Once We Too Were Strangers: Can a Heritage of Displacement be Leveraged to Build Support for Present-Day Refugees?*

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Abstract

Prior studies suggest that subtle messaging interventions can reduce prejudice by stimulating perspective-taking. For instance, there is evidence that reminding citizens of their family's experiences with displacement can induce empathy toward refugees. We test the generalizability of this treatment in five new studies in Cyprus, Turkey, and Greece, and find no evidence that descendants of displaced Turks, Greeks, or Greek Cypriots become more sympathetic toward refugees when a comparison to their own family experiences is drawn. In some cases, they become more hostile. Our study raises doubts about the scalability of this strategy to reduce anti-refugee bias. Drawing ingroup-outgroup parallels generates context-specific effects and does not move policy attitudes. We conclude by discussing the practical limitations of light-touch interventions and calling for more research on how prejudice-reduction interventions can be scaled.

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1 Introduction

In the face of a major refugee influx, citizens often express hostility toward new arrivals, even in societies where many families once fled similar levels of danger and persecution (Connor 2018). A heritage of displacement may not trigger sympathy for contemporary refugees/immigrants *automatically* (cf. Wayne and Zhukov 2022); however, recent studies suggest that it can if that heritage is made salient (Williamson et al. 2021). For instance, Dinas et al. (2021a) show that a vignette comparing the Syrian refugees' experience with the displacement of Germans and Greeks in the 20th century leads to greater empathy for refugees among Greeks and Germans whose ancestors had been displaced.

This striking result echoes findings in an influential literature in psychology which has shown that perspective-taking (PT)—"the active consideration of others' mental states and subjective experiences" (Todd and Galinsky 2014, 374)—can reduce stereotype expression (Galinsky and Moskowitz 2000) and racial bias (Todd et al. 2012; Shih et al. 2009) and foster intergroup helping behavior (Bilewicz 2009) toward stigmatized groups (Batson et al. 2002), while increasing willingness to engage in intergroup contact (Wang et al. 2014). These insights are increasingly used in political science to explore ways to reduce bias against vulnerable groups, including immigrants and refugees (e.g., Adida et al. 2018; Kalla and Broockman 2023; Williamson et al. 2021). Among different approaches to induce PT, "light touch" interventions, which are by definition "easy to implement, brief, and cheap" (Paluck et al. 2021), seem especially promising because they are *scalable*. The "shared history" intervention of Dinas et al. (2021a) discussed above does not require door-to-door canvassing or direct contact with the target, thus making it cost-effective and scalable through an advertising campaign, or the rhetoric of a political leader. And if subtle reminders of one's ancestors' experiences are sufficient to build support for contemporary refugees among refugee descendants, then such an approach can be used to build allyship for other marginalized groups, prompting respondents to recategorize themselves and "the other" as part of a larger group, bound by a common set of experiences.

Our attempts to apply this approach to new contexts, however, serve as a cautionary tale. Across

five studies in Turkey, Cyprus, and Greece, we find null or negative effects. Reminding respondents of their own country's experience with displacement rarely makes them significantly more supportive of present-day refugees; in some contexts, the descendants of the displaced persons actually become more hostile. These results cast shadow on the external and temporal validity of the shared history interventions.

For a test of external validity, we first tried the approach in Cyprus, whose citizens share a common culture with (but a much more recent mass displacement than) citizens of Greece. We then applied it to Turkey, where the historical displacement is the same as that of Greece (due to post-WWI population exchanges between the two countries) but which has a different culture and political climate. We then checked temporal validity, by testing the approach again in Cyprus (twice) and in Greece (in a direct replication of the original study). The Cyprus and Greece studies were both done several years after the initial onset of the European refugee crisis, which allows us to capture treatment effects in a changed immigration context. Furthermore, we also test the sensitivity of this approach to subtle differences in treatment wording and outcome measures. For instance, we introduce a novel behavioral measure—agreeing to have your name printed in a newspaper ad in support of refugees—which we believe provides a more meaningful measure of behavior than standard quasi-behavioral outcomes like signing an online petition. These differences in scope, design, and context taken together offer a broader perspective on the effectiveness of these "salience treatments" than any previous study.

