

When Hearts Meet Minds: Complementary Effects of Perspective-Getting and Information on Refugee Inclusion*

Claire L. Adida[†] Adeline Lo[‡] Melina Platas[§]
Lauren Prather[¶] Scott Williamson^{||}

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Abstract

Perspective-getting and correcting misconceptions are two common interventions to promote inclusion toward outgroups. However, each strategy has limitations. Empirical work on information corrections yields inconclusive results, and empathy-based interventions may reproduce the biases they are meant to alleviate. We clarify the strengths and weaknesses of each strategy and offer a design to identify the conditions under which they are most effective. Using three studies on refugee inclusion with nearly 15,000 Americans over three years, we find that information and perspective-getting affect different outcomes. Perspective-getting affects warmth, policy preferences, and behavior, while information leads to factual updating only. We show that combining both interventions produces an additive effect on all outcomes, but neither strategy enhances the other. Bundling the strategies helps guard against potential backfire effects of information, however. Our results underscore the promise and limits of information and perspective-getting for promoting inclusion, highlighting the benefits of integrating the two strategies.

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[†]Professor of Political Science, UC San Diego.

[‡]Assistant Professor of Political Science, University of Wisconsin Madison.

[§]Assistant Professor of Political Science, NYU Abu Dhabi.

[¶]Associate Professor of Political Science, UC San Diego.

^{||}Assistant Professor of Social and Political Sciences, Bocconi University

Hostility toward refugees is a global phenomenon (Bansak, Hainmueller and Hangartner, 2016; Wike, Stokes and Simmons, 2016; Cowling, Anderson and Ferguson, 2019). In the United States, the American public has often expressed exclusionary attitudes toward refugees, even during humanitarian crises such as World War II or the more recent Syrian and Afghan conflicts (Pew Research Center, 2015; Hartig, 2018). These attitudes are frequently reflected in restrictive policies that seek to limit refugee admissions (Gibney, 2003; Hinnfors, Spehar and Bucken-Knapp, 2012). With forced displacement currently at historic highs (UNHCR, 2020), and conflict between refugee and host communities contributing to instability in a number of countries (Salehyan and Gleditsch, 2006; Fisk, 2018; Rügger, 2019, though see Lehmann and Masterson, 2020; Shaver and Zhou, 2021), researchers are seeking to understand why people oppose refugees and to identify strategies for strengthening acceptance of this vulnerable group (Bansak, Hainmueller and Hangartner, 2016; Adida, Lo and Platas, 2018*a*; Dinas et al., 2017; Williamson et al., 2021; Facchini, Margalit and Nakata, 2022). This research has most recently become acutely relevant to the U.S. refugee resettlement program, with the Department of State’s January 2023 launch of the Welcome Corps, a private refugee sponsorship initiative.

To date, this research has placed particular emphasis on two inclusionary strategies. The first involves correcting misconceptions about migrants. Existing research suggests that people often overestimate how much refugees or immigrants differ culturally from the host community, or the extent to which they contribute to crime, unemployment, and other negative social and economic outcomes (Alesina and Stantcheva, 2020). These misconceptions may contribute to hostile attitudes

by increasing perceived threats to host communities (Sides and Citrin, 2007), and scholars have evaluated whether information corrections can improve inclusion by reducing these perceived threats.

The second strategy leverages emotions through perspective-getting or perspective-taking exercises in which respondents reflect on the experiences of an outgroup member. Perspective-getting is designed to expose members of the ingroup to the experiences and feelings of an individual from the outgroup, while perspective-taking encourages participants to put themselves in the shoes of an outgroup member to consider what it would feel like to go through their experiences. These exercises can improve inclusion by increasing empathy for, reducing perceived social distance with, or changing attributions to the outgroup (Kalla and Broockman, 2020).

Yet both approaches have their limitations. Studies that seek to reduce hostility toward migrants by correcting misconceptions yield contradictory results, with some producing more inclusive views and policy preferences (Facchini, Margalit and Nakata, 2022), and others producing null or very small effects (Hopkins, Sides and Citrin, 2019). These limited effects reflect a broader literature on political misperceptions, which indicates that correcting information on a variety of topics typically produces only small reductions in misconceptions and even less updating of political attitudes; it may also occasionally backfire (Bursztyn and Yang, 2021; Chong and Druckman, 2007; Nyhan and Reifler, 2010). Meanwhile, studies relying on perspective-getting and perspective-taking exercises produce more reliably positive effects on inclusive attitudes (Adida, Lo and Platas, 2018*b*; Kalla and Broockman, 2020), but scholars have questioned their scope. Indeed, some have argued

that empathy-based interventions may reproduce or even exacerbate ingroup biases (Simas, Clifford and Kirkland, 2019; Bloom, 2016), making them effective only on groups to whom we already feel close.

This paper provides a blueprint for a more effective and comprehensive inclusion-ary approach. Our theoretical framework identifies the factors that can enhance or limit the effectiveness of information-correction and perspective-getting as inclusion-promoting strategies.¹ Information-correction may not work because the information provided may not be new, it may not be salient to exclusionary attitudes, or it may be rejected by individuals motivated to believe information that is more consistent with their priors. Perspective-getting may have limited effects because such interventions rely on activating empathy, which may work only on groups with whom we already feel kinship. We identify the criteria that are most likely to shape the effectiveness of each strategy, and then develop an argument for why combining the two might address each individual strategy's limitations: perspective-getting may attenuate an individual's motivation to resist new information; and information may correct the perception of dissimilarity that limits the effectiveness of empathy-based interventions.

Our research design involves a sequence of three studies on more than 15,000 individuals over the course of three years. We use the first two studies as intervention-builders: they help us design an information-correction intervention and a perspective-getting intervention that allow us to test the effectiveness and limita-

¹We draw from (Kalla and Broockman, 2020), who find that perspective-getting produces most reliable effects on inclusion. Our intervention in this paper is therefore a perspective-getting intervention.

tions of each strategy. They also identify a comprehensive set of outcomes that may be shaped differently by each intervention: belief updating, warmth toward refugees, preferences toward a policy of increasing the refugee cap, and political behavior in support of refugees. The final study then provides a comprehensive test of our individual and combined interventions on each of these four outcomes.

The first study identifies facts about refugees about which Americans are most misinformed, revealing that Americans know little about the refugee vetting process even though such knowledge is salient to their policy preferences: respondents who believe that refugees experience low levels of vetting are less likely to support open policies toward refugees than those who believe vetting is more extensive. This first study allows us to identify the piece of information most likely to be new and salient to individual preferences toward refugees and refugee policy. Having identified a salient piece of misinformation about refugee policy, we use Study 2 to pilot our interventions and outcome measures. In particular, the study identifies a hard case for empathy activation: a narrative about a Muslim refugee from Somalia. If indeed empathy-based interventions work only for groups with whom we already feel close, then ours is a hard case for an effective perspective-getting intervention.

In study 3, we provide the core test of our argument about the individual and combined effects of these strategies. We embed an experiment in a survey of more than 9,000 Americans. The experiment provides participants our information and perspective-getting interventions identified in the first two studies individually and, in one arm, combined. Specifically, some respondents are randomly assigned to receive accurate information about the vetting process, and others to receive the same

accurate information, but embedded in a perspective-getting narrative designed to activate empathy toward refugees. A third group receives the refugee narrative only, and a control group proceeds directly to the outcome questions about information updating, warmth toward refugees, and pro-refugee policy preferences and behaviors.

