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IN THEIR SHOES

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ABSTRACT

We explore the mechanics of empathy. In a controlled immersive virtual reality experiment, we show that neutral information on unauthorized immigration magnifies the empathetic response of subjects when they witness the struggles of unauthorized migrants. We conjecture that perceiving others as similar magnifies empathy: it makes it possible to live their experience as if one were 'in their shoes.' In a separate, incentivized experiment, we show that the same neutral information increases perceived similarity to unauthorized migrants. We provide similar evidence in observational data, showing that contact with a given foreign origin group induces a greater empathetic response – more charitable donations – after the country of origin of this group is hit by a natural disaster, and a higher perceived similarity to this group. Together, our evidence suggests that the ability to put oneself in the shoes of another person or group can be enhanced through standard policy tools such as neutral information provision and inter-group contact.

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Introduction

Following natural disasters, ethnic conflicts, wars, or economic crises, the circumstances of a group experiencing struggles often depend on the empathy from other groups who witness those struggles. Empathy may take the form of charitable donations, collective mobilization, and voting for policies to support the struggling group. How is empathy built? Why do different people witnessing the same episodes of struggles by others respond differently? Can the ability to put oneself in the shoes of another person or group be enhanced through standard policy tools?

We show that empathy can be enhanced: neutral information provision and inter-group contact magnify empathy, in two very different settings. We first show, in a controlled field experiment, that the increase in positive attitudes towards immigration, for subjects who have witnessed the plight of migrants crossing the US Southern border in an immersive virtual reality experience, is magnified if subjects have previously received neutral information about unauthorized immigration. This immersive virtual reality piece, called Carne y Arena® and created by Academy Award® winner director Alejandro González Iñárritu, is the closest to a real-life experience we could hope for.[1]

We then go one step further and shed light on a potential mechanism underlying this magnification of empathy. Guided by concepts from cognitive science, we conjecture that neutral information may magnify empathy because it increases the perceived similarity between subjects and out-groups: they experience the suffering of unauthorized immigrants as if they were 'in their shoes.' To test this hypothesis, we construct an incentivized measure of perceived similarity which can readily be applied to any pre-defined out-group. We then show, in a separate experiment, that the same information provided in the field experiment increases perceived similarity to unauthorized migrants.

These experimental results indicate the potential for building empathy toward out-groups through the combination of information provision and an emotional (immersive) experience, and suggest a mediating role of perceived similarity. To go beyond this experimental setting, we study a different, broader and naturally-occurring context with costly individual choices. We examine inter-group contact to foreign ancestries across US counties. Inter-group contact occurs naturally on a scale larger than any experiment could rival, and is a commonly-used policy tool, for instance, to desegregate schools, or to allocate refugees across cities. First, we show that the flow of charitable donations towards disaster stricken countries is magnified in US counties where residents are more likely to have been in contact with people from those countries, for plausibly exogenous reasons. Second, using large, newly-collected, national surveys, and leveraging the same incentivized measure of perceived similarity, we show that individuals in those US counties also feel more similar to those foreign origins.

[1] Carne y Arena received a special Academy Award, the only time such an award was given to a virtual reality piece.

Together, our experimental and observational evidence highlights how conventional policy interventions, such as neutral information provision and inter-group contact, can alter individual behavior by inducing more empathy towards out-groups.

We now turn to a detailed description of our findings.

We begin by building intuition with a simple conceptual framework, where we adapt from cognitive science the notions of perceived similarity and association between memories to analogous notions of perceived similarity and association between social groups. Consider a person (*self*) potentially feeling empathy toward a person belonging to another social group (*other*). We introduce an empathy term in the utility function of *self* that results from the interaction of two components. First, building on a long-standing hypothesis from social psychology, we assume that *self* will find it easier to simulate the experiences of *other* – put themselves 'in their shoes' – if *self* perceives *other* as more similar to them. We label this first component by S (perceived similarity). Second, conditional on perceiving *other* as similar, we assume that empathy is triggered when *self* witnesses salient events where *other* experiences struggles. This is intuitive: *self* is more likely to pay attention when the situation experienced by *other* is difficult and thus more salient. We label this second component by E (salient event). When the interaction of those two components is positive *self* puts a positive weight on the well-being of *other* and is willing to take actions favorable to them. The main insight from this conceptual framework is that through their interaction (S and E), perceived similarity (S) magnifies the empathetic response of *self* to witnessing the struggles of *other* (E). To bring this conceptual framework to the data, we identify plausible shifters to E and S and trace out their impact both on observable actions in favor of struggling others (empathy response) and on a measure credibly related to perceived similarity.

In a controlled field experiment, we recruit subjects ($n = 718$) whom we randomly confront with combinations of two treatments: the immersive virtual reality experience Carne y Arena from director Alejandro González Iñárritu showing the struggles of unauthorized migrants, and statistical information on unauthorized immigration. We measure the empathy response of participants with targeted charitable donations and policy views. Attitudes towards immigration improve by 70% (p-value < 0.01) when information precedes Carne y Arena, significantly more than the 32% increase (p-value < 0.01) of Carne y Arena alone. Our conceptual framework suggests that information magnifies empathy because it increases perceived similarity towards unauthorized migrants.

To test this hypothesis, we first show that the magnification of empathy induced by information happens only if information comes *before* Carne y Arena, not *after*: the change in attitudes in favor of immigration is statistically indistinguishable for participants who go through Carne y Arena alone, versus subjects who receive the information treatment *after* Carne y Arena (+32% versus +36%).