We find no evidence that people with a family history of displacement respond more positively to our treatments than those without such family experiences. These results are striking in part because the family history approach has been successfully applied in Germany (Dinas et al. 2021a), Korea (Hong et al. 2021) and the U.S. (Williamson et al. 2021; Wayne and Zhukov 2022). The problem, therefore, is not that the approach never works, but that there is no easy way to predict when it will, and this limits the lessons we can learn regarding the generalizability and scalability of light-touch interventions. Our study contributes to the literature on prejudice-reduction interventions while offering a new model for replicating and extending experimental studies, by testing

the sensitivity of treatment effects to small changes in wording, outcomes, and national/political or temporal context.

2 Research Design

We test the potential for analogical thinking to build support for refugees by applying the shared history approach of Dinas et al. (2021a) to Greece, Turkey, and Cyprus. The authors hypothesize that making a heritage of displacement salient will induce empathy toward refugees among citizens with a family history of displacement. We test this hypothesis with five survey experiments implemented via telephone (in Turkey and Greece), door-to-door (in Cyprus 2018), and online (Cyprus 2021 and 2022). Like Dinas et al. (2021a), we presented respondents with a brief paragraph about an ongoing refugee crisis. Respondents randomly assigned to treatment saw a version that draws a parallel between this crisis and a historical refugee crisis that affected the respondent's own ethnic group. Of note, our study is not meant to be a replication of Dinas et al. (2021a) in the laboratory sense, repeating an experiment under identical conditions. Rather, we test the sensitivity of the intervention to changes in wording as well as context. Other studies follow similar approaches. For instance, Hong et al. (2021) replicate the Dinas et al. (2021a) study in Korea without drawing direct parallels between the experiences of Korean refugees and current refugees, yet they are able to find positive effects. We sometimes use the the exact wording of the original treatment (in our survey in Greece) and sometimes modify it slightly. For example in the Cyprus 2021 survey, respondents were shown the following paragraph:

> As you may know, more than 80 million people around the world have been forced to flee their homes due to wars like the one in Syria. More than 12 million Syrians have been displaced and many will not be able to return home. Hundreds of thousands have applied for asylum in Europe and in 2015-2016 more than half a million Syrians were granted asylum in European Union countries. Ethnic wars like the one in Syria create humanitarian crises and cause terrible suffering. *[Treated respondents only:]* just like

what we went through during the 1974 refugee crisis after the Turkish invasion.

We discuss later (and in the appendix, in more detail) the differences between our treatments and the original study. There are also contextual differences of note: For Greeks and Turks, the historical crisis mentioned in the vignette was the 1924 population exchanges that forcibly uprooted Greeks living in Turkey and Turks living in Greece. For Cypriots, the crisis was the internal displacement of Greek Cypriots from Northern Cyprus in the 1974 Turkish invasion. Roughly half of respondents in Cyprus are from *refugee families* in which either the respondent, their parents, or grandparents were displaced. Our primary quantity of interest is the interaction between receiving *treatment* and being from a *refugee family*, as well as the net effect of treatment on these families.[1]

The Turkish context shares the advantages of the original study of Greece (in many ways, it is the flipside of the mirror opposite of the Greek side), but it also shares its drawbacks in that there were significant pre-displacement differences between the exchanged populations and natives in Greece or Turkey. We chose to also analyze data from Cyprus, because of the recency of displacement experiences compared to that in Turkey and Greece and because it offers a setting in which not only displacement was as-if random, but also pre-treatment differences between the displaced and non-displaced populations were absent or negligible.