Our results are three-fold. First, we find that both information and perspective-getting interventions affect the outcomes under study, but not uniformly. The information treatment affected only factual updating positively, it had no effect on warmth toward refugees, or pro-refugee behavior, and it backfired by reducing support for pro-refugee policy. By contrast, perspective-getting on its own produced limited effects on updating misperceptions, but did generate increases for the other three outcomes. Second, the combined treatment affected all outcomes, pushing respondents toward greater inclusion, but there was no beneficial interactive effect: we find no evidence that the two strategies enhance each other. Finally, we find some evidence that bundling the two strategies may guard against the potential backfire from information treatments.

Our paper joins a growing literature investigating the effectiveness of correcting misconceptions and perspective-getting narratives on outgroup inclusion, while clarifying the contours of their impact. A combined intervention successfully shifts all four inclusionary outcomes, revealing that these two strategies can work as complements to improve outgroup inclusion. Yet, we show the limits of these interventions. Information-correcting interventions improve misconceptions without shifting warmth, policy preferences, or behavior in an inclusionary direction. Perspective-getting shifts individuals' warmth, policy preferences, and behavior toward outgroups

but has limited effects on accurate updating of misconceptions. And neither improves the effect of the other, although we have suggestive evidence that embedding correcting information in a perspective-getting narrative may act as a protective shield against backfire effects.

1 Information, Emotion, and Prejudice Reduction

Social scientists seeking to understand what shapes public attitudes and behavior toward outgroups, including refugees in particular, have placed particular emphasis on two strands of inquiry: the first focuses on the role that information - and misconceptions - play in sustaining or alleviating exclusionary attitudes, and the second looks instead to the role of narratives or empathy-inducing exercises. Below, we identify the contributions and limitations of each approach.

1.1 Correcting Misperceptions

Do individuals exclude others because of misconceptions they hold about the social group(s) to which they belong? A number of interventions aim to correct misperceptions that outgroups pose economic, cultural, or even security threats. Misconceptions about social outgroups are common, and these misconceptions correlate with negative views of outgroups (Abrajano and Lajevardi, 2021). For example, Americans overestimate how many Black Americans receive benefits from welfare (Delaney and Edwards-Levy, 2018), overestimate the percent of crimes committed by Black and Hispanic Americans (Ghandnoosh, 2014), and incorrectly think that Muslim-

Americans are more likely to support violence against civilians (Williamson, 2020). Immigrants are typically perceived to be more culturally different, poorer, less educated, and less employed than they actually are (Alesina, Miano and Stantcheva, 2018).

Such misconceptions are not innocuous: they are usually correlated with negative attitudes toward the outgroup in question. For example, individuals in the United States and Western Europe overestimate the size of their country’s immigrant population (Alesina, Miano and Stantcheva, 2018; Citrin and Sides, 2008). Such misconceptions may increase exclusion by contributing to heightened perceptions that the outgroup threatens the ingroup (Fisk, 2018; Quillian, 1995; Stephan and Stephan, 2000). Indeed, we know that Americans who overestimate the size of minority groups demonstrate more negative attitudes toward these groups, whether Muslims, immigrants, Blacks, or Hispanics (Alba, Rumbaut and Marotz, 2005).

Politicians and pundits actively promote misconceptions and misinformation for political purposes. Regarding refugees, for instance, Republican Senator Ted Cruz argued that the vetting of refugees was insufficient and “an invitation to terrorist attacks here in the United States,” (Ryan Tillman, 2021) following America’s pulling out of Afghanistan. Former President Trump advocated for Executive Order 13769 “Protecting the Nation from Foreign Terrorist Entry into the United States” by claiming that it would “keep radical Islamic terrorists out” by establishing “extreme vetting” of foreign entrants (Siddiqui, 2017). This order banned entry into the United States of people from a number of Muslim-majority countries, in addition to suspending all refugee admissions for 120 days and ending admittance of refugees

fleeing from the Syrian Civil War.² In 2016 and 2017 a Pew Survey found that around half of Americans viewed refugees as a major threat to the well-being of the United States.³ In fact, refugees have been involved in no terrorist attacks on American soil and are subjected to extensive vetting by multiple US government agencies.

In response to such misconceptions, refugee advocates often attempt to provide corrective information intended to mitigate perceptions of refugees as threatening to national security, or the economy.⁴ However the efficacy of these types of campaigns on increasing inclusion remains an open question (Adida et al., 2021). Studies that have tried to correct specific pieces of information about outgroups have generated mixed results. In two experimental studies providing statistical facts about the size and characteristics of the immigrant population in the United States, Grigorieff et al. (2016) find that individuals do update their beliefs, as well as their policy preferences, accordingly. On the other hand, in seven separate survey experiments conducted over more than a decade, Hopkins et al. (2018) find largely null effects of correcting information on attitudes toward immigrants. Adida et al. (2018*b*) also find that providing information about the number of Syrian refugees admitted to the United States did not change sentiments or behaviors toward this group. Even more concerning, some studies have found that providing correcting information may actually generate backlash (Nyhan and Reifler, 2010).

There are several reasons why individuals may not update their factual beliefs

²This EO was blocked by a number of courts, and eventually superseded by EO 13780 and revised by presidential proclamations. Its most recent iteration (February 2020) has been upheld by the United States Supreme Court on June 26, 2018.

³Pew Research Center, January, 2017, “The World Facing Trump: Public Sees ISIS, Cyberattacks, North Korea as Top Threats.”

⁴For examples, see: Migration Policy Lab, RCUSA.

or attitudes in response to information. First, in order for information to result in factual updating, the information must be new (the respondent did not already hold accurate factual beliefs). Second, even if information does lead to the updating of factual beliefs, it may not necessarily lead to changes in attitudes or policy preferences. Recent research on correcting misconceptions suggests that people typically do update their factual beliefs after receiving corrected information, but without necessarily changing related political and ideological views (Bursztyn and Yang, 2021; Nyhan, 2021; Porter and Wood, 2021). One reason for this disconnect is that the factual beliefs being corrected may not be relevant to attitude and policy preference formation. For example, if the size of an outgroup locally is more important for attitude formation than the size of the outgroup nationally, providing information on the latter is unlikely to affect attitudes toward the outgroup. As another example, if someone opposes abortion on religious rather than public health grounds, providing information that abortion bans increase unsafe abortions or are detrimental to women’s health is unlikely to affect their position on abortion. In both of these examples, the information may be both new and related to the topic at hand, but may not play a causal role in preference formation.

Third, even if information addresses beliefs that are relevant to preference formation, an individual may *resist* updating their attitudes in response to the corrected information. This resistance may occur because the individual is motivated to interpret the information in a way that aligns with their existing sentiments, policy attitudes, and political behaviors (Bolsen, Druckman and Cook, 2014), or because the individual holds strong priors based on previously-consumed information, which

causes them to update less (Little, 2022).

Finally, causality may run in the opposite direction. The individual who holds negative attitudes toward Muslims may justify this with the belief that Muslims engage in terrorist activity. Changing this belief with corrective information will not affect the individual’s attitudes, because this belief is a product not a predictor of the exclusionary attitude (Nyhan, 2021).

Acknowledging the many ways in which information can fail to change beliefs and attitudes, in this study we select a piece of information we know to be both new and relevant to preference formation. Specifically, we conduct an initial survey to assess common misconceptions about refugees and find that Americans tend to underestimate the amount of security vetting refugees undergo. As discussed above, the vetting process is one that politicians have frequently highlighted as a source of concern, and we also find that beliefs about vetting correlate with policy preferences. Thus, this piece of information is a “best case” scenario for testing the effect of information on updating and attitude change.