This is consistent with our hypothesis. If information comes *before* Carne y Arena, then subjects, induced to feel similar to migrants, live the traumatic experience of migrants as if they were 'in their shoes.' If information comes *after* Carne y Arena, they cannot 're-live' this experience with their newly acquired perspective on migrants. This further shows that our results are not driven by specific pieces of information that may, on their own, improve attitudes. We also confirm our results are not due to a composition of heterogeneous effects between different demographic groups, differential attention paid to information, nor distortions in attention to different types of information.[2]

To further test that information increases perceived similarity, we run a second large scale experiment ($n = 1,505$) where we randomly provide subjects with the same information on unauthorized immigration. We introduce a revealed-preference measure of perceived similarity. Subjects answer a personality test (McCrae and Costa, 1987). They are then incentivized to guess how many personality traits they share with an unauthorized migrant, from zero to five, which determines their perceived similarity score. We conjecture that this reflects the 'true object' of perceived similarity: if a subject believes they share many personality traits with an unauthorized migrant, it reveals that they feel similar to them on a deep, personal level. We show that information induces a shift in measured perceived similarity to unauthorized migrants (20% higher, p-value < 0.01).

We find suggestive evidence that for subjects who are ex ante more hostile to unauthorized migrants (more conservative compared to more liberal subjects), Carne y Arena alone has a weaker impact on attitudes towards immigration (+26% for conservatives versus +39% for liberals). However, the information treatment has a stronger impact on perceived similarity to migrants (+23% for conservatives versus +17% for liberals), so that the combined effect of information *before* Carne y Arena has an almost equally strong impact on attitudes (+58% for conservatives and +63% for liberals). This suggests that the ability to put oneself 'in the shoes' of others – enabled by shifting perceived similarity – is key to unlocking empathy, even for groups who would otherwise have a weak empathetic response. This ability can be enhanced through simple policy interventions such as information provision.

In a second empirical exercise, we turn to observational data. We start from the observation in Bursztyn et al. (2024) that the presence of communities of a specific foreign origin f in a US domestic county d causes more charitable donations from county d to country f when f is devastated by a natural disaster. Our conceptual framework suggests an explanation for this finding: if the presence in county d of people from foreign origin f induces residents in d to perceive themselves as more similar to people from f, then they will have a stronger empathetic response – donate more – when they witness the struggles of people in country f devastated by a natural disaster.

[2]The Carne y Arena treatment effect is also surprisingly persistent: attitudes are still 41% higher after two months.

To test this hypothesis, we run a new large-scale online survey ($n = 2,400$) across US counties. We collect information on contact with people of specific foreign origins (friend, neighbor, or co-worker from Haiti, Japan, or the Philippines),[3] and perceived similarity towards those foreign origins. We use the same revealed preference measure of perceived similarity as in our experimental exercise, applied to specific foreign origins instead of unauthorized migrants. To identify plausibly exogenous shifts in contact, we aggregate our measure of contact at the domestic county $d \times$ foreign country f level, leverage the bilateral structure of our data – with several counties and countries – to control for county and country fixed effects, and adopt the instrumental variable strategy using historical migration shocks of Burchardi et al. (2019) to predict inter-group contact.[4] We first extend to those three countries the findings in Bursztyn et al. (2024) that residents in counties where they are more likely to be in contact with a given foreign origin send more donations when that country is struck by a natural disaster.[5] A one percentage point increase in the predicted likelihood of contact with a specific foreign origin group increases the number of charitable donations flowing to that foreign country after a natural disaster by approximately 1.2% (p-value < 0.01). We then show that a one percentage point increase of the predicted likelihood of contact of residents in county d with foreign origin f induces an increase in perceived similarity of residents in d to foreign origin f of 1.3% (p-value < 0.01).[6] These findings are consistent with our hypothesis that inter-group contact magnifies empathy because it increases perceived similarity. It is the observational analog to our experimental finding that information magnifies empathy because it increases perceived similarity.

We do not attempt to directly manipulate perceived similarity, for two reasons. First, this is not feasible: we were allowed to provide neutral information to Carne y Arena visitors but not to manipulate them further; and for privacy reasons, the charitable organizations that graciously shared their data did not grant us permission to directly contact their pool of donors.[7] Second, our approach directly informs the efficacy of policy relevant interventions, neutral information provision and inter-group contact, instead of engineered manipulations which may be hard to replicate at scale.

Finally, as a proof-of-concept showcasing the versatility of our perceived similarity measure, we use multi-dimensional scaling techniques to construct a 'spatial' representation of perceived similarities

[3] We select those three countries because they were devastated by terrible natural disasters over 2010-17, attracted substantial numbers of charitable donations over that period, and are ethnically and culturally distinct.

[4] We describe how we adapt the identification from Burchardi et al. (2019) to our empirical setting in section 3.

[5] Bursztyn et al. (2024) collect data on contact with a single foreign origin group, Arab-Muslims. With data on three different foreign origins, we are further able to control for county fixed effects when predicting contact.

[6] Our results are robust to controlling for individual observables, and even for individual fixed effects when feasible.

[7] Direct manipulation also faces obvious limitations in this case: not knowing ex-ante where natural disasters will strike, we would need to manipulate the perceived similarity of potential donors for as many foreign countries as possible, and then wait for a disaster to occur in one of them to measure donors' empathetic responses. Any alternative experimental intervention would lose the external validity of our setting, with real-life donors, endogenously deciding to donate part of their income following naturally occurring natural disasters.

not just between subjects and foreign countries, but also between foreign countries.[8] We show that more liberal respondents live in a 'smaller world' than more conservative respondents: they perceive foreign countries as closer to themselves, as well as closer to each other.

Related literature. We contribute to three separate strands of the literature. First, in cognitive science, our paper relates to a growing theoretical and experimental literature that studies salience (Bordalo et al., 2013), attention (Bordalo et al., 2020), associative memory (Bordalo et al., 2023; Kahana, 2012; Pantelis et al., 2008), and memory retrieval (Kensinger and Ford, 2020). This literature has developed powerful concepts to describe the categorization of mental representations (memories), their similarities, and the associations between them. We bring those concepts into the study of interactions between social groups. In the same way that mental representations of physical attributes or other memories can be perceived as more or less similar (Pantelis et al., 2008), we show that subjects have mental representations of social groups which they perceive as more or less similar to themselves and to each other. Our evidence highlights how the interaction between perceived similarity and salient events – such as episodes of struggles by others – acts as a powerful tool to activate empathy.