When Turkey invaded in 1974, all Greek Cypriots living on the northern half of the island were forced to relocate to the south. Prior to the war, there were no meaningful political or demographic differences between Greek Cypriots living in the north or south. The island is small—the most far-flung communities in Cyprus are within a 2-hour drive of the capital—and the prewar Greek population was small (around half a million). Indeed, the north/south distinction only became meaningful after the imposition of a buffer zone to end the war via partition; and the location of the buffer zone was unrelated to population characteristics that could be connected to present-day

[1]The studies were pre-registered (osf.io/8brce, aspredicted.com #69995, osf.io/7xrhg/ respectively) and approved by University of Pennsylvania IRB (protocols #848955, #849305, #850782). See Appendix H for a discussion of ethical considerations.

differences in social preferences. The points where the Turkish invasion started were determined on the basis of military exigencies, and the location of the ceasefire line was determined by the progress of the war and by the timing of an international intervention that halted the advance of the Turkish army. Greeks and Turks were intermingled throughout the island before the war and episodes of interethnic violence occurred both in the present-day north and south.

While our Greece study uses the same treatment, outcomes, and sampling locations as Dinas et al. (2021a), varying only the temporal context, our other studies feature different outcome measures ranging from empathetic concern to support for granting work permits (see Appendix D.4). Our experiment vignettes likewise vary considerably in how much detail they include (see Appendix D.3). Importantly, extant theory regarding the best way to make shared family history cognitively salient does not generate predictions regarding the specific wording that should be used in such interventions. Thus, it is important to explore different ways to express the same sentiment. As a result of modifying these vignettes, the control conditions in several of our experiments are stronger than the original study. We note that, the more information about the refugee crises (both at home and abroad) is provided in the control condition, the less likely it is that informational equivalence problems will arise, confounding the interpretation of treatment effects across subgroups of the population that may differ with respect to their prior knowledge of these crises (e.g. descendants vs not).

In general, we opted for creating "stronger" treatments, feeling that the original treatment was too weak. In one of our experiments (Cyprus, 2022), we created several versions of the same treatment to test effects of small modifications in language that might trigger different emotional responses or appeal to different subgroups of the population. Ultimately, we view that our approach provides a fuller exploration of the robustness of such light-touch cognitive interventions than any other published study, though of course it is possible to create even more stringent, richer tests. While our five studies are too few in number to isolate the effects of different outcomes versus treatments versus context, they do test how robust the shared history approach is to these sorts of variations.

3 Results

Across all five studies, we regressed outcomes on *refugee family*, *treatment*, and their interaction, along with a set of pre-registered control variables using OLS, we average them to facilitate comparison and present the results in Figure 1.[2] While our estimate of the treatment effect is positive in four out of five studies, it never reaches $p < 0.05$. Moreover, the interaction between treatment and refugee families—the respondents for whom the historical parallel ought to hold the most resonance—is generally negative, as is the net effect on refugee families (treatment effect plus interaction effect). This interaction is what is at the core of the "family history" theory, which is why we focus on it in our analysis. We estimate the underlying effect driving the results in all five studies using a random-effects meta-analysis, as shown in Figure 2 (details in Appendix E.3). On the whole, refugee families may be more sympathetic than non-refugee families in the control condition, but treatment affects them negatively ($p < 0.05$), leading to a net loss of sympathy.[3]

To determine whether the treatment affected behavior as well as attitudes, Dinas et al. (2021a) asked respondents about three quasi-behavioral outcomes. First, since respondents were incentivized via a raffle, they asked them if they would commit to donate a portion of their potential winnings to a refugee relief organization. We repeated this question in our Greece, Turkey, and Cyprus 2021 surveys. As Figure 3 reveals, the treatment failed to increase donations among refugee families in any of the three countries. Second, respondents were asked if they would agree to have a letter sent to parliament on their behalf advocating for increasing (or decreasing) the number of refugees admitted. We replicated this outcome in Greece and Turkey but found no effect (see Appendix F, Tables F1 and F2). Third, respondents were asked if they would sign an online petition asking refugees to be moved from open-air camps to hostels and reception centers. While this housing issue was no longer relevant in Greece and Turkey by the time of our study, it remained an acute problem in Cyprus in 2022. Pournara Reception Center, a camp for newly arrived asylum-seekers, had been in the news for months due overcrowding and unhealthy living

[2]Question wording for these outcomes is listed in appendix D.4. See F for individual results.