1.2 Empathy for Outgroups

A second strategy for reducing exclusion focuses on emotion, aiming to promote empathy for outgroups through perspective-getting or perspective-taking exercises. A perspective-getting exercise is designed to expose members of the ingroup to the experiences and feelings of an individual from the outgroup. Perspective-getting is closely related to perspective-taking, in which people are encouraged to put themselves in the shoes of someone else to consider their views. These exercises may reduce

prejudice by activating empathy, including concern or sympathy directed toward the outgroup, as well as feeling what it would be like to go through the outgroup’s experiences. These exercises may also reduce exclusionary attitudes and behaviors by lowering the perceived social distance with the outgroup and shifting attributional thinking (Kalla and Broockman, 2020).

Both perspective-getting and perspective-taking are among the tools used by refugee advocates to encourage support of refugees. Perspective-getting campaigns include UNHCR’s “See refugees through new eyes,” a video campaign in Bulgaria that shows the experience of a refugee trying to settle in a new country⁵ and *Clouds over Sidra*, which is a virtual reality tour of a Syrian refugee camp in Jordan.⁶

Substantial evidence in the social sciences indicates that perspective-getting and perspective-taking can be effective at reducing exclusionary attitudes, while shifting policy preferences and behaviors in a more inclusive direction (Adida, Lo and Platas, 2018*b*; Broockman and Kalla, 2016; Kalla and Broockman, 2020; Williamson et al., 2021). But others have also argued that empathy-based interventions may exacerbate polarization (Simas, Clifford and Kirkland, 2019) and ingroup bias (Bloom, 2016). This work suggests that a limitation to empathy-based interventions is that empathy is easier to experience for groups or individuals with whom we already feel kinship. Simas et al. (2019), for example, find that individuals who score higher on an index of empathic concern more strongly favor their own political party relative to the other political party. The correlation is significant both for measures of ingroup favorability (positive) and outgroup favorability (negative). Similarly, Bloom (2016) argues that

⁵UNHCR, See refugees through new eyes.

⁶See <https://www.with.in/watch/clouds-over-sidra/>.

it is not possible to empathize with everyone. Because empathy takes cognitive and emotional effort (Cameron et al., 2019), it is natural for individuals to favor empathizing with some over others. The implication of this *empathy bias* (Fowler, Law and Gaesser, 2021) is that empathy-based interventions such as perspective-getting or perspective-taking risk reproducing the biases that divide us.

In our study, we build a perspective-getting exercise with a “hard-test” narrative about a Muslim refugee from Somalia. By doing so, we explicitly test whether empathy-based interventions work when the protagonist is culturally distant from most Americans. Additionally, we bundle perspective-getting with information-correction to test whether correcting a common misconception about the refugee vetting process might attenuate empathy bias by changing how dissimilar individuals believe refugees to be from them. Social scientists have shown that cultural proximity and kinship are also social constructs (Adida, 2014; Laitin, 1986); if we successfully correct people’s perceptions that refugees are a security threat, do we make it easier for them to absorb an empathy-based intervention?

1.3 Individual, additive, and interactive effects

The above sections help us understand what we know and do not yet know about the effectiveness of inclusionary strategies. In this section, we describe two factors that help us understand how and why these strategies might work.

First, we propose a research design that brings together these two interventions and explicitly tests for interaction effects between them. Specifically, we aim to test the effects of embedding information about the refugee vetting process *within* a

perspective-getting exercise that delivers a human-centered narrative about a refugee. There are a number of reasons why pairing the two strategies will help us understand the ways and extent to which each strategy *works*.

First, perspective-getting may improve the uptake of new information through two possible mechanisms, one emotional and the other cognitive.⁷ Perspective-getting exercises have been tied to directly promoting open-mindedness in educational contexts (Southworth, 2021, 2022). They may further spur emotions such as empathy that open one up to integrating new information or allow for softening of previously held beliefs (Morisi and Wagner, 2020). When individuals encounter information that conflicts with their priors or their attitudes about an outgroup, they may experience an emotional reaction that leads them to resist incorporating that information. This process may happen even subconsciously such that individuals never think deeply about the new information and instead reject it before considering it. By creating more openness toward new information, empathy generated by perspective-getting exercises may be able to counteract this typical emotional response, making it more likely that an individual responds to the information by updating their beliefs.

Previous research also shows that individuals who engage in a more complex cognitive task are less likely to rely on out-group stereotyping (Galinsky and Moskowitz, 2000; Todd, Galinsky and Bodenhausen, 2012). As we detailed above, the so-called

⁷Our work has some similarities to literature in the health sciences that emphasizes the usage of narratives to introduce information for health-promoting behaviors (Dahlstrom, 2014, as an example), which notes that obstacles of numeracy and relatability can make it harder for “information-alone” campaigns to change behaviors in ways that narrative-based interventions suffer less from (Perrier and Martin Ginis, 2018), or empathy for others experiencing health vulnerabilities (Moyer-Gusé and Nabi, 2010).

“hot” cognitive pathway largely bypasses deep cognitive thinking. When presented with new information on its own, this hot pathway may be the only information processing that is triggered. However, when information is paired with perspective-getting, individuals are set in a “cold” cognitive pathway, carefully considering the perspective they are receiving. Information presented in this context can be more deeply considered and may therefore be more likely to lead to updating.

At the same time, correcting information may improve the effect of perspective-getting by alleviating empathy bias. Scholars across the social sciences have warned against empathy as a foundation for morality (Fowler, Law and Gaesser, 2021) or policymaking (Bloom, 2016), because – they argue – we tend to feel empathy more readily for people with whom we already feel kinship. If this is true, then empathy-based decisions are likely to reproduce the biases that divide us (Simas, Clifford and Kirkland, 2019). Yet we also know that identity groups are social constructs (Laitin, 1986), and that cultural proximity can be fluid and endogenous (Adida, 2014; Adida and Robinson, 2022). By embedding information about the lack of security threat posed by refugees into our perspective-getting exercise, we can test whether perspective-getting becomes more effective when combined with information that reduces an important perceived difference between refugees and Americans.

The second feature of our research design that allows us to identify the contours of each inclusionary strategy, is the testing and implementation of a comprehensive set of outcome indicators: belief updating, warmth, policy preference, and behavior. Existing studies tend to focus on only one or a subset of outcomes. Testing for effects on a richer set allows us to identify which outcome each strategy moves independently,

and whether a combined intervention is more likely to shape certain outcomes over others. We expect that the combination of information and perspective-getting will affect both belief updating and inclusionary attitudes. However, whether these effects work separately or jointly is not clear *ex ante*.

1.4 Observable Implications

The argument we laid out above leads to a number of observable implications about the effectiveness of each individual strategy relative to a control, but also about the effectiveness of the combined treatment relative to each individual treatment. Below, we present our hypotheses.⁸

The studies include four outcome variables: belief updating, warmth, policy preference, and behavior. Before measuring these outcomes, participants in our experiment are divided into four groups: control, information alone (Info), perspective-getting alone (PG), and information embedded in a perspective-getting exercise (PG-Info).

First, we expect minimal effect of correcting information on inclusionary outcomes. Providing new and salient information should result in belief updating, but we expect that individuals' motivated resistance to information challenging their priors will limit the effect of information on other measures of inclusion.

H1: Info increases belief updating relative to the control, but it does not affect any other outcome (warmth, policy preference, behavior) relative

⁸Hypotheses are preregistered in our pre-analysis plan unless otherwise noted, available at XXXXXXXXXX. In this PAP, we organized our hypotheses by outcome; here, we organize them by intervention-type.

to the control.

Second, we expect a positive effect of perspective-getting on warmth and inclusionary behavior, as predicted by the existing literature. However, we do not expect that perspective-getting on its own will change belief updating (since we provide no actual information in that intervention) or policy preferences, which may be sticky and particularly difficult to shift.

H2: PG increases warmth and inclusionary behavior relative to the control, but it does not affect belief updating or policy preference relative to the control.