Second, our work also contributes to a longstanding literature in social psychology, and more recently, in neuroscience, on the role of perceptions of others, for instance in building empathy (see for example Krebs, 1975; Davis, 1994).[9] Recent studies have focused on lab experiments manipulating labels of in-groups versus out-groups, e.g.Vaughn et al. (2018) who examine neural responses to observing pain in others, or Hagenbach and Kranton (2023) who measure whether one subject is able to remember information about shared traits with another, depending on whether they compete or cooperate. We propose practical measures of perceived similarity and empathetic responses; consider commonly used policy tools that can affect perceived similarity, such as engineered information provision or naturally occurring inter-group contact; study policy-relevant empathy-inducing events, such as unauthorized migrations and natural disasters; and we bring the question to natural settings.

Finally, we contribute to the political economy literature in several ways. We add to a growing literature on information provision and attitudes (see, for example, the recent review by Haaland et al. (2023), and in the context of immigration, Haaland and Roth (2020), Alesina et al. (2023), and Kalla and Broockman (2023)) and bring a new channel through which information, even information not designed to be persuasive, can change attitudes – by shifting perceived similarity. Fouka, Mazumder, and Tabellini (2022) show that the assimilation of immigrant communities is shaped, in part, by their

[8] To do so, we adapt methods used in cognitive science to categorize mental representations, and to define the associations between 'proximate' memories (see e.g. Pantelis et al., 2008).

[9] More broadly, our work relates to a long tradition in economics of modeling altruism (Becker, 1974) and the dynamic evolution of preferences (e.g. Becker and Murphy, 1988).

perceived position relative to other out-groups. We extend our bilateral measure of perceived similarity to one foreign origin group to a multidimensional space, which allows us to measure perceptions of relative positions between different foreign origin groups. Our work also brings a new mechanism – increased perceived similarity – through which inter-group contact can shift attitudes, thus contributing to the long literature on inter-group contact (see, for example, Enos (2014), Rao (2019), Mousa (2020), Lowe (2021), Bursztyn et al. (2024); Bailey et al. (2022), and the meta-analyses by Pettigrew and Tropp (2006) and Paluck et al. (2019)). We also contribute to the literature on the formation of and change in attitudes toward out-groups (Fernández et al., 2019; Fouka and Tabellini, 2022; Enke et al., 2023) by shedding new light on how empathy can be built.

Section 1 provides a simple conceptual framework to interpret our empirical setup and results. Section 2 discusses our experimental designs and data. Section 3 describes our empirical strategy. Section 4 presents our main results. Section 5 shows additional results and robustness checks.

1 Conceptual framework

We propose a model of social preferences that formalizes the mechanics of empathy (Becker, 1974).[10] To do so, we adapt the notions of categorization and association of memories from cognitive science to the study of social interactions. We define the concept of *perceived similarity* between an individual and social groups, adapted from the concept of similarity between mental representations used to study associative memory (e.g. Kahana, 2012; Bordalo, Gennaioli, and Shleifer, 2020), combined with triggers generated by *salient events* (Bordalo, Gennaioli, and Shleifer, 2013). We follow the cognitive science literature (Kensinger and Ford, 2020) and postulate that the combination of a salient event affecting someone together with a high perceived similarity entails a strong emotional charge, and therefore induces a lasting imprint in one's memory. The model we present is illustrative, meant to clarify our experimental and empirical setup and interpret our results.

Agent i at time t maximizes the following utility function,

$$u_i(t) = v_i\left(c_i\left(t\right)\right) + \sum_g a_{i,g}(t) v_g\left(c_g\left(t\right)\right). \tag{1}$$

The quasi-concave function $v_i(c_i(t))$ is the individualistic component of i's preferences. In addition, agent i also cares about the well-being of others, $v_g(c_g(t))$, members of various social groups $g \in \{1, \cdots G\}$. Both measures of well-being, v_i or v_g, depend on 'consumption' c_i or c_g, which can readily be extended to include choices such as support for a policy favorable to certain groups, or

[10] Gary Becker typically uses the word 'altruism' to describe 'caring about others.' In keeping with the social psychology literature (e.g. Batson et al., 2002) we use instead the word 'empathy,' to describe the process, in our case a modification of preferences, which may lead to acts that benefit others, 'altruistic' acts.

charitable donations targeting those groups. How much agent i cares about group g is governed by the parameter $a_{i,g}(t)$. Up to this point, our model is similar to any model with social preferences, as in models of altruism (e.g. Becker, 1974), or models of social identity (e.g. Akerlof and Kranton, 2000).

The key innovation is to endogenize the parameter $a_{i,g}(t)$ governing how much agent i cares about group g. We assume $a_{i,g}(t)$ is shaped by agent i's past experiences, which have imprinted memories that i retrieves when they decide whether or not to take actions favorable to any group g. Formally,

$$a_{i,g}(t) = \sum_{\tau<t} S_{i,g}(\tau) E_{i,g}(\tau). \tag{2}$$

$S_{i,g}(\tau)$ is the perceived similarity between agent i and group g at time τ. $S_{i,g}(\tau)$ is persistent, and evolves as agent i learns about and interacts with group g.[11] $E_{i,g}(\tau)$ is a salient event witnessed by agent i at time τ and associated with group g. By its very nature, $E_{i,g}(\tau)$ is instantaneous, as an event only occurs at a specific point in time. However, despite the fleeting nature of the event $E_{i,g}(\tau)$, it can leave a long-lasting imprint in agent i's preferences, if it happens at a time when agent i perceives group g as more similar. To recap, agent i cares about group g at time t ($a_{i,g}(t)$ large) if at one or several points earlier in their life ($\tau_1, \tau_2, \cdots < t$) they have witnessed one or several salient events affecting group g ($E_{i,g}(\tau_1), \cdots$ large) and they perceived themselves as close to that group when those events occurred ($S_{i,g}(\tau_1), \cdots$ large).[12]

The dynamics of empathy in equations (1) and (2) capture four important insights. First, how much a person cares about others depends on a cumulative process – the sum over past life experiences.[13] Second, a person cares more about groups they perceive as similar – the S term. The notion of similarity between individuals belonging to different social groups builds upon models of associative memory (Kahana, 2012): groups with shared experiences and values, or who interact frequently, feel closer to each other. We model the perceived similarity between mental representations of social groups exactly as Bordalo et al. (2023) model the perceived similarity between mental representations of past experiences.[14] Third, salient events are more likely to trigger empathetic reactions and change

[11] As our empirical evidence only allows us to identify one-shot instances where S moves, we refrain from further modeling the full dynamics through which S is constructed and evolves.