[3]The meta-analysis is exploratory and was not pre-registered.

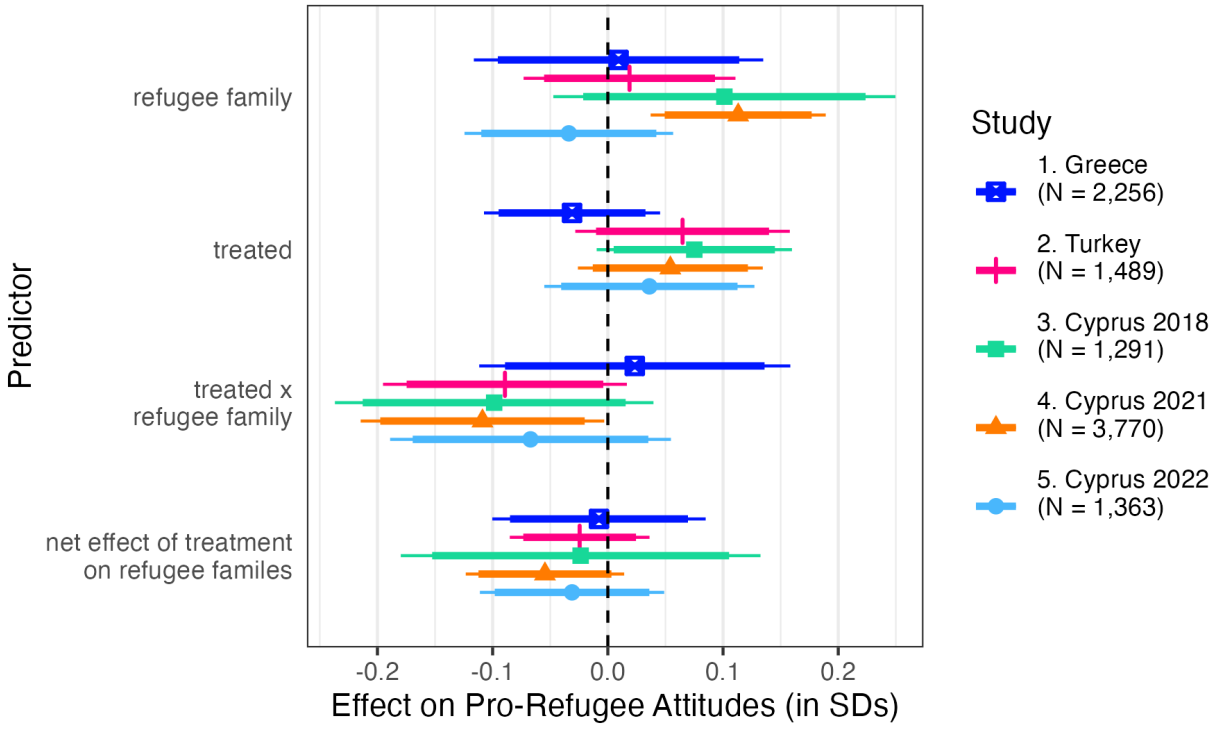

Figure 1: Effect of treatment on attitudes toward refugees across five studies. OLS regression with robust standard errors. CIs: 95% (outer bars) and 90% (inner bars). The outcome for each survey consists of all attitude questions that we asked, standardized and—if there were multiple questions—averaged. In Greece, these measures are: *Study in Greek schools*, *Residence permit*, *Money to Greeks*, *Terror threat*, and *Increase crime*; Turkey: *admissions*, *permits*, *concern*, and *therm refugees*; Cyprus 2018: *asylum*; Cyprus 2021: *concern*; and Cyprus 2022: *children*, *families*, *open camp*, *permits*, *admission*, and *men*. See appendix D.4 for question wording.