Finally, we expect that the combined treatment will have greater effects on all outcomes relative to either independent treatment.⁹

H3: PG-Info increases belief updating, warmth, inclusionary policy preference, and inclusionary behavior relative to either PG or Info.

2 Research Design

Our research design allows us to test the independent effects of the two common strategies to inclusionary strategies outlined above, to examine whether their combination leads to additive or interactive effects, and to identify the full set of outcomes our strategies shape. To do so we require a piece of information about refugees

⁹In our PAP, we were agnostic about whether or not this would occur, recognizing that this is truly an empirical question.

that is both new and salient to refugee attitudes, an identification strategy that allows us to causally estimate the independent effects of correcting information and perspective-getting, and a comprehensive set of outcomes capturing belief updating, warmth, policy preferences, and behavior. Our empirical investigation relies on three separate studies: the first identifies new and salient information in the refugee vetting process; the second identifies a hard test for perspective-getting; and the third implements a survey experiment to assess the effectiveness of our independent and combined interventions.

We use public opinion data from three surveys of American adults to achieve our empirical objectives. Each survey recruited large national samples of respondents via Lucid. These samples are representative on several key demographic characteristics. The first survey, with a sample size of 3,840 respondents, was conducted in the fall of 2019, and intended to provide a baseline measure of Americans' knowledge about refugee populations and refugee policy in the United States. A second survey, administered on 2,011 respondents in the spring of 2021, served as a pilot in which we tested several versions of the treatment. The third survey, with a sample size of 9,407 respondents, was implemented in the fall of 2021, using results from the first and second survey to design an intervention intended to counter the misconceptions identified in the 2019 survey and reduce negative attitudes toward refugees through a hard test of a perspective-getting exercise. We discuss each study in turn.¹⁰

¹⁰In each study, we chose a pure control rather than a placebo, because there is no obvious placebo option that we were confident would not also shift attitudes or convey information.

3 Study 1: designing the information-correction intervention

The purpose of the first study was to assess the American public’s general knowledge with regard to the country’s refugee population and refugee policies. This purely descriptive endeavor enabled us to identify the pieces of information most Americans already knew about refugees, and the ones most Americans missed. We tested Americans’ knowledge about the most common sources of misperceptions about refugees - misperceptions that have been weaponized politically to shape public preferences toward migrants and migration policy. We asked respondents to provide their best guesses about the number of refugees admitted in the year prior (addressing the common trope that the country is overrun by refugees), the demographic characteristics of the refugee population, their country of origin, and their language abilities (addressing the common trope that refugees cannot assimilate culturally), whether refugees pay taxes (addressing the common trope that refugees do not contribute to society), and the extent of US government vetting of refugees as well as the frequency of refugees’ involvement in terrorist and criminal activities (addressing the increasingly common trope that refugees threaten US security).

Table 1 below summarizes the average American conception about each of the above criteria (under the “Prior” column), comparing it to factual information (under the “Actual” column).¹¹ Contrary to the widespread misconceptions the public holds

¹¹Sources used to determine the factual answer are as follows. The Migration Policy Institute provides the number of refugees admitted over time (<https://www.migrationpolicy.org/programs/data-hub/charts/us-refugee-resettlement>). The Department of Homeland Security Annual Flow Report provides the percent of refugees who are women and the per-

about immigrants (Alesina, Miano and Stantcheva, 2018), our 2019 survey suggests that Americans have a relatively accurate understanding of the refugee population.

Table 1: Prior Beliefs and Actual

Indicator	Prior	Actual
Refugees admitted 2018	20,000 (median)	22,491 (2019)
% women and children	59.42	70 (2015-2017)
% Muslim	28.26	16 (2019); 32 (2002-2016)
% Christian	32.32	80 (2019); 46 (2002-2016)
% speak English fluently	26.51	21.5 (2011-2015)
% speak some English	33.62	34.7 (2011-2015)
% speak no English	39.87	43.9 (2011-2015)
Refugees pay taxes	Not sure: 40% Yes: 34% No: 24%	Yes
% of terrorist activity	29.22	0
Months of vetting	6-12	18-24
Equal crime likelihood (rel. US citizens)	50% equal	Equal likelihood
Origin countries	Mexico, Syria	DRC, Burma, Ukraine (2018); Burma, Iraq, Somalia (since 2002)

Yet there is one issue-area where respondents displayed significant misconceptions about refugees: the extent to which they represent a security threat. Although cent of refugees who are minors (https://www.dhs.gov/sites/default/files/publications/Refugees_Astylees_2017.pdf). The Pew Research Center provides the proportion of refugees who are Muslim vs. Christian (<https://www.pewresearch.org/fact-tank/2016/10/05/u-s-admits-record-number-of-muslim-refugees-in-2016/>). The Office of Refugee Resettlement’s 2016 Annual Report to Congress provides the English fluency of refugees (<https://www.acf.hhs.gov/orr/report/office-refugee-resettlement-annual-report-congress-2016>). Evans and Fitzgerald (2017) confirm, relying on the American Community Survey, that refugees pay taxes. The Migration Policy Institute provides information on refugee terrorist activity and on length of the vetting process (<https://www.migrationpolicy.org/news/refugee-resettlement-program-unsuitable-target>). Amuedo-Dorantes et al. (2018) provide evidence that refugees are no more or less likely to commit crimes relative to US citizens. Finally, the Pew Research Center provides the country of origin for most refugees to the United States (<https://www.pewresearch.org/fact-tank/2019/10/07/key-facts-about-refugees-to-the-u-s/>).

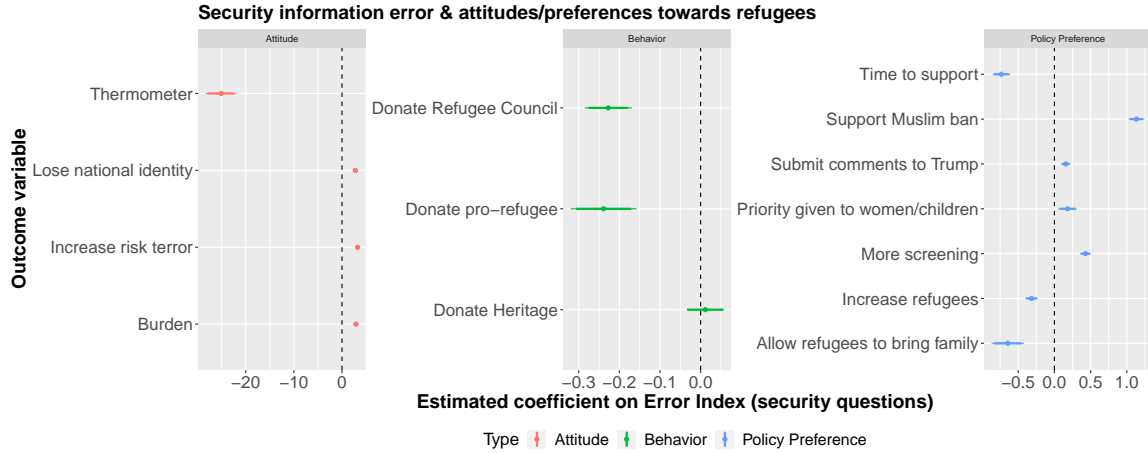
the evidence overwhelmingly confirms that refugees have committed close to 0% of domestic terrorist activity, our sample on average reported that refugees have committed one third of domestic terrorist activity in the US. Relatedly, Americans substantially under-estimate the amount of time dedicated to vetting refugees: the process typically takes 18-24 months, yet the modal respondent estimated that the process takes only 6-12 months. 60% of the sample gave answers well below 18-24 months, with almost 10% of respondents indicating that there is no vetting process at all. This misconception appears to be specific to refugees as a terrorist threat, rather than a criminal threat: when asked if refugees are less likely than, equally likely as, or more likely than US citizens to commit serious crimes, the modal respondent (50%) answered correctly that refugees and US citizens are equally likely to engage in serious criminal activity (Amuedo-Dorantes, Bansak and Pozo, 2018). We report these results graphically in SI-5.