[12] In this paper, we only consider events which induce *positive* attitudes towards other groups, $E \geq 0$, i.e. events which may trigger empathy. We conjecture that our model may be applied to events that induce *negative* attitudes, i.e. events that trigger antipathy, but our empirical design does not allow us to directly explore this conjecture.

[13] Assuming that preferences are shaped by cumulative life experiences has a long history in economics, for instance in models of habit formation (Duesenberry, 1949; Constantinides, 1990) or addiction (Becker and Murphy, 1988).

[14] As in Bordalo et al. (2023), we could add the notion of interference, here interference between the mental representations of perceived-to-be proximate groups, by replacing $S_{i,g}$ in equation (2) by $r_{i,g} = {}^{S_{i,g}}/_{\sum_{h=1}^{G} S_{i,h}}$. This would capture the notion that agent i is less likely to alter their preferences in favor of group g if there are many other groups that agent i also perceives as similar. We abstract from interference between proximate groups in this paper, and design our empirical protocol to neutralize interference: when comparing actions by agents i and j, we hold fixed the comparison to other groups ($\sum_{h=1}^{G} S_{i,h} = \sum_{h=1}^{G} S_{j,h}$), using either randomization or fixed effects.

preferences – the E term. This builds upon Bordalo et al. (2013), who show that salient events are more likely to alter preferences and shift expectations. And fourth, perceived similarity and salient events are complementary, so that the effect of a given event is magnified when the agent feels similar to others – the interaction term in equation (2). This builds upon Kensinger and Ford (2020), who describe a wide body of evidence showing that emotional events, especially negative events, are more easily retrieved from memory. Our model directly captures this mechanism: if agent i feels similar to group g and *then* witnesses a salient event affecting g, especially a negative event, i will feel strong emotions, will remember this event vividly whenever asked to help g, and will likely choose to do so.

Importantly, empathy depends on the interaction between perceived similarity and salient events. The impact of a given salient event affecting group g is *magnified* if agent i already feels similar to g. Because perceived similarity is persistent ($S_{i,g}$) and salient events are fleeting ($E_{i,g}$), the order in which agents are subjected to shocks matters: a salient event has a bigger impact if it happens after an agent has been induced to feel similar to a given group than if it happens before.

We use this conceptual framework to guide the interpretation of our empirical findings. Both our experimental and observational settings feature two shocks, corresponding to the two building blocks of our model of empathy: salient events and similarity. In our experimental setup, subjects are exposed to an immersive experience where they witness the struggles of unauthorized migrants created by a uniquely talented movie director, a salient event (shock to $E_{i,migrants}$); and we provide subjects with information about unauthorized immigrants to the US, which increases perceived similarity (shock to $S_{i,migrants}$). In our observational setup, natural disasters hitting those foreign countries are tragic, salient events (shock to $E_{i,foreign}$); and residents in counties where they are more likely to be in contact with people from specific foreign origins feel more similar to them (shock to $S_{i,foreign}$). In both settings the combination of a higher perceived similarity and salient events enhances empathy so that subjects are more likely to choose actions favorable to others: the marginal utility gain derived from contributing to the well-being of group g, for instance by supporting policies favorable to group g or making a subsequent charitable donation aimed at group g, increases with $a_{i,g}(t)$ in equation (1).[15]

To conclude, we note that because the preferences in equations (1) and (2) are cumulative, past

[15]It is likely that the salient emotional events we study may also increase perceived similarity, and as such could be described instead as (E&S-shock): the immersive experience of Carne y Arena may induce visitors to perceive unauthorized migrants as similar because of their shared (virtual) experience, and witnessing the suffering in a country devastated by a natural disaster may induce someone to perceive the people of that country as similar because it reminds them of their own suffering. This would reinforce the impact of the salient events on future empathetic actions. Our experimental and empirical designs control for this possible reinforcement: we always compare groups exposed solely to a salient event, to groups exposed to both an ex-ante similarity shock and the same salient event, i.e., we either compare 'S then E' to 'E' or 'S then (E&S)' to '(E&S),' and isolate the magnification induced by the ex-ante similarity shock.

We also conjecture that a similar, mirror image, mechanism may operate: if someone is induced to feel *less* similar to another, they may subsequently feel less empathy towards them when witnessing their suffering. This is reminiscent of the systematic attempt by genocidal regimes to portray their perceived enemies as less than human, or alien.

events (at $\tau < t$) affect future choices (at $t \geq \tau$). This implies that in a forward-looking dynamic version of our model, agent i may strategically decide to live in certain areas (residential choice), interact with certain people (social networks), or select certain sources of information (news diet), so as to optimally affect their future selves' perceived similarity to others. Such an extension may explain patterns of spatial, social, and political polarization. We leave those questions for future research.

2 Experimental protocol and observational data

2.1 Experimental protocol

We experimentally trigger two types of shocks about unauthorized migrants: a salient event experienced by migrants – the $E_{i,migrants}$ term in equation (2); and an information shock which affects perceived similarity towards migrants – the $S_{i,migrants}$ term in equation (2).