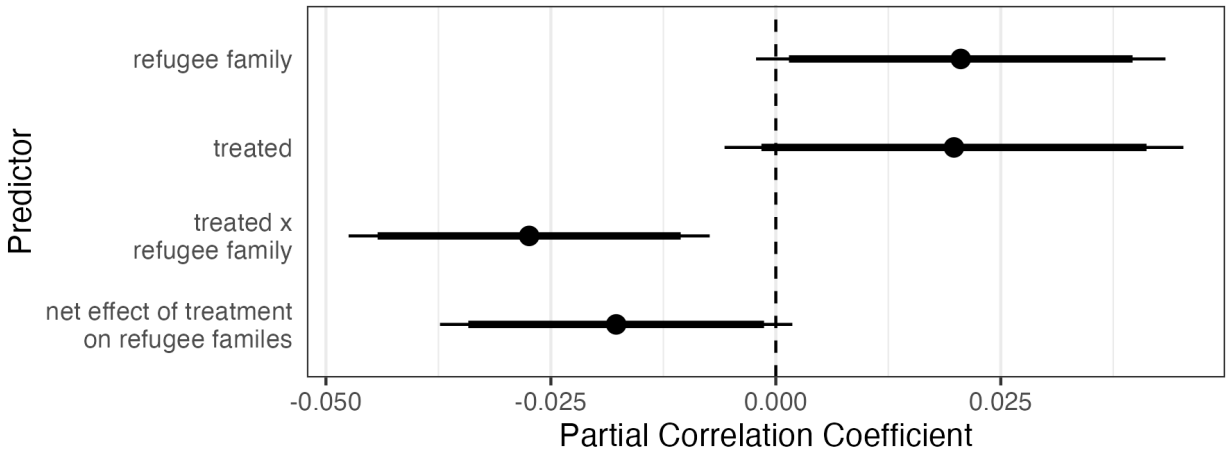

Figure 2: Meta-analysis of all five studies using OLS with study-level random effects. Outcome represents the association between predictor and pro-refugee attitudes after controlling for other variables. CIs: 95% (outer bars) and 90% (inner bars).

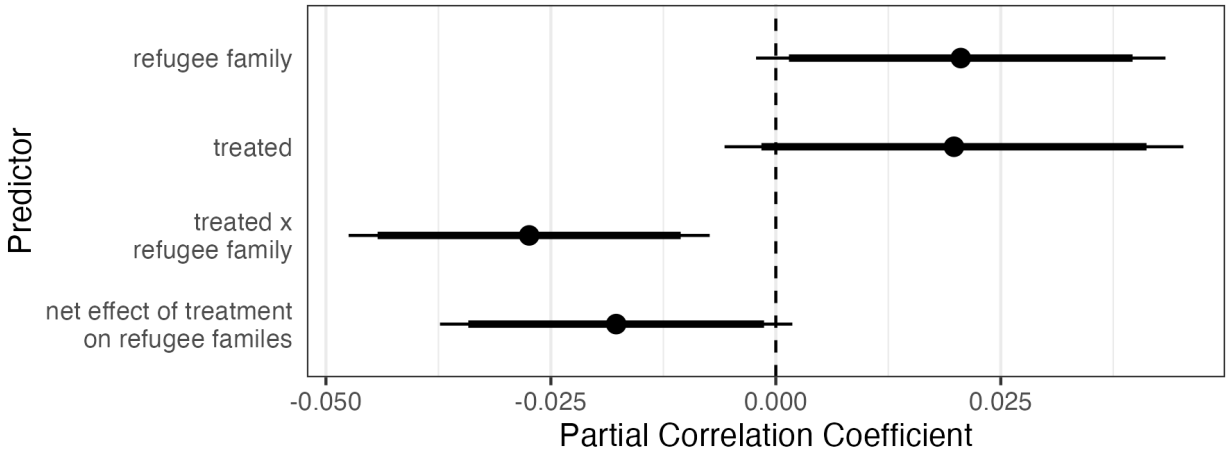

Figure 3: Effect of treatment on willingness to donate to a refugee relief organization. OLS regression with robust standard errors. CIs: 95% (outer bars) and 90% (inner bars).

conditions sparking international condemnation.

Dinas et al. (2021a) report that their online-petition-signing outcome did not respond to treatment, so we opted to try something stronger. We designed a full-page open letter, to be published in a leading Cypriot newspaper, calling upon the government to move children and unaccompanied minors among the refugees to safe housing immediately (see Figure 4). Respondents were asked if they would like to have their name added to the list of signatories to be published in the newspaper. Those who agreed were sent to a website where they could leave their full name, town, and email address. This behavioral measure goes well beyond similar approaches in political behavior studies. Those with greater political knowledge will likely realize that online petitions have virtually no effect on policy, nor is anyone likely to see their name. Online petitions therefore, come closer to measuring private sentiments than meaningful political action. Those who agree to sign such a petition are not necessarily any more likely to take actions that may affect their reputations or require effort. By contrast, signing the letter in a high-profile newspaper is a public action visible to signatories' friends, family, colleagues, and people outside of their social network they might interact with (in the neighborhood, school, or marketplace) in a country where asylum is highly controversial. As such, we have a better measure of a costly political action compared to previous studies. This measure also has a realistic chance of influencing public debate and government

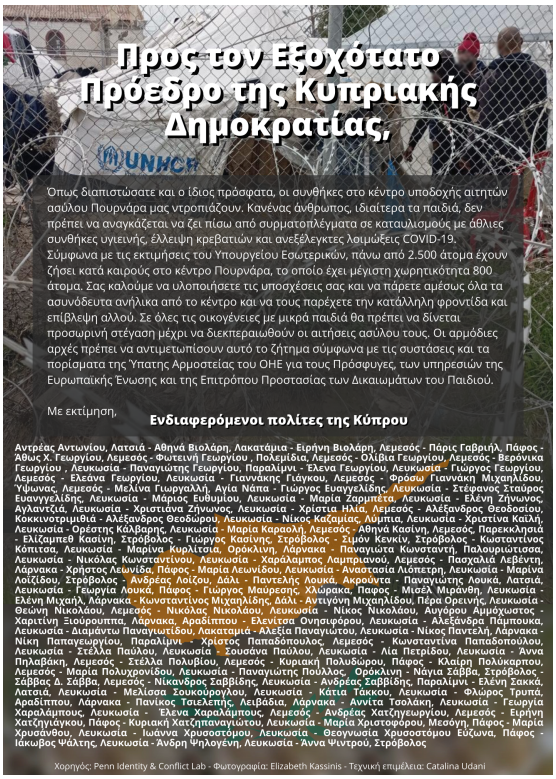

Figure 4: Ad published in the newspaper *Phileleptheros* (March 27, 2022) with signatories' names.

policy by publicly shaming the government to enforce human rights laws.

In contrast to the quasi-behavioral outcomes used in Dinas et al. (2021a), our new behavioral outcome shows *negative* interaction effects (see Figure 5, left panel). The effect is stronger (and statistically significant) when the sample is limited to refugee descendants rather than first-generation refugees (right panel). We explore the net effects in Figure 6. Treatment appears to make refugee descendants significantly less likely to sign the letter, but first generation refugees who experienced displacement first hand more likely.[4] Perhaps first-generation refugees feel their refugee identity more strongly not only because they have first-hand experiences of wartime displacement, but also because they have suffered bias and discrimination at the hands of their co-nationals. By contrast, their offspring might see the refugee identity as an undesirable differentia-

[4]In Appendix Table F7, we provide evidence that this is not merely due to age. Moreover, Cyprus might not be an isolated case: Hong et al. (2021) find positive effect of a similar salience treatment for the first-generation Korean refugees, but not their descendants.

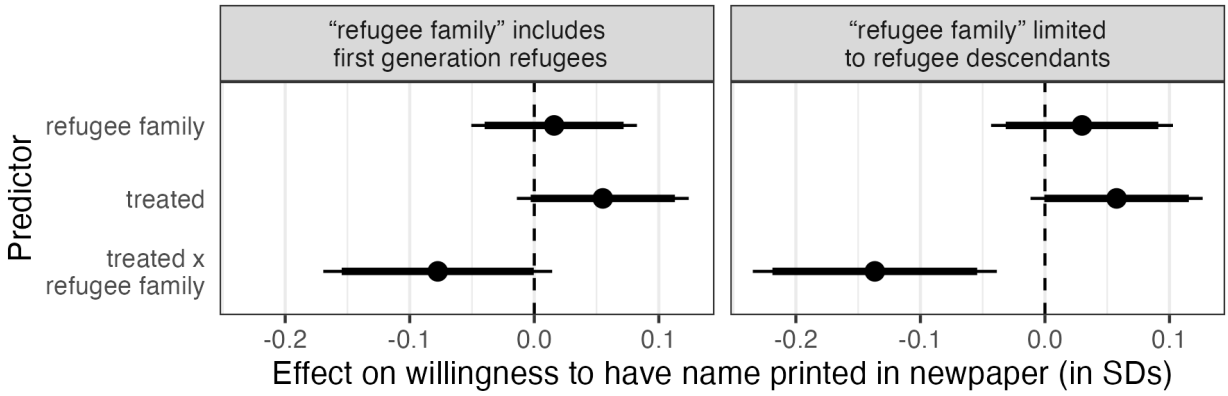

Figure 5: Effect of treatment on adding one's name to a newspaper ad advocating for asylum seekers. OLS regression with robust standard errors. CIs: 95% (outer bars) and 90% (inner bars).

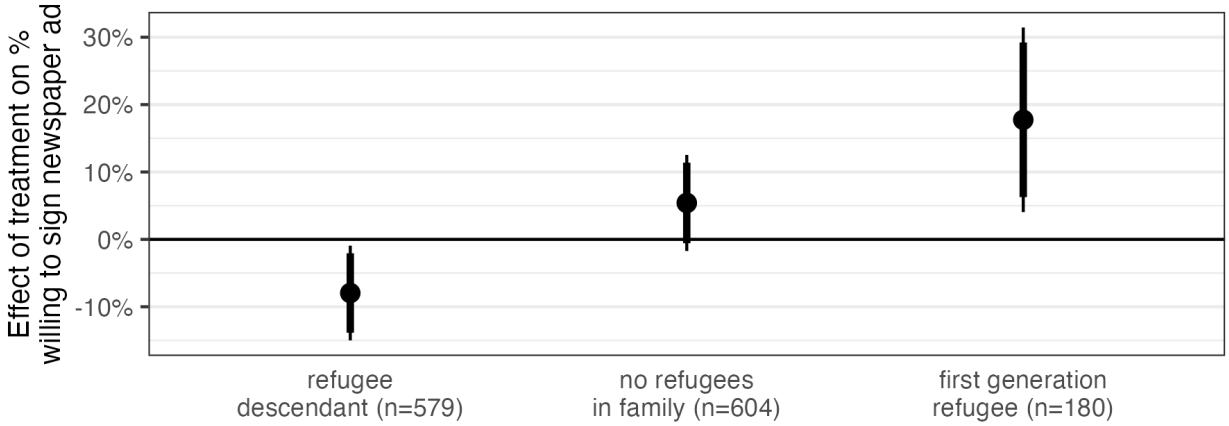

Figure 6: Net effect of treatment willingness to add one's name to a newspaper ad advocating better conditions for asylum seekers, split into subsamples according to refugee status. Estimates based on OLS regression with robust standard errors. CIs: 95% (outer bars) and 90% (inner bars).

tion from their national ingroup, and feel less solidarity with other refugees.

Different mechanisms could explain why the treatment might backfire. A "competitive victimization" mechanism could be activated, whereby descendants see contemporary refugees' tales of victimization as competing with their own for legitimacy (see Dinas et al. 2021b). To test this mechanism, we used an alternate wording of the treatment that focused on Cypriot suffering ("As Cypriots, we know what it means to lose everything from war") as a rationale for helping asylum-seekers. Thus, we remind Cypriots of their refugee heritage without drawing an explicit

comparison between the two crises (see Appendix G).[5]

4 Conclusion

Although previous studies have shown that small nudges such as reminders about the plight of one's ancestors can generate sympathy for marginalized groups, our replications illustrate the limits of such interventions (also see Turkoglu et al. 2022). In five studies, which differ in either temporal, cultural, or political context, connecting present-day refugee crises to historical ingroup displacements failed to generate greater support for refugees among the descendants of the displaced—the very group targeted by this intervention. Only in Cyprus do we see a positive effect, and even then, only among survivors of displacement themselves and not their offspring. This echoes similar results from Korea, where Hong et al. (2021) find that those who personally experienced displacement are more receptive to a salience treatment than those who have other forms of war victimization.

Contextual differences, differences in relative group status of the descendants of displaced compared to refugees (Meiske 2023) or wording of the treatment may in part explain why our findings are incongruous with Dinas et al. (2021a) or Hong et al. (2021) or Williamson et al. (2021). We discuss contextual differences in detail in Appendix A6, and leave it for future work to identify the precise conditions in which salience treatments can generate positive vs negative effects. The point we wish to make here is broader: If a major goal of prejudice-reduction research is to develop strategies to sway public opinion, then light-touch messaging interventions seem particularly fraught. If drawing a slightly stronger or weaker parallel to a nation's own political history affects the sensitivity of results of an experimental intervention, or if the intervention works when it does not draw an explicit parallel and backfires when it does in some countries but not

[5]This is similar to what Hong et al. does in their replication of Dinas et al. in Korea. Unlike Hong et al., we find that the effects of this "acknowledgment of suffering" treatment are indistinguishable from those of our standard salience treatment, though they are also less precise.

others, then the intervention will be hard to scale up and use effectively across contexts. There is, as of yet, no "best practices" toolbox that can reliably work across cultures.

In our study, we find suggestive evidence that the "salience treatment" works among older Cypriots who personally experienced displacement in 1974. However, the same intervention makes their offspring more hostile to contemporary refugees from other countries. This divergence is interesting and unexpected; it is also not predicted in prior theory. Given the inconsistencies we found across contexts and sub-populations, it is not clear what drives this effect, which deserves further study. Our findings overall suggest that analogic perspective-taking based on these "light-touch" interventions that amount to priming one's pre-existing feelings and experiences is too unpredictable to be useful as a tool for building outgroup support without substantial piloting in each new time and place where it is to be deployed.

Scaling up to achieve durable mass-level effects is complex (Kalla 2018) and reliable–and durable–effects might require longer interventions that involve the sustained exchange of ideas and opinions (Majumdar et al. 2022), non-judgemental conversation and listening of others' perspective (Kalla and Broockman 2020), or appropriate schooling to overcome prejudice at an early age (Alan et al. 2021; Weiss et al. 2023). To translate basic research into actionable policies and campaigns, researchers should not only test the portability of existing treatments across contexts, as we have done here, but also look for ways to deliver those treatments efficiently at scale. Interventions that rely on mass communication to propagate effects are inherently risky in that even the initial interpretation of the message takes place beyond the reach of a trained door-knocker who can steer the cognitive processing in a productive direction. Yet there are encouraging results. After finding that a non-judgemental exchange of narratives can reduce prejudice through door-to-door canvassing, Kalla and Broockman (2020) discovered that phone calls could achieve similar gains while requiring less time and cost. Similarly, Alan et al. (2021) took a successful model of perspective-taking and incorporated it into a school curriculum, demonstrating its potential to be rolled out nationwide. Weiss et al. (2023) show that educational modules that can be added to school curricula can offer ways to scale up interventions to reduce bias at an early age. These

approaches offer a compromise between costly door-to-door canvassing and the lack of control inherent in a mass media campaign. Ultimately, when it comes to reducing prejudice, there may be no easy shortcuts, and no substitute for sustained positive intergroup contact combined with long-term investments in civic education. There may also be a role for rapid cognitive interventions to complement and enhance the effects of deeper structural change.

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