Our 2019 survey also asked respondents about their feelings and attitudes about refugees, support and opposition to refugee policies, and whether they would engage in behaviors designed to support refugees. As a result, we are able to analyze whether these security-related misconceptions are significantly correlated with these outcomes. We use an “error index” as our explanatory variable. This is a (scaled) index of errors respondents have in the security threat knowledge variables (equally weighted). Here, larger values are equivalent to more error. The dependent variables are grouped into “attitudes”, “behaviors”, and “policy preferences”.¹²

Figure 1 illustrates the correlation coefficient between the error index and each

¹²These questions are described in more detail in the SI.

Figure 1: Correlation between information error and attitudes/preferences towards refugees.



Each point reflects the estimated coefficient of the error index on the corresponding outcome variable in a linear regression with controls for respondent gender, race, education, party, approval for Trump, state of residence, baseline empathy, and whether the respondent has immigration history in their family's first, second and third generations. Full regression results are reported in SI-6.

outcome variable in a multiple regression framework. Respondents who overestimated the terrorist activity of refugees and underestimated the vetting process were more likely to view refugees unfavorably, more likely to hold restrictive refugee policy preferences, and more likely to exhibit behaviors associated with opposition to refugees. It is not possible to determine the direction of causality, as some respondents may exaggerate their answers about the security threat posed by refugees because they already view them negatively for other reasons. Omitted variables may also explain the correlation: respondents may overestimate the security threat because of the media they consume, but other characteristics of this media may actually be driving opposition to refugees. However, the correlation documented suggests

that misconceptions about the security threat of refugees are salient to individuals' attitudes and behaviors toward refugees.

4 Study 2: piloting interventions and outcomes

We used our second survey to develop the interventions and outcomes for our main study. Drawing from previous literature, we settled on four outcome measures that capture different ways in which people may be more or less inclusionary toward refugees. First, other researchers have probed how well informed the public is about refugees and immigrants in the United States by asking numerical, factual questions about migration issues (Hopkins, Sides and Citrin, 2019). Respondents' knowledge – specifically pertaining to security – is relevant to our hypotheses about misperceptions. We therefore asked respondents how long it takes the US government to vet refugees, to ascertain whether they hold misconceptions about the security risks associated with refugees. Second, individuals may hold negative attitudes toward refugees themselves, disliking them as people. We follow others in using a feeling thermometer to gauge negative sentiment toward refugees as people (Alrababa'h et al., 2020). Third, feelings of warmth toward refugees may diverge from policy preferences, as the latter may be shaped by other inputs such as partisanship (Grigorieff, Roth and Ubfal, 2016). To assess our respondents' inclusionary attitudes toward refugee policy, we asked if the cap on the number of refugees admitted annually should be increased, decreased or kept the same. This question reflects a recurring political debate in the United States related to refugee policy. Finally, respondents' views

of refugee policy may be less relevant to political outcomes than their willingness to engage in pro-refugee political behaviors, particularly in the context of a survey. To gauge if our treatments could prompt political action on behalf of refugees, we followed Adida et al. (2018b) by asking if respondents would write a letter to the White House in support of their policy views.

We piloted several narratives for the perspective-getting intervention, with the goal of evaluating whether respondents reacted differently toward narratives highlighting various aspects of a refugee’s identity. The core narrative in our treatments focused on a refugee who fled Somalia and ended up in the midwest region of the United States. Given the politicization of anti-Muslim sentiment toward refugees since the Syrian Civil War and then during the Trump administration, we sought to examine whether respondents reacted differently to this perspective-getting narrative when the protagonist was described as a “refugee” or a “Muslim refugee.” The *Muslim refugee* narrative also included an additional sentence about the refugee’s difficulties explaining his religion once he arrived in the United States.¹³ In Table 2, we report how these two treatments affected responses toward the three inclusionary outcomes described above: the feeling thermometer, views of the refugee cap, and willingness to write a letter expressing their views of this policy.

The results suggest that the perspective-getting treatment is less effective in moving respondents’ preferences toward the refugee cap and willingness to write a letter of support when the narrative is about a *Muslim refugee* than when it is

¹³We also included a third, somewhat different perspective-getting treatment about a Muslim living in the United States that did not mention any refugee status or story, to test whether narratives about Muslims would prime thinking about refugees and therefore affect attitudes toward refugees. See the SI.

Table 2: *Effects of Perspective-Getting with Refugee vs. Muslim Refugee Treatments*

	(1)	(2)	(3)
	Thermometer	Refugee Cap	Letter Intent
Refugee Treatment	9.01*** (1.76)	0.48*** (0.13)	0.15*** (0.04)
Muslim Refugee Treatment	9.84*** (1.72)	0.24 [†] (0.13)	0.11** (0.04)
Constant	55.54*** (1.23)	-0.07 (0.09)	-0.07* (0.03)
Observations	1,517	1,522	1,519

[†]p<0.10; *p<0.05; **p<0.01; ***p<0.001

OLS regressions of outcomes on *Refugee* and *Muslim Refugee* treatments.

about a generic *refugee*. For the refugee cap, the magnitude of the *Muslim refugee* treatment is half that of the *refugee* treatment, and this difference is statistically significant at 0.10. For willingness to write a letter, the *Muslim refugee* treatment effect is 27 percent smaller than the *refugee* treatment effect, though the difference is not statistically significant. The effects are similar for the feeling thermometer, with the *Muslim refugee* treatment slightly larger. On balance, these results suggest that the “Muslim refugee” narrative provides a harder test of perspective-getting for an intervention targeting a sample of American adults. Since we are interested in probing the limits of perspective-getting interventions, we use this harder test for our third and final study.¹⁴

¹⁴The pilot also included an early version of the information treatment. However, this version included some information that was not directly related to the vetting process or security threats generally, making it more difficult for us to assess the effects of specific information meant to address an important misconception related to security threats. In addition, the perspective-getting narratives included elements that may have provided respondents with information about the vetting process, which made it difficult to untangle the effects of perspective-getting on its own. Piloting the treatments allowed us to identify and improve these shortcomings in Study 3, which we describe in the following section. For the sake of transparency, all pilot results are reported in the SI.

5 Study 3: information-correction and perspective-getting as individual and combined interventions

Our third study builds on the first two to test whether and how providing respondents with information, perspective-getting, or the combination of both shapes beliefs and attitudes about refugees. Respondents were randomly assigned to a control group or one of three treatment groups. The control group proceeded directly to the outcome questions (first tested in Study 2, and described below). The first treatment group was provided with information about the vetting process in a short paragraph citing government procedures, agencies involved, and average length of time. Respondents were also provided with a link to a US government infographic with additional information about vetting.

The second treatment group was provided with this same informational paragraph, but it was embedded within our hard test for perspective-getting: a narrative about a Muslim refugee admitted to the United States. This narrative was based on real-world stories we gathered from available US newspapers and we debriefed respondents on the fictional nature of the story at the conclusion of the survey – with links to the sources we used to construct the story. This narrative introduced the respondent to Abdi, a Muslim refugee from Somalia who spent 9 months in a refugee camp before going through the US vetting process. After being vetted, we describe Abdi as eventually settling down in the US. Abdi is quoted in the story expressing how much he desired to leave the camp.