Carne y Arena treatment. Carne y Arena is an Academy-winning virtual reality piece created by director Alejandro González Iñárritu. The visitor is immersed in the experience of unauthorized migrants crossing the US Southern border, based on true accounts. The exhibit has three stages. First, the visitor enters a room which is a replica of the cells where unauthorized migrants apprehended at the US border are held. They are invited to remove their shoes and wait several minutes. The room is cold and contains artifacts from migrants recovered in the Southern border deserts: backpacks, shoes, and water bottles. Second, they enter, barefoot, a large space covered with rough sand, and are fitted with a virtual reality set. They are immersed in a series of interactive scenes of unauthorized migrants crossing the US Southern border, and being apprehended and processed by border patrol. They can move around the protagonists as if they were there. The virtual reality (VR) experience culminates with a final scene where an armed border patrol officer orders the visitor to kneel, pointing his weapon directly at them. Third, having left the VR space and recovered their shoes, the visitor is invited to read through short testimonies from the real-world migrants and border patrol officers who participated in creating the virtual reality piece. The visit lasts about 15 minutes.

Information treatment. We present subjects with statistical information about unauthorized immigration to the US and about the economic conditions in their origin countries. The information consists of a series of 12 exhibits. The information we provide is balanced, grouped into three equal-sized sets which we categorize as 'negative' (e.g. 287,000 pounds of drugs were seized at the border in 2020), 'positive' (e.g. 93% of unauthorized immigrant children aged 13-17 are enrolled in high school), and 'emotional' (e.g. 30,557 unaccompanied children were apprehended at the Southwest border in

2020).[16] Our information package is carefully designed to be neutral and non-persuasive. We conjecture it will have a positive impact on perceived similarity either because it reveals that immigrants to the US do not differ substantially from natives along a range of observable characteristics or because respondents may hold negative misperceptions about the true characteristics of immigrants to the US (Bursztyn and Yang, 2022): simply learning about the true statistical characteristics of immigrants induces respondents to feel more similar to them.

We collect two main measures, a measure of attitudes in favor of migrants, and a novel measure of perceived similarity towards unauthorized migrants. In addition, we collect demographic information on participants and measure their ability to retain the information provided in the information block.

Attitudes. We construct an index of attitudes in favor of migrants, combining six components. We first ask subjects to choose their preferred policies from a list containing two pro-immigration policies – the DREAM Act and asylum policies – and policies unrelated to immigration. Selecting pro-immigration policies reveals positive attitudes (for each, we assign value 1 if selected, 0 otherwise). We then ask them to rank their preferred policies, and record their ranking of the DREAM Act and asylum policies, if selected: a higher rank for either reveals positive attitudes (we assign a score from 1 to 8, least to most preferred). We then ask subjects to choose their preferred policy among anti- and pro-immigration policies: selecting pro-immigration policy reveals positive attitudes (we assign scores from 1 for the most anti-immigration policy – deport all unauthorized migrants, to 5 for the most pro-immigration policy – grant full citizenship to all unauthorized migrants). Finally, we ask subjects to choose a charitable donation to be made on their behalf to a charity supporting immigrants, animal welfare, or environmental projects: choosing the immigrant charity reveals positive attitudes (we assign value 1 if selected, 0 otherwise). Each of the six components (support the DREAM Act or not, support asylum policy or not, the rank of the DREAM Act if selected, the rank of asylum policy if selected, immigration policy views, and donate to the immigrant support charity) is individually standardized (mean zero and std. dev. one). Our final index is the standardized sum of those six components.[17] The standardizations are made for the control group so that coefficient estimates are expressed as percentages of a standard deviation within the control group.

Perceived similarity. We introduce an incentivized measure of the perceived similarity between a subject and a pre-defined 'other,' in this case an unauthorized migrant from Mexico. We ask subjects

[16] See appendix C for the list of information exhibits.

[17] Combining outcomes into an index increases precision by decreasing survey measurement error and limits the potential for biases from multiple hypothesis testing (Broockman et al., 2017; Bursztyn et al., 2017).

to complete a short personality test ('big 5 traits,' McCrae and Costa, 1987),[18] and then to guess how many personality traits, from zero to five, they have in common with an unauthorized immigrant from Mexico, offering incentives to guess correctly.[19] We are not interested in whether a respondent is 'right' or 'wrong,' but only whether they perceive unauthorized migrants as similar to them or not, on a personal level.[20] The incentive is solely meant to ensure they are careful in their answer. This measure is standardized to mean zero and std. dev. one for the control group.

Demographics. We collect information on gender, age, income, marital status, ethnicity, education, occupation, political leaning, and national origin of both the subject and their parents. We use this information to explore heterogeneity between groups.

Attention. For subjects who receive the information treatment, we measure their ability to retain information. We quiz them on each of the twelve information modules, offering incentives to answer correctly, and measure the share of correct answers (between 0 and 1). We use this quiz to confirm that subjects exposed to the information treatment are equally attentive no matter when they receive it – same treatment effect, and to test (and rule out) whether the Carne y Arena treatment distorts the ability of subjects to process certain types of information.

Our experimental protocol randomly assigns subjects to different treatment arms. We run two separate experiments: one field experiment to test whether the interaction of information and Carne y Arena affects attitudes towards immigration, and one online experiment to test whether our information treatment affects perceived similarity.

Field experimental protocol. Subjects are randomly assigned to one of six groups:

'Baseline': our control group in most specifications; we measure attitudes before any treatment.

'CyA': we measure attitudes just after subjects go through Carne y Arena.

'Info then *CyA'*: we measure attitudes after subjects have received our information treatment and *then* gone through Carne y Arena, in that order.

'CyA then *Info'*: we measure attitudes after subjects have gone through Carne y Arena and *then* received our information treatment, in that order.

'Info': we measure attitudes just after subjects receive our information treatment.

[18] See appendix C for the list of personality test questions.

[19] To decide which respondents are eligible for a reward, we anonymously administer the personality test to a single individual. The response of that individual is not meant to be statistically representative.

[20] McCrae and Costa (1987)'s personality traits are not designed to be context dependent. There is no particular reason to believe a trait, e.g. "agreeableness," is shared or not with an unauthorized immigrant from Mexico.

'Long Run': we do not measure attitudes on site, but at a later period instead (over two months), to explore the persistence of the experience treatment.

Online survey experimental protocol. Subjects are randomly assigned to one of two groups:

'Control': respondents receive no information; we measure perceived similarity directly.

'Treatment': we measure perceived similarity after respondents receive our information treatment.

Our experimental protocols are summarized in table 1, and descriptive statistics are shown in appendix table A1 (panels A and B). For our in-the-field experiment, we recruit subjects who visited

TABLE 1: EXPERIMENTAL PROTOCOL

Panel A: On site (Carne y Arena)

Dallas, Texas

	On site (short run)				*Follow-up (long run)*	
'Baseline'	`Attit`	*`Info`*	*`CyA`*	($n = 83$)	`Attit`	($n = 8$)
'CyA'	*`CyA`*	`Attit`	*`Info`*	($n = 99$)	`Attit`	($n = 8$)
'CyA *then* Info'	*`CyA`*	*`Info`*	`Attit`	($n = 87$)	`Attit`	($n = 10$)

Omaha, Nebraska

	On site (short run)				*Follow-up (long run)*	
'Baseline'	`Attit`	*`Info`*	*`CyA`*	($n = 82$)	`Attit`	($n = 29$)
'CyA'	*`CyA`*	`Attit`		($n = 83$)	`Attit`	($n = 34$)
'Info *then* CyA'	*`Info`*	*`CyA`*	`Attit`	($n = 63$)	`Attit`	($n = 28$)
'CyA *then* Info'	*`CyA`*	*`Info`*	`Attit`	($n = 74$)	`Attit`	($n = 24$)
'Info'	*`Info`*	`Attit`	*`CyA`*	($n = 85$)	`Attit`	($n = 34$)
'Long run'	*`CyA`*			($n = 62$)	`Attit`	($n = 22$)
Total A				($n = 718$)		($n = 197$)

Panel B: Online (information and perceived similarity)

'Control'	`Simil`	`Attit`		($n = 750$)
'Treatment'	*`Info`*	`Simil`	`Attit`	($n = 755$)
Total B				($n = 1,505$)

Notes: This table shows, across treatment arms, the ordering of *`treatments`* (Carne y Arena, *`CyA`*; and information, *`Info`*) and `measurements` (demographics [upon arrival]; perceived similarity, `Simil` [online survey only]; attitudes in favor of migrants, `Attit`; and information retention quiz [right after receiving information]). Follow-up measures of attitudes are collected online approximately two months after the on site visit.

the Carne y Arena art installation on site ($n = 718$): at Fair Park in Dallas, Texas (May-June 2022), and at Kaneko in Omaha, Nebraska (June-September 2022). We present results with both locations combined as our baseline but also show robust results for each location separately (see section 5). We keep only data from respondents who reach the end of the on site survey. By design, all subjects

are visitors, and our experimental protocol only varies the order of those treatments and measures randomly. The data collection and randomization are done using Qualtrics[XM].[21] For our online experiment (February 2023), we recruit subjects online ($n = 1,505$) using Prolific and randomize the assignment to control or treatment using Qualtrics[XM].[22] The observable characteristics of respondents, both on site, in follow-up surveys, and online, are balanced between randomized groups, except for gender in some of the smaller Omaha groups (see appendix tables A2, A3, and A4).

Both experiments received IRB approval and are pre-registered with the AEA's RCT registry.[23]

2.2 Observational data

In our observational setting, echoing our experimental setting, we use two types of plausibly exogenous shocks: natural disasters devastating foreign countries, a salient event experienced by people in those foreign countries – the $E_{i,foreign}$ term in equation (2); and contact with people of foreign origin, which affects perceived similarity towards those origins – the $S_{i,foreign}$ term in equation (2).

Charitable donations data. Our charitable donation data are from Bursztyn et al. (2024). The data comes from two major charitable organizations that channel US donations to foreign non-governmental organizations. We use all 55,152 individual donations made to Haiti, Japan, and the Philippines from 2010 to 2017. Most donations are made after a country suffers from a devastating natural disaster.[24] We keep only donations for which we can identify the US county of residence of the donor. Identifying the county of residence of a donor is key, as we will exploit plausibly exogenous variation at the county, not individual level. We carefully remove donations to foreign country f made by donors with ancestral origins in f (e.g. a Haitian-American donating to Haiti).[25] Our primary variable is the total number of donations made by donors residing in domestic county d towards foreign country f over the entire 2010-17 period.

Historical immigration data. To identify plausibly exogenous variations in the likelihood residents in domestic county d have close contacts with people of foreign origin f, we implement the identification

[21] https://www.qualtrics.com/. See appendix section B for the complete survey.

[22] https://www.prolific.co/ and https://www.qualtrics.com/. See appendix section C for the complete survey.

[23] University of Chicago Social and Behavioral Sciences Institutional Review Board approval IRB22-0551, American Economic Association's registry for randomized control trials AEARCTR-0009194 (Andries et al., 2022, on June 8, 2022).

[24] For the three countries in our dataset, the main disaster events are: for Haiti, the 2010 earthquake; for Japan, the 2011 Tohoku earthquake, tsunami, and nuclear disaster; for the Philippines, the 2013 Bohol earthquake and super typhoon Yolanda. Each of those tragic events caused thousands of deaths. See appendix figure A1 for the distribution of the total number of donations to each country over our sample period. Those three countries were chosen ex ante, among those receiving charitable donations in 2010-17, according to the following criteria: they all have a sizeable diaspora in the US, and they are ethnically and culturally different, to avoid spillovers in our perceived similarity measures.

[25] See the details and the various validation tests in Bursztyn et al. (2024) for linking donors to their likely ancestral origins using their first and last names and machine learning techniques.

strategy in Burchardi et al. (2019), described in details in section 3.2. We use the same data on immigration from the individual files of the Integrated Public Use Microdata Series (IPUMS) samples of the 1880, 1900, 1910, 1920, 1970, 1980, and 1990 waves of the US population census and from the 2006-10 five-year sample of the American Community Survey (ACS). Our key measure, $I_{d,f,t}$, is the number of immigrants who reside in domestic county d at time t, were born in foreign country f, and emigrated to the US between $t-1$ and t (the interval between two consecutive census waves).

Contact and perceived similarity survey. To test whether inter-group contact magnifies the empathy response to natural disasters through its impact on perceived similarity, we conduct a large-scale survey ($n = 2,400$) using Prolific.[26] We collect demographic information and drop respondents we cannot match to a US county of residence, who do not complete the survey, or are younger than 18 (about 0.4% of observations). Our resulting sample is somewhat more feminine, foreign-born, Hispanic, and liberal than the US population (see appendix table A5). We elicit from each respondent two main measures, *contact* and *perceived similarity*, towards three foreign countries, Haiti, Japan, and the Philippines. We ask questions about contact *after* questions about perceived similarity, to avoid a mechanical impact from the contact to the perceived similarity questions. We also carefully remove respondents with foreign origin f (born in f or parents born in f) for contact and perceived similarity towards f, to remove endogenous relationships between contact and perceived similarity.[27]

Contact. To measure contact with foreign origins, we ask respondents whether they have regular interactions with Haitian-, Japanese-, or Filipino-Americans (close friends or family members, neighbors, or workplace acquaintances, with whom they speak at least once a month). The ordering of the foreign origins is randomized. For each foreign origin, we construct an index equal to 1 if the respondent answers yes to any of the three contact questions, 0 otherwise.[28] For our identification strategy, we average this measure of contact at the county $\times$ country level.

Perceived similarity. Our measure of perceived similarity towards people from Haiti, Japan, and the Philippines, mimics our measure of perceived similarity towards an unauthorized immigrant from Mexico in our experimental setting. Each respondent first answers a short personality test ('big 5 traits,' McCrae and Costa, 1987). They are then told that we asked the same questions to three people, from Haiti, Japan, and the Philippines. We ask them to guess how many personality traits

[26] https://www.prolific.co/. See appendix D for the complete survey.

[27] This means for instance that for a respondent with Haitian origins, we remove their response on contact and perceived similarity with Haiti, but we keep their responses for Japan and the Philippines. Our findings are robust to keeping *all responses*, or to removing *all respondents* with origin from Haiti, Japan, or the Philippines. See section 5.

[28] We also explore the robustness of our results to using each category of contact separately. See section 5.

they share with each group, offering financial incentives to form the correct guess.[29] The ordering of the questions (Haiti, Japan, or the Philippines) is randomized. For each foreign origin, perceived similarity is the number of personality traits in common, normalized to mean zero and std. dev. one.

Descriptive statistics for our nationwide data on charitable donations and historical migrations, and surveys on contact and perceived similarity, are shown in appendix table A1 (panels C and D).

3 Empirical strategy

3.1 Experimental setting

Specification. We estimate the effect of a specific treatment on a specific outcome,

$$Outcome_i = \alpha + \beta \cdot Treatment_i + \epsilon_i, \tag{3}$$

where $Treatment_i$ takes values zero or one according to which experimental arm individual i is assigned to, and β measures the impact of a given treatment on a given outcome. For instance, for $Outcome_i = Attitudes_i$, $Treatment_i = 0$ if $i \in$ 'Baseline,' and $Treatment_i = 1$ if $i \in$ 'CyA,' β measures the impact of Carne y Arena on attitudes towards immigration, expressed as a percentage of a std. dev. of our composite index among the control group ('Baseline'). We measure attitudes for the control group ('Baseline') *before* they have gone through the Carne y Arena immersive experience. Their attitudes therefore correspond to the unconditional attitudes among the selected group of visitors, including their anticipation of what Carne y Arena will be about. For the treatment group ('CyA'), we measure attitudes *after* they have experienced Carne y Arena. This population is ex ante identical to the control group, the only difference being their actual, randomized exposure to Carne y Arena.

Discussion. We carefully design our experimental protocol to minimize any form of experimenter, demand, or 'Hawthorne' effects. All respondents are selected among the same group of museum-goers, all fill out one part of our survey before entering the exhibit and the other part after. Only the ordering of questions varies between treatment arms. Respondents are told that our survey is designed to study "The Power of Art"[30] which in our view, given that all respondents are visitors to an art exhibit, does not reveal information about the hypotheses we aim to test. We also minimize any form of 'John Henry' effect: only one person at a time is allowed to go through Carne y Arena, so friends cannot communicate about the survey until after they have completed it and exited the exhibit hall; we also directly instruct visitors not to communicate with friends or partners about their questionnaire; and

[29] We use the answers from a randomly selected respondent from Haiti, Japan, or the Philippines.

[30] We thank Katie Cutright from the Emerson Collective for suggesting this choice of words.

respondents answer questions (on an individual tablet) in a dark and quiet space in a secluded waiting area, under a solemn atmosphere, so that they are unlikely to be influenced by others.[31]

3.2 Observational setting

Specification. We estimate, for residents of domestic county d, the impact of personal contact with people from a specific foreign origin country f (Haiti, Japan, the Philippines) on charitable donations to that country, and perceived similarity with people from that country,

$$Donations_{d,f} = \beta^{Don.} Contact_{d,f} + \delta_d^{Don.} + \delta_f^{Don.} + \epsilon_{d,f}^{Don.}, \tag{4}$$

$$Similarity_{i,d(i),f} = \beta^{Sim.} Contact_{i,d(i),f} + \delta_d^{Sim.} + \delta_f^{Sim.} + Control_i + \epsilon_{i,d(i),f}^{Sim.}. \tag{5}$$

Equation (4) measures the impact of inter-group contact on charitable donations, analogous to the impact of information on empathy measured by the difference between the experimental treatment effects on attitudes of information *then* Carne y Arena versus Carne y Arena alone. $Donations_{d,f}$ is the inverse hyperbolic sine of the number of charitable donations from county d to country f;[32] $Contact_{d,f}$ is the mean of personal contact with people from foreign country f, averaged among respondents who reside in county d; the $\delta's$ are county and country fixed effects. $\beta^{Don.}$ measures the impact of contact with a given foreign origin on donations towards that same foreign origin. Given that contact takes values between zero and one, and that our measure of donations approximates the logarithm function, $\beta^{Don.}$ is akin to an elasticity. Equation (4) exploits cross-sectional variations at the county $\times$ country level, identified from plausibly exogenous historical shocks, as explained below.

Equation (5) measures the impact of contact on perceived similarity, analogous to the information treatment effect on perceived similarity in the experiment. $Similarity_{i,d(i),f}$ is our index of perceived similarity with people from foreign country f for individual i who resides in domestic county $d(i)$; $Contact_{i,d(i),f}$ is our zero-one measure of personal contact between individual i, residing in county $d(i)$, with people from origin f; the $\delta's$ are county and country fixed effects; and $Control_i$ are individual observable controls. The parameter $\beta^{Sim.}$ measures the impact of personal contact with a given foreign origin on perceived similarity with that same foreign origin. Given that both contact and similarity are normalized, $\beta^{Sim.}$ is also akin to an elasticity. While equation (5) varies at the individual $\times$ foreign

[31] The physical setting in Dallas (May-June 2022) and in Omaha (June-September 2022) allowed us to run the before and after sections of our survey without interference: visitors both enter and exit the Carne y Arena virtual reality experience in a quiet and dark space inside the exhibit hall. We attempted to run the same experiment in Richmond, CA (Craneway Pavilion, October 2022-April 2023, AEARCTR-0010772, Andries et al., 2023), but as visitors exited into the crowded space of another exhibit, it proved physically impossible to implement our experimental protocol there.

[32] The inverse hyperbolic sine (IHS, or *arcsinh*) function approximates the logarithm function but is well-defined at zero: $IHS(x) = \ln\left(x + \sqrt{x^2+1}\right)$. It is commonly used instead of the log function in applied settings with count data that sometimes takes the value zero. It offers an imperfect solution (Chen and Roth, 2023) to the known selection biases arising from selectively dropping zeros (Silva and Tenreyro, 2006).

country level in our nationwide survey (section 2.2), we exploit the same identification strategy for *Contact*, which only varies at the county × country level, as in equation (4).

Identification. We control for county and country fixed effects, removing any confounding factor at the county or country level.[33] The county fixed effects ensure that we are not picking up the tendency of residents in some counties to be more likely to donate to *any* foreign cause, and to feel closer to people from *any* foreign origin. The country fixed effects ensure that we are not picking up the proximity of a specific foreign country to *any* US county, which would affect both charitable donations and perceived similarity. They also absorb any possible interference between the perceived similarity to one country and to other countries. We also carefully remove respondents of foreign origin f from our measure of charitable donations to f and from our measure of perceived similarity to f (e.g. we remove the charitable donations and the question on perceived similarity to the Philippines for any Filipino-American donor or respondent).

However, omitted county × country-specific factors may create a spurious correlation between the likelihood of contact of residents in a county with people from a foreign origin *and* the generosity and perceived similarity towards the same foreign origin. For instance, a US county may have a strong Roman Catholic identity, the majority faith in the Philippines. Local residents may then be more likely to have contact with Filipinos (e.g. because local churches attract Filipino migrants), they may be more generous towards the Philippines (e.g. to finance the reconstruction of churches after a natural disaster), and they may feel similar to Filipinos (e.g. because of their shared faith).

To address this concern, we implement the identification strategy from Burchardi et al. (2019). They show how to use the coincidental timing of historical emigration from specific countries (e.g. the large inflow of Filipinos to the US as a whole in the 1930's, as the Philippines gained independence from the US in 1934) and immigration towards specific counties (e.g. the large inflow of non-Filipino migrants to Stockton county in the Central Valley of California in the 1930's, following the construction of a deep water canal connecting Stockton to the San Francisco Bay) to construct instruments for the current composition of foreign origins across US counties. We use those instruments to predict the likelihood that residents in d have close contact with people from f (e.g. many non-Filipino residents in Stockton have contact with Filipino-Americans today). Our first stage regression is

$$Contact_{d,f} = \sum_{t=1880}^{2010} \gamma_t I_{-r(d),f,t} \frac{I_{d,-c(f),t}}{I_{\cdot,-c(f),t}} + \delta_d^{Cont.} + \delta_f^{Cont.} + \eta_{d,f}, \tag{6}$$

where $Contact_{d,f}$ is our measure of contact aggregated at the domestic county d and foreign origin f

[33] For equation (5) at the individual level, we can even include individual fixed effects as we measure, for the same respondent, their perceived similarity to each of the three foreign origins (see section 5 and appendix table A18).

level; $I_{-r(d),f,t}$ is the total number of migrants from country f to the US at time t, leaving out those who settle in d's region ('leave-out-push factor' in Burchardi et al., 2019); $\frac{I_{d,-c(f),t}}{I_{\cdot,-c(f),t}}$ is the fraction of migrants from *any* country, not just Haiti, Japan, and the Philippines, who settle in county d among all migrants who emigrated at time t, leaving out foreign country f's continent ('leave-out-pull factor' in Burchardi et al., 2019); the δ's are county and country fixed effects. The $I_{-r(d),f,t} \times \frac{I_{d,-c(f),t}}{I_{\cdot,-c(f),t}}$'s are our excluded instruments ('push-pull-leave-out' instruments in Burchardi et al., 2019). Intuitively, we predict residents in Stockton are likely to have contact with Filipino-Americans (large $Contact_{Stockton,Philippines}$) if many Filipinos emigrated to non-West Coast areas of the US in the 1930's (large $I_{-West\,Coast,Philippines,1930}$) *and* Stockton was an attractive destination for non-Asian immigrants in the 1930's (large $\frac{I_{Stockton,-Asia,1930}}{I_{\cdot,-Asia,1930}}$). Our identifying assumptions are

$$\text{Cov}\left(I_{-r(d),f,t}\frac{I_{d,-c(f),t}}{I_{\cdot,-c(f),t}}, \epsