33
Pre-WWII: Information	(0,1)	(0,1)	(0,1)	(0,1)	0.15	0.15	0.06	0.07	0.36	0.36	0.23	0.25
Immigrant Grandparents	(1,1)	(0,1)	(0,1)	(0,1)	1.00	0.95	0.74	0.36	0.00	0.23	0.44	0.48

A2 Power Analysis

A2.1 Effect of Holocaust Exposure on Attitudes

We run our main model specification (Equation 1, Figure 2a) on a sample of $n = 1527$ respondents across four exposure categories: survivor ($n = 121$), descendant ($n = 271$), non-descendant ($n = 641$), and non-Jew ($n = 494$). This model includes 19 covariates, including pre-/post-war demographics, and dummies for exposure category and treatment. Including these variables reduces our total effective n to 1301 due to missingness.

A power analysis suggests that this design is capable of detecting effect sizes as small as $f^2 = 0.016$.[2] Cohen (1992) suggests f^2 values of 0.02, 0.15, and 0.35 represent small, medium and larger effect sizes, respectively. Thus, our study is sufficiently powered to pick up significant differences even on relatively small effects.

In a robustness check of our main analysis, we run the model on the subsample of $n = 510$ respondents assigned to the *control condition only*, as was our pre-registered plan: 39 survivors, 78 descendants, 224 non-descendant Jews, and 169 non-Jews. This

2. Inputs for the power analysis are: 19 numerator degrees of freedom, 1282 denominator degrees of freedom, significance level of $p = 0.05$, and power level of 0.8.

model includes 16 covariates, yielding a final sample size of $n = 425$ and an f^2 of 0.047 — allowing us to detect medium and large effects, but not small effects.

To illustrate the relative power of each pairwise comparison in our main analysis, Table A2.4 reports the minimum effect size d (Cohen, 1992) we can detect using a t-test comparing mean differences in attitudes across any two sub-samples, where d values of 0.2, 0.5, and 0.8 represent small, medium, and large effect sizes.[3] Using pairwise comparisons, our sample size allows us to pick up medium and large effects, but not the smallest effects – particularly when comparing survivors to descendants. Table A2.5 reports observed Cohen's d values from our data — mean differences in support for refugees between each pair of groups divided by their pooled standard deviation. These are standardized versions of the values in Figure 1. In all paired comparisons besides Survivor vs. Descendant, these values exceed the minimum effect sizes in Table A2.4.

Table A2.4: Minimum effect sizes for each pairwise comparison (Cohen's d)

Exposure	Desc.	Non-desc	Non-Jew
Survivor	0.31	0.28	0.28
Desc.		0.2	0.21
Non-desc			0.17

Table A2.5: Cohen's d for values reported in Figure 1.

Exposure	Desc.	Non-desc	Non-Jew
Survivor	0.01	1.53	1.94
Desc.		1.73	1.97
Non-desc			1.96

A2.2 Effect of Primes on Attitudes

Our second set of analyses examines the effect of our experimental treatment on attitudes toward Syrian refugees across these four groups. The main model specification is a linear model regressing support for refugees on our 3-level treatment variable. However, our results hold with the full set of control variables (as we show on pp. A18-A20).

3. Cohen's d for the control only: Survivor–Descendant $d = 0.5$; Survivor–Non-descendant $d = 0.49$; Survivor–Non-Jew $d = 0.46$; Descendant–Non-descendant $d = 0.37$; Descendant–Non-Jew $d = 0.39$; Non-descendant–Non-Jew $d = 0.29$.

For the full sample, our sample size provides enough statistical power to detect effects as small as $f^2 = 0.006$, which Cohen (1992) defines as "small" effects.[4]

To calculate power for our supplementary interaction models, we use the `Superpower` R package for a) a 3×4 ANOVA design and b) a 3×2 ANOVA design.[5] We specify the observed n for each of the categories (e.g. survivor-control, survivor-inclusive, survivor-exclusive, etc.), the observed μ for our dependent variable for each category, and the full sample standard deviation for our dependent variable ($sd = 1.96$), with a significance level of $\alpha = .05$. Effect sizes are reported as partial ETA-squared, η_p^2. Table A2.6 lists power levels (scaled 0-100) and minimally detectable effect sizes for the $3x4$ model as compared to the observed η_p^2 in our results.

Table A2.6: Power analysis for linear model with 3×4 interaction effect

Effect	Power	Minimum Detectable Effect Size	Observed Effect Size
Condition	98.5	0.01	0.02
Group	100	0.1	0.1
Condition × Group	51.9	0.01	0.005

Results indicate that the study is sufficiently powered to observe the main effects of treatment and exposure, but *not* of the interaction of the two. A non-significant interaction effect may therefore be indicative of low statistical power, rather than a true null.

We repeated the power analysis for a 3×2 interaction model, which collapses the four exposure categories into two (survivors + descendants vs. non-descendants + non-Jews). These results, shown in Table A2.7, indicate that a 3×2 design allows us to detect smaller effect sizes. However, our study remains underpowered, with only a slightly

4. Breaking the power analysis out by subgroup, sample sizes are sufficiently powered to detect small effects among non-descendant Jews ($f^2 = 0.015$) and non-Jews ($f^2 = 0.019$), but only medium effects for the smaller survivor ($f^2 = 0.081$) and descendant ($f^2 = 0.036$) samples.

5. Note: These power analyses for interactive models were conducted after the study. These types of post-hoc power analyses rely on identifying population-level parameters with sample-specific statistics and so are somewhat less valuable than prospective ones (Zhang et al., 2019).

better-than-even (52%) chance of correctly rejecting the null hypothesis of no interaction effect. Still, as we report in pp. A18-A20, our models were nonetheless able to detect significant interaction effects in a 3×2 design.

Table A2.7: Power analysis for linear model with 3×2 interaction effect

Effect	Power	Minimum Detectable Effect Size	Observed Effect Size
Condition	96.6	0.01	0.02
Group	100	0.02	0.02
Condition × Group	52.3	0.005	0.003

A3 Survey Design

A3.1 Survey Flow

The survey proceeded as follows. Holocaust survivors and descendants, so identified by a screener question, first answered several questions about their (or parents' or grandparents') pre-war, wartime, and post-war experiences.[6] We asked these questions prior to treatment to ensure proper branching. Next, we assigned all respondents to one of three treatment conditions, as described below. After treatment, subjects answered questions about various intergroup attitudes, beginning with their attitudes about Syrian refugees.

Respondents who indicated they were Jewish answered additional questions pertaining to potential social pathways by which political attitudes might transmit across generations: involvement in the Jewish community, Holocaust education and remembrance activities. Non-survivors then answered questions about their parents' backgrounds.[7] The survey concluded with a battery of socio-demographic questions.

6. While social desirability bias may exist in this self-reported measure (e.g. over-reporting of relatives as survivors, due to the broad nature of the term), this should be relatively rare. For example, using follow-up questions about forced transport to ghettos, camps and related matters, we found no cases of individuals who reported being survivors but left Europe prior to Nazis' rise to power.

7. Descendants answered questions about their *non*-survivor parent(s), since they would have provided information on survivor relatives at the beginning of the survey.

One potential concern with our survey flow is that it asks Holocaust survivors and their descendants, but not non-exposed populations, to recall details about their (or their relatives') experiences in the Holocaust prior to receiving treatment. Practically, it was important to ask these questions at the outset to properly branch subsequent survey sections and collect enough information on pre-war demographic covariates to fully specify our model and assess patterns of attrition (SI A1.3). Yet there is some recent evidence that priming immigrant histories has a small positive effect on support for immigration (Williamson et al., 2020), and reflecting on the Holocaust may conceivably prime survivors and descendants to be more supportive of refugees.

This is plausible, but likely inconsequential for our results. First, it is doubtful that such effects would be large enough to account for the differences we find across populations. Priming effects from past immigration studies have found effect sizes equivalent to a 0.06-0.08 standard deviation shift (Williamson et al., 2020) – far smaller than the differences we find between survivor/descendant populations and the non-exposed groups. Moreover, what we are priming in this study are not immigration histories, but historical victimization. Making one's experience in the Holocaust more salient could theoretically increase support for refugees (e.g. by reminding survivors of the horrors refugees are fleeing), but it could just as easily increase suspicion of outgroups by recalling past trauma and enhancing threat perceptions.

A3.2 Treatment

Our experimental treatment emphasized either an outgroup or ingroup oriented interpretation of the "never again" imperative. A control condition emphasized neither.

1. *Outgroup focused, inclusive:* In 1939, the St. Louis ocean-liner carried German Jewish refugees fleeing the worsening situation in Germany to the United States. However, due to strict immigration quotas at the time – and despite knowledge about the

danger Jews faced in Nazi Germany – the refugees were sent back to Germany where many died in the Holocaust. Today, advocates of admitting more Syrian refugees to the United States frequently cite the Jewish imperative to 'never again' turn a blind eye to such slaughter, warning that many Syrians may die if they are not admitted to the US, while those opposed warn that extremists and terrorists may hide among the refugees. What do you think...

2. *Ingroup focused, exclusive:* In 1933, Hitler rose to power by stoking anti-Semitic views in Germany, arguing that Jews were an inferior, corrupt race bent on world domination. The spread of these attitudes among the German population was the precursor of the violence to come – leading to anti-Jewish pogroms and, eventually, the Holocaust and near destruction of European Jewry. Today, advocates of restricting the entrance of Syrian refugees to the United States frequently cite the Jewish imperative to 'never again' go like lambs to slaughter, warning that extremists and terrorists may hide among the refugees, while those opposed warn that many Syrians may die if they are not admitted to the US. What do you think...
3. *Control:* Advocates of admitting more Syrian refugees to the United States frequently warn that many Syrians may die if they are not admitted to the US. On the other hand, those opposed warn that extremists and terrorists may hide among the refugees. What do you think...

To avoid order effects, we used two versions of the control. The second version reverses the order of these two statements so that the anti-immigrant statement comes first.

A3.3 Additional Covariates

We also measure several covariates that could potentially confound our analysis or otherwise moderate the relationship between exposure and attitudes towards refugees.

For those with ties to the Holocaust, measured covariates included: pre-war residence of the survivor's family, pre-war profession of the survivor's family, pre-war religiosity of the survivor's family, pre-war socio-economic status of the survivor's family,[8] whether they (or their parent/grandparent) were forced to move to a ghetto, sent to a concentration camp, served in an underground movement, or were under captivity at war's end, whether they (or their parent/grandparent) received aid from non-Jews that helped them survive the Holocaust, and how often they (or their parent/grandparent) spoke about the Holocaust when they (or their children) were growing up.

For all Jews, we asked whether they were Reform, Conservative, Orthodox, Other, or Non-Jewish, how active they were in their local Jewish community, and how active they were in Holocaust education. For all respondents, we collected information on political interest, political ideology, party ID, age, gender, income, education, and parents' background (age, religiosity, SES, country of origin, profession, immigration to U.S.).

We did not ask respondents whether they self-identify as Ashkenazi, Mizrahi or Sephardi. However, we can (roughly) approximate these proportions by using countries of family origin as a proxy variable, with the assumption that Jewish families from European countries could be classified as Ashkenazi. In our data 58% of survivors grew up in Western Europe, 40% in Eastern Europe, and less that 1% each in North Africa, the Middle East or Central Asia. Among descendants, 26% had survivor relatives from Western Europe, 69% from Eastern Europe, 5% from both Western and Eastern Europe, and less than 1% from North Africa and other regions. Among the broader non-descendant Jewish sample, 84% marked both parents' countries of origin as the United States, meaning they were at least third-generation Americans. Of the remaining non-descendants, 8.5% had parents from Eastern Europe, 3% from Western Europe, 2.5% from the Americas (excluding U.S.), 2% from the Middle East and North Africa and less than 1% from other

8. If descendants of (multiple) survivors indicated multiple pre-war residences, professions, religiosity or socio-economic status, we chose one at random.

regions. If we assume that *all* of the non-descendants with parents from outside Europe are Mizrahi and Sephardi, while their proportions among third-generation American Jews is similar to that in the broader U.S. Jewish population (1% and 3% according to a recent Pew Survey,[9] with 6% identifying as some combination), then the maximum percentage of Sephardi and Mizrahi Jews in the non-descendant sample is 12.9%. Making a similar calculation for survivors and descendants, we estimate the maximum share of Mizrahi or Sephari Jews in those samples to be less than one percent each.

A4 Additional Observational Analyses

A4.1 Analyses Restricted to the Control Group

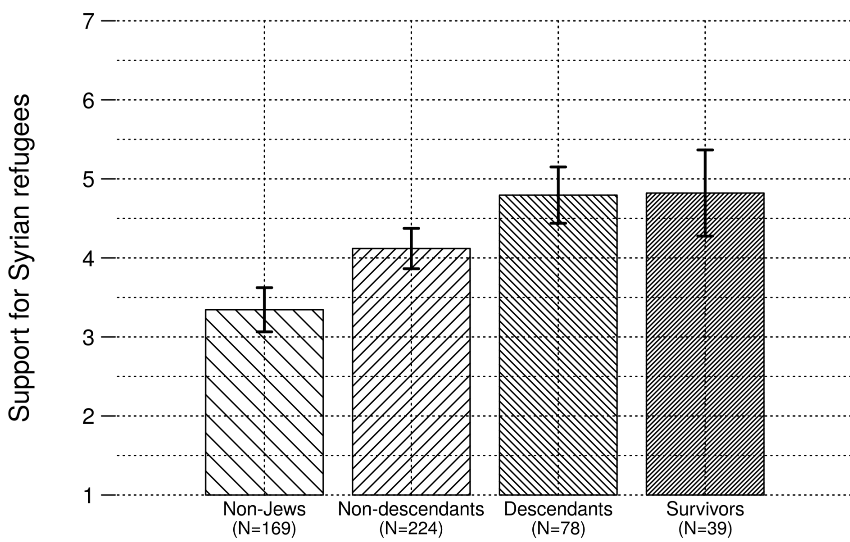
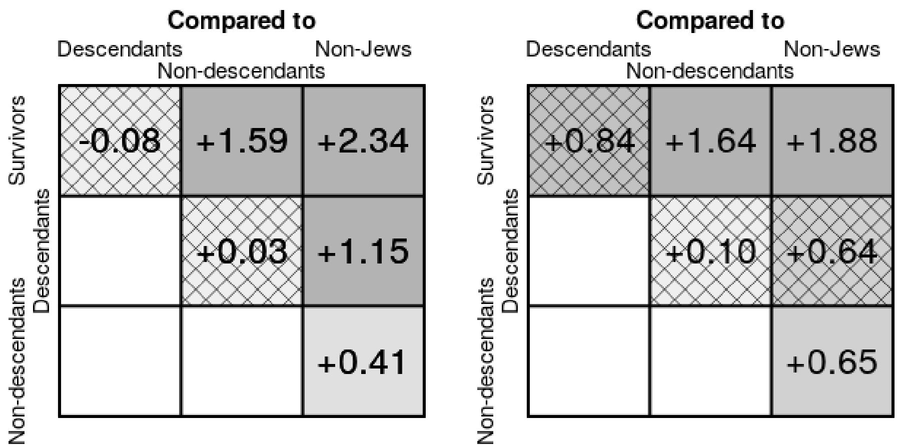


Figure A4.4: Replication of Figure 1, respondents in control group only.

9. https://www.pewforum.org/2021/05/11/jewish-americans-in-2020/



(a) OLS (b) Sequential-g

Figure A4.5: Replication of Figure 2, respondents in control group only.

A4.2 Alternative Measures of Outgroup Attitudes

Figure A4.6 replicates the analyses in Figure 2, with alternative measures of outgroup attitudes. These include (a) building a U.S.-Mexican border wall,[10] (b) imposing a ban on Muslim migration to the U.S.,[11] (c) establishing a "safe zone" for civilians in Syria,[12] and (d) intervening in armed conflicts to protect civilians, as a general policy.[13]

If the outgroup protection hypothesis is correct, we should expect negative relationships between genocide exposure and support for the border wall and travel ban, and positive relationships with support for "safe zones" and responsibility to protect. This is, indeed, what we find. Survivors are less supportive of the travel ban than non-descendants and non-Jewish Americans, and marginally less supportive of the border wall. Survivors are also more supportive than non-descendants of military measures to

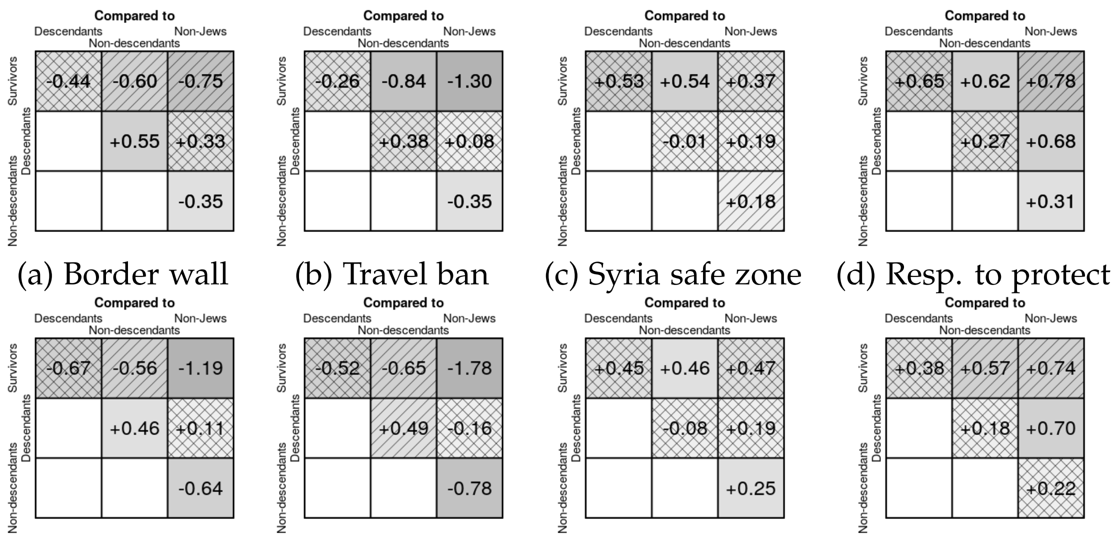
10. Question wording: "How strongly would you support or oppose building a wall along the US-Mexican border in an attempt to stop illegal immigration?"

11. Wording: "How strongly would you support or oppose a temporary ban on Muslim immigrants to the United States in order to reduce the chance of a terrorist attack?"

12. Wording: "How strongly would you support or oppose the United States establishing a safe zone in Syria for civilians fleeing ISIS and the Assad regime?"

13. Wording: "Do you think the United States has or does not have a responsibility to intervene in armed conflicts to stop the killing of civilians?"

protect civilians in Syria and elsewhere. Non-descendants, in turn, are less supportive of the wall and travel ban than non-Jewish Americans, and more supportive of a responsibility to protect civilians. The only deviant result is that descendants are *more* supportive of the border wall and travel ban than non-descendants. All others suggest that individuals more directly exposed to the Holocaust are more supportive of outgroups.



(e) Border wall (f) Travel ban (g) Syria safe zone (h) Resp. to protect

Figure A4.6: Alternative outgoup attitude measures, OLS (a-d) and ACDE (e-h).

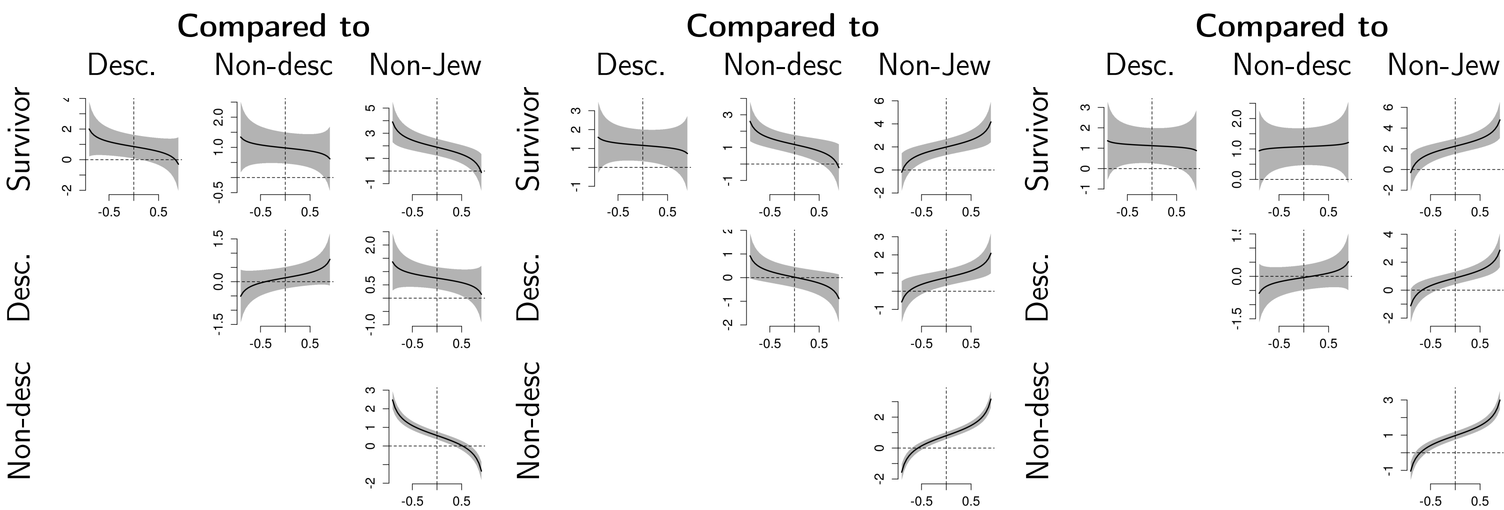
A4.3 Sensitivity Analysis of ACDE Estimates

Sequential-g estimation rests on two assumptions. First is sequential unconfoundedness, which requires that there are no omitted variables for the effect of treatment on the outcome (conditional on pretreatment confounders), and no omitted variables for the mediator's effect on the outcome (conditional on treatment, pretreatment and intermediate confounders). Second is the assumption of no intermediate interactions, meaning that the effect of the mediator on the outcome is independent of intermediate confounders.

We assess violations of sequential unconfoundedness through a sensitivity analysis that evaluates how ACDE estimates change for different levels of post-treatment confounding in the mediator-outcome relationship (Acharya, Blackwell and Sen, 2016, 11).

The results of this analysis – for each pairwise comparison and each mediating variable – are in Figure A4.7. The black lines represent ACDE estimates (vertical axes) at different levels of correlation between mediator and outcome errors (horizontal axes). Our main ACDE estimates correspond to values where this correlation is zero. These results show that, in most cases, the unmeasured confounding for the mediator's effect would have to be quite severe (approaching $\rho = 1$ or -1) to change our substantive results.

Figure A4.7: Sensitivity analysis of sequential-g ACDE estimates. Black lines show the estimated ACDE (vertical axes) at different levels of correlation between mediator and outcome errors (horizontal axes). Gray areas show 95% confidence intervals.



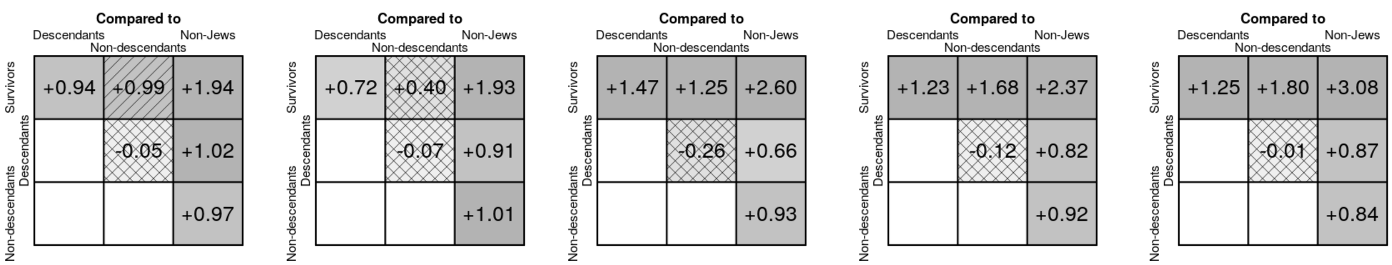
(a) Mediator: Party ID (b) Mediator: Education (c) Mediator: Income

A4.4 Telescopic Matching

In addition to sequential-g estimation, we estimated ACDE's with telescopic matching. This procedure uses nonparametric matching to impute counterfactual outcomes for fixed values of each mediating variable, and uses these imputations to estimate the direct effect of exposure, holding mediating variables constant (Blackwell and Strezhnev, 2018).

Because telescopic matching requires binary treatments and mediators, we dichotomize all covariates (e.g. above/below median education, etc.) and treatment assignments, splitting the sample into pairwise comparisons. Let E_i be i's exposure category (e.g. 1 if survivor, 0 if descendant), and M_i be the value of a mediator (e.g. 1 if Republican, 0 if Democrat). We match respondents with $M_i = 1$ to others with $M_i = 0$, but similar values of $\mathbf{X}_i^{(pre)}$ and identical exposure E_i. After imputing potential outcomes for matched respondents, we perform a second matching stage with respect to E_i, minimizing imbalance on $\mathbf{X}_i^{(pre)}$. The ACDE estimate is $\hat{\tau} = \frac{1}{N}\sum_i^N \left(\widehat{\text{Attitudes}}_{i10} - \widehat{\text{Attitudes}}_{i00}\right)$, where $\widehat{\text{Attitudes}}_{i10}$ ($\widehat{\text{Attitudes}}_{i00}$) are i's imputed attitudes under $E_i = 1$ (0) and $M_i = 0$.

Figure A4.8 reports ACDE estimates separately for three potential mediators — party identification, education and income — along with dummy variables indicating experimental treatment group. Differences across exposure categories are generally of similar magnitude and direction as those in Figure 2 in the main text.



(a) Party ID (b) Education (c) Income (d) Outgroup-prot. (e) Ingroup-prot.

Figure A4.8: Telescopic matching estimates of ACDE of genocide exposure on outgroup attitudes. Values represent average differences in support for increasing admission of Syrian refugees between groups in the rows and columns, while holding each mediating variable constant. Darker shades indicate larger differences. Diagonal lines indicates that differences are insignificant at the 95% (single) or 90% confidence level (double).

A4.5 Wartime Experiences and Out-Group Attitudes

Tables A4.8-A4.11 report the results of analyses to detect potential heterogeneities due to wartime experiences: whether survivors or families of descendants received help from non-Jews (52% of survivors and 53% of descendants in sample), whether they had been relocated to a ghetto (48%, 64%), interned in a camp (32%, 53%), or had joined armed resistance groups (5%, 22%). These self-reported personal and family experiences do not significantly influence outgroup attitudes in any model.

Table A4.8: Wartime experiences (received help from out-group member) and support for Syrian refugees. OLS and sequential-g estimates, bootstrapped standard errors in parentheses. Coefficient estimates for pre- and post-treatment control variables not shown. S: survivors, D: descendants.

Model type	Model 1 OLS	Model 2 OLS	Model 3 OLS	Model 4 seq-g	Model 5 seq-g	Model 6 seq-g
Helped by out-group	0.33 (0.29)	0.47 (0.37)	-0.27 (0.49)	0.57 (0.26)	0.65 (0.35)	0 (0.41)
Pre-WWII covariates	Yes	Yes	Yes	Yes	Yes	Yes
Post-WWII covariates	No	No	No	Yes	Yes	Yes
Exposure	S, D	D	S	S, D	D	S
AIC	726.3	411.8	317	742.7	410.2	346.8
N	177	103	74	177	103	74

Significance (two-tailed): ′p<.1,*p<.05,**p<.01,***p<.001

Table A4.9: Wartime experiences (relocated to ghetto) and support for Syrian refugees.

Model type	Model 1 OLS	Model 2 OLS	Model 3 OLS	Model 4 seq-g	Model 5 seq-g	Model 6 seq-g
Relocated to ghetto	0 (0.3)	0.51 (0.42)	-0.53 (0.48)	-0.04 (0.29)	0.51 (0.4)	-0.65 (0.44)
Pre-WWII covariates	Yes	Yes	Yes	Yes	Yes	Yes
Post-WWII covariates	No	No	No	Yes	Yes	Yes
Exposure	S, D	D	S	S, D	D	S
AIC	727.6	411.9	315.9	748.4	413.2	342.7
N	177	103	74	177	103	74

Significance (two-tailed): ′p<.1,*p<.05,**p<.01,***p<.001

A5 Additional Experimental Analyses

In addition to the experimental results in Figure 4, we considered model specifications that include demographic and pre-WWII covariates (Table 1), and regional fixed effects:

$$\text{Attitudes}_i = \theta \cdot T_i + \beta_1' \mathbf{x}_j^{(pre)} + \text{Region}_j^{(pre)} + \epsilon_i$$
$$\text{Attitudes}_i = \theta \cdot T_i + \phi \cdot \text{Exposure}_i + \gamma \cdot T_i \times \text{Exposure}_i + \beta_1' \mathbf{x}_j^{(pre)} + \text{Region}_j^{(pre)} + \epsilon_i$$

where the first equation is an expansion of the treatment-only model in equation 4, and the second is an expansion of the (3×4 and 3×2) interactive model in equation 5. Figure A5.9a shows estimates from these expanded specifications, ordered left to right, which are numerically close to those in Figure 4 in magnitude and significance.

Table A4.10: Wartime experiences (interned in camp) and support for Syrian refugees.

Model type	Model 1 OLS	Model 2 OLS	Model 3 OLS	Model 4 seq-g	Model 5 seq-g	Model 6 seq-g
Interned in camp	0.07 (0.31)	0.03 (0.38)	0.29 (0.57)	0.11 (0.28)	0.11 (0.35)	0.3 (0.53)
Pre-WWII covariates	Yes	Yes	Yes	Yes	Yes	Yes
Post-WWII covariates	No	No	No	Yes	Yes	Yes
Exposure	S, D	D	S	S, D	D	S
AIC	727.6	413.5	317	747.7	415.3	344.3
N	177	103	74	177	103	74

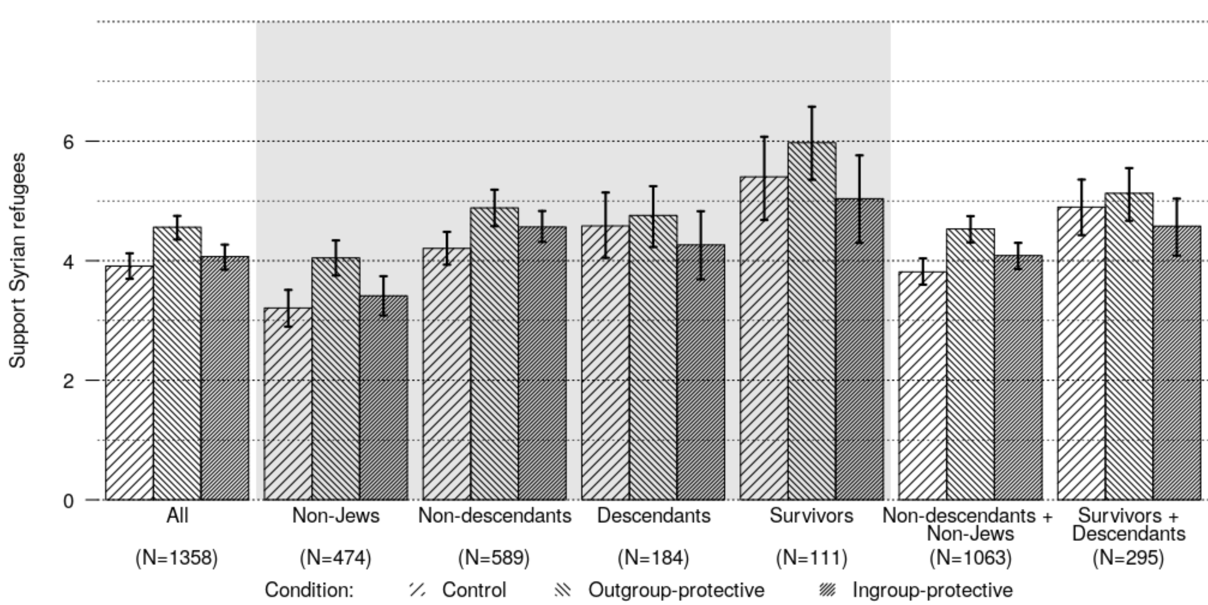
Significance (two-tailed): ′p<.1,*p<.05,**p<.01,***p<.001

Table A4.11: Wartime experiences (fought in armed resistance movement) and support for Syrian refugees.

Model type	Model 1 OLS	Model 2 OLS	Model 3 OLS	Model 4 seq-g	Model 5 seq-g	Model 6 seq-g
Active in resistance	-0.04 (0.42)	0.06 (0.42)	-0.62 (1.44)	0.6 (0.45)	0.42 (0.45)	1.57 (1.23)
Pre-WWII covariates	Yes	Yes	Yes	Yes	Yes	Yes
Post-WWII covariates	No	No	No	Yes	Yes	Yes
Exposure	S, D	D	S	S, D	D	S
AIC	727.6	413.5	317.1	744.3	414.8	343.3
N	177	103	74	177	103	74

Significance (two-tailed): ′p<.1,*p<.05,**p<.01,***p<.001

Figure A5.9: Experimental analyses, with covariates included.



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