Finally, the third treatment group read only Abdi’s story including his time spent in the refugee camp, but did not see any informational facts about the vetting process. To ensure that respondents were paying attention to the vignette, we asked all respondents in the treatment groups to briefly summarize how they felt about what they had just read prior to seeing the outcome questions. We report balance across the three treatment groups and control on a number of demographic variables and prior knowledge about refugee vetting in the SI.

We examine the effects of the treatment vignettes on the four main outcomes described above for study 2. The first outcome reflects respondents’ beliefs about the number of months of refugee-vetting. We ask this question both pre and post-treatment to measure the actual updating that occurs based on exposure to our treatments. We create two variables to measure how the treatments affect misperceptions. First, we code a dummy variable that captures whether respondents correctly identified the standard vetting period as 18 to 24 months in the post-experiment question. Second, we create a variable that subtracts the pre-experimental answer from the post-experimental answer to measure updating directly. The remaining dependent variables measure respondents’ attitudes and behaviors towards refugees. These questions all appeared after our treatment vignettes. They include the feeling thermometer ranging from 0-100 about refugees in the United States, the question about the refugee cap policy in the United States, and the question about respondent willingness to write a letter to the White House expressing their opinion on the refugee cap. The survey questions used to measure each of these outcomes can be found in Table 3.

Table 3: Main Outcome Variables

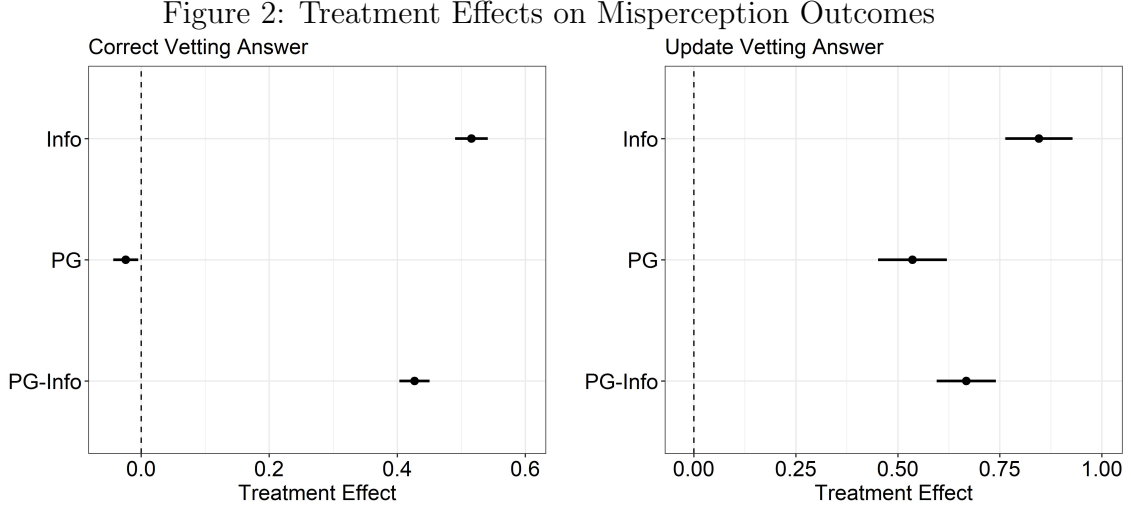
Type of Outcome	Survey question
Belief updating	“Approximately how many months of vetting does a refugee go through before being resettled into the United States? If you do not know, please give your best guess.”
Warmth	“On a scale from 0 to 100, where 0 = completely unfavorable and 100 = completely favorable, how do you feel about refugees who come to the United States?”
Policy Preference	“Every year the federal government decides how many refugees to allow to be resettled in the United States. The refugee cap was at its highest when the refugee resettlement program was first formalized in the United States in 1980, with a cap of 231,000. In the mid-1990s, it hovered around 100,000 to 150,000, but then decreased to the 70,000-80,000 range in the 2000s. Under President Obama, it was raised to 110,000, and under President Trump it was reduced to 15,000. This year the number of refugees permitted to enter the United States is 125,000. Next year, do you think the number should be higher, lower, or stay the same?”
Behavior	“Would you be willing to write a letter to the current president’s administration advocating for your position on how many refugees should be admitted into the United States?” (1) Yes, I would like to write a letter supporting an increase in refugees, (2) Yes, I would like to write a letter opposing an increase in refugees, (3) No, I would not like to write a letter.

6 Results

In this section, we present results for tests of our pre-registered hypotheses (unless otherwise stated). We regress the four outcomes on the treatment groups to test the hypotheses. In all cases, full regression results are displayed in the SI. The four experimental groups are referred to as *Info* for the information only treatment, *PG* for the perspective-getting only treatment, *PG-Info* for the information embedded

in the perspective-getting vignette treatment, and *Control* for the respondents who did not receive a vignette with information or the perspective-getting exercise.

We use two different measures of respondents’ willingness to update misconceptions about the refugee vetting process. The first is a dummy variable coded as 1 if respondents correctly answered the post-experimental vetting question by stating that refugees are typically vetted for 18 to 24 months. The second measure subtracts respondents’ pre-experimental answer from the post-experimental answer to the vetting question, indicating whether respondents updated their perceptions of how long the process takes. The treatment effects on both of these outcomes are shown below in Figure 2. Treatment effects on the inclusion outcomes – the feeling thermometer, attitudes toward the refugee cap, and willingness to write a letter to the White House about their refugee cap views – are reported in Figure 3.



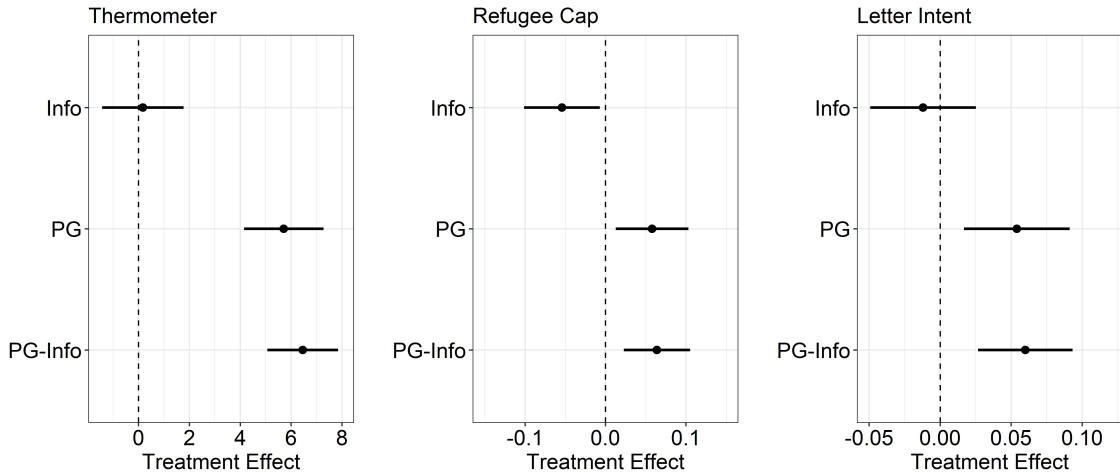
Note: Treatment effects on likelihood of answering correctly about the length of vetting (left) and updating vetting answer more accurately from pre to post (right). Full regression results are reported in SI-8. 95% c.i.

We discuss each of our hypotheses in turn. Our first hypothesis predicted limited effects of our information-only treatment. Indeed, we designed our vignette to provide information we knew to be new and salient for most Americans. As a result, we expect to find an effect on belief-updating. But the existing literature – emphasizing the role of motivated reasoning and the fact that beliefs may be consequences, not determinants, of attitudes – did not give us any reason to expect effects beyond that. The results reflect these expectations. As shown in Figure 2, the *Info* treatment increased correct answers to the vetting question by 50 percentage points relative to the control group. Likewise, the *Info* treatment substantially increased the likelihood that respondents updated from their pre-experimental answer to the post-experimental answer by choosing a longer period of time. However, as shown in Figure 3, the *Info* treatment did not strengthen inclusionary attitudes. The treatment produced a precisely estimated null effect on the thermometer outcome and the letter outcome, relative to the control group.

In addition, we note some evidence of a possible backfire effect of correcting information on policy preferences: individuals who received the correcting information were more likely to favor restrictive refugee policy than individuals in the control, and this effect is statistically significant at the 95% confidence level. Altogether, these results suggest that even when providing information that is new and seemingly relevant to Americans’ attitudes toward refugees, correcting misperceptions has limited inclusive effects.

Our second hypothesis predicted effects of perspective-getting based on the main findings in this literature to date: even with a hard case for perspective-getting (one

Figure 3: Treatment Effects on Inclusion Outcomes



Note: Treatment effects on feeling thermometer (left), attitudes toward the refugee cap policy (center), and willingness to write a letter advocating for changes to the refugee cap (right). Full regression results are reported in SI-8. 95% c.i.

about a culturally-distant Muslim refugee), we expected to find inclusionary effects on Americans’ feelings of warmth toward refugees, as well as their willingness to write a letter to the president advocating for more inclusive refugee policy. We were less certain if perspective-getting would affect attitudes toward this more inclusive policy directly.

Our findings are consistent with this expectation, indicating wide-ranging effects of perspective-getting. As shown in Figure 3, we see a significant increase in responses to the feeling thermometer, with an average increase of six percentage points. This increase is somewhat larger than findings from similar studies, including Williamson et al. (2021). We also observe an increase in preferences for inclusionary refugee policy in the *PG* treatment group, which is similar in magnitude to the effect of canvassing on support for transgender laws in Broockman and Kalla (2016). Finally,

we observe an increase in expressed willingness to write a letter supporting these policy views, also similar in magnitude to the effect of perspective-getting on letter writing in Adida, Lo and Platas (2018b).

In sum, all inclusionary outcomes increase in the *PG* condition. On the other hand, as shown in Figure 2, the *PG* treatment does not increase the likelihood that respondents answer the vetting question correctly. The *PG* treatment does increase how many respondents update their answer to a longer time period, perhaps because Abdi references feeling stuck in the refugee camp. However, this effect is weaker than that of the *Info* treatment and is driven by a small number of respondents overcorrecting by choosing the longest-possible (and also incorrect) answer choice. The distributions of responses for this question in the different treatment groups can be seen in the SI. These results on the misperception outcomes are unsurprising given that our perspective-getting treatment provided no factual information about the refugee vetting process.

Altogether, these results provide compelling evidence of the effectiveness of perspective-getting on changing attitudes toward an out-group, insofar as we designed this intervention as a hard test. Skeptics of empathy-based interventions argue that empathy bias means that individuals will feel and show empathy more readily toward those with whom they feel close. Nonetheless, here we find substantively large and statistically significant effects among a representative sample of American adults even when the subject of the perspective-getting narrative is described as a Muslim refugee.

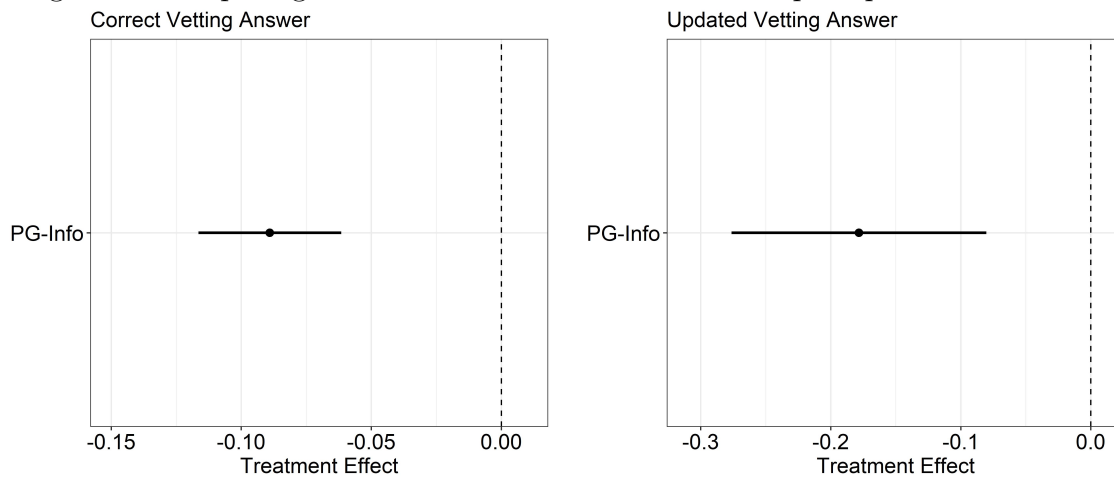
Finally, our third hypothesis asked whether combining information-correction

with perspective-getting could enhance the effectiveness of each individual strategy. We proposed that perspective-getting might open people up to accepting new information, while new information might alleviate the potential for empathy bias. If so, then we would expect our combined intervention *PG-Info* to generate increases in all four outcomes above and beyond the effects of each individual intervention relative to *Control*.

The coefficients for the *PG-Info* treatment in Figures 2 and 3 demonstrate a statistically significant effect on all four outcomes. With the incorporation of the vetting information into the perspective-getting vignette, respondents became substantially more likely to answer the vetting question correctly and to update their vetting answer from their pre-experimental to post-experimental responses. Likewise, the combined treatment produced warmer attitudes on the feeling thermometer and increased willingness to write a letter to the president supporting a more inclusive refugee policy. Importantly, the negative effect of information-only on respondents' policy preference also disappeared: when the information is delivered as part of a perspective-getting narrative, the effect is inclusionary.

Are these effects from the *PG-Info* treatment interactive? In other words, does this combined treatment work above and beyond the effects of *Info* and *PG* individually? In Figure 4, we show the effects of *PG-Info* relative to responses in the *Info* treatment group. Respondents in the combined treatment group are slightly less likely to answer the vetting question correctly or to update the vetting answer, suggesting that the information is delivered somewhat more effectively on its own. However, recall that the *PG-Info* treatment still improved accuracy relative to the

Figure 4: Comparing Effects of Info and PG-Info on Misperception Outcomes



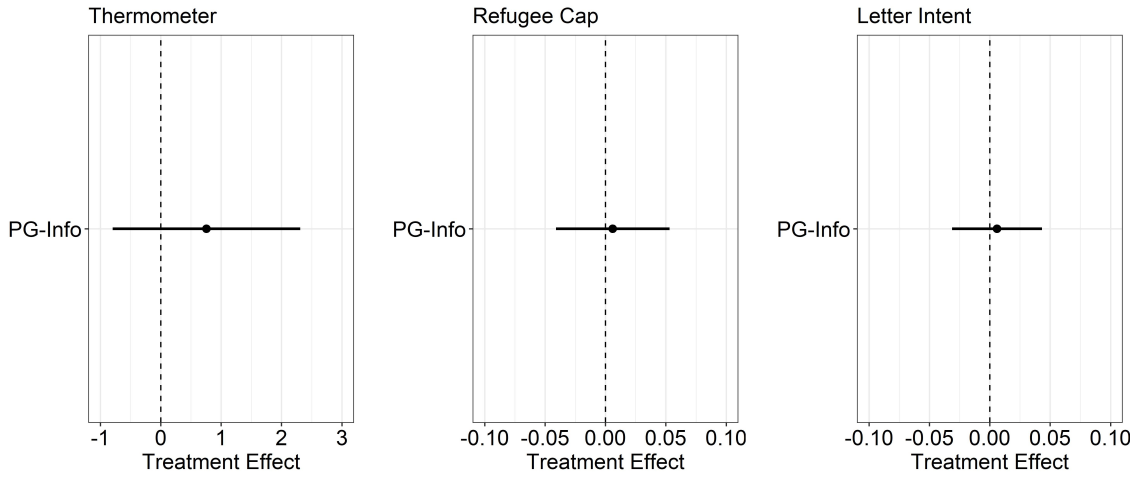
Note: Effect of PG-Info compared to Info on answering correctly about the length of vetting (left) and updating vetting answer more accurately from pre to post (right). Full regression results are reported in SI-8. 95% c.i.

control group as well as the *PG* treatment group that did not receive the factual information.

In Figure 5, we compare the effects of the combined *PG-Info* treatment on the inclusion outcomes relative to the *PG* treatment. Here we see that the combined treatment modestly out-performed the *PG* treatment in generating warmth toward refugees, but the difference is not statistically significant. Attitudes toward the more inclusive refugee policy and willingness to write a letter supporting that policy were nearly identical in the two groups. Thus, combining information with perspective-getting does not erode the effectiveness of the latter, and if anything may modestly improve it.

Together, these results suggest the combined treatment offers additive but not interactive benefits. This strategy affects all four outcomes, reducing misperceptions

Figure 5: Comparing Effects of PG and PG-Info on Inclusion Outcomes



Note: Effect of PG-Info compared to PG on feeling thermometer (left), attitudes toward the refugee cap policy (center), and willingness to write a letter advocating for changes to the refugee cap (right). Full regression results are reported in SI-8. 95% c.i.

about refugees while also increasing inclusionary attitudes. However, combining the two treatments does not enhance the effect of each individual strategy as we thought could be the case, and it marginally decreases the effectiveness of correcting information. The benefit of a combined strategy, therefore, is in its ability to shape a more comprehensive set of outcomes that advocates may care about. Though this evidence remains suggestive, the effects of the *PG-Info* treatment on the policy outcome also implies that incorporating information within a perspective-getting narrative can protect against potential backfire effects.

7 Discussion

Information and perspective-getting are common strategies that advocates use to increase inclusive attitudes toward refugees, as well as other outgroups. This article identifies the limitations of each strategy and offers arguments for why combining the two might offer a solution.

We argue that information-correction may not work for three reasons: the information provided may not be new, it may not be salient to outgroup attitudes, or it may be rejected by individuals motivated to believe information that is more consistent with their priors. We have further argued that perspective-getting may have limited effects because such interventions rely on activating empathy, which – scholars argue – may be easier to do for members of groups with whom we already feel close.

We then designed a set of survey experiments that address these challenges. We developed an information intervention that presents information we know to be new and salient for refugee attitudes. We developed a hard test for a perspective-getting intervention, relying on a narrative about a Muslim refugee – a group that is culturally distant from the average American. And we developed a combined information/perspective-getting intervention to: (1) improve the effect of information by reducing motivated resistance to new information and (2) improve the effect of perspective-getting with information that reduces the likelihood of empathy bias.

Our findings help us understand the extent and limit of each inclusionary strategy. First, we find that information correction does increase updating, but it has no effect on any other measure of refugee inclusion. Second, we find that even a

hard test of perspective-getting increases refugee inclusion. Third, we find that a combined intervention does not enhance the effectiveness of each individual strategy: the effects of information and perspective-getting combined are additive, not interactive. Fourth, we find that a combined intervention also does not substantially erode the effectiveness of either individual strategy. Providing information does not make perspective-getting any less effective at improving refugee attitudes and behaviors and may in fact counter backlash; and embedding the information in a refugee narrative modestly reduces updating but still generates large improvements in accuracy.

One potential limitation of the information treatment would be if respondents interpreted the lengthy vetting process to indicate bureaucratic incompetence rather than a thorough screening of security risks. We think this possibility is unlikely, since the treatment not only included information about the length of the process, but also incorporated information about how many US agencies participate in the vetting and the types of checks they perform. To further address this possibility, we analyzed respondents' written summaries of the treatment using topic models. Of the 1,901 respondents who received the *Info* treatment, only 14%, or 271 respondents, wrote nonsense in response to this prompt. And, as shown in the topics model in the SI, none of the most common topics appear to focus on bureaucratic incompetence.

A different but equally important concern is that our largely survey-based design captures what individuals claim to prefer in a survey, rather than more meaningful behavior such as voting or advocacy. In the SI, we offer two additional analyses that increase our confidence in the meaningfulness of our results. First, we test whether

respondents rushed through our survey and - importantly - were more likely to rush through our survey if given certain treatments rather than others. This analysis shows that, as expected, respondents who received longer treatments spent more time on the survey; but post-treatment, the length of time spent on each survey was statistically equivalent.¹⁵ Second, we manually recode each letter to discard nonsensical text and to make sure that respondents' actual letters were consistent with the sentiment they had claimed they wanted to convey.

This analysis, which we present in the SI, shows three key results. First, our results hold when we analyze treatment effects on our recoded variable (discarding non-sensical text). Second, we find relatively few instances of valence mismatch between the tone of the letter and the option respondents chose in the type of letter (positive or negative) they said they would write. Finally, although the frequency of nonsensical letters is not trivial (approximately 47%), writing nonsense is not significantly correlated with treatment. Together, these findings indicate that respondents were not rushing through the survey meaninglessly, a conclusion further reinforced by our topic model analyses in the SI, showing that the majority of letters included content relevant to the topic of refugee policy.

Our findings have implications within and beyond academia. First, we shed light on the complicated relationship between information and attitudes, revealing that individuals may update factual beliefs without shifting their policy or personal preferences. This finding reinforces research questioning the role of misinformation in

¹⁵Our evidence also casts doubt that our effects are driven by treatment intensity: our shortest treatment (*Info*) had the strongest effect on updating and no effect on inclusionary measures; our longest treatment (*PG-Info*) had stronger effects on inclusion but not on updating.

explaining exclusion. Second, we provide evidence of both the promise and limit of empathy-based interventions: on one hand, these interventions are effective even with a hard case (a Muslim refugee). On the other hand, perspective-getting did not enhance the effect of information-correction, suggesting that the effect of perspective-getting was not the result of openness to new information. Third, we find no backlash or unintended effect of combining the two strategies: while they do not enhance one another, they still complement each other. In fact, the marginal backfire effect of information-correction on policy preferences disappears when information is combined with a perspective-getting narrative.

This leads us to the key implication of our findings beyond academia. Refugee advocates use information-provision and narrative strategies to influence public opinion about refugees. Our study shows that combining the two strategies may help advocates achieve their goals, with no concern for backlash or unintended consequences: bundling information into a perspective-getting narrative may enable refugee advocates to simultaneously improve beliefs about and warmth toward refugees, as well as increase policy preferences and behavior that enhance refugee inclusion.

Taken together, our findings may provide evidence to suggest that exclusionary attitudes toward refugees, and perhaps Muslim refugees in particular, is taste based rather than statistical. Indeed, even providing information that was new and relevant to refugee attitudes yielded limited impact beyond factual updating. At the same time, bundling information-correction with a refugee narrative did not enhance the prejudice-reducing effects of perspective-getting. Finally, our findings cast doubt on the claim by empathy-skeptics that interventions meant to activate empathy will

suffer from bias because individuals are more likely to feel empathy toward those with whom they already feel kinship: even a narrative about a Muslim refugee from Somalia moved our respondents toward greater refugee inclusion.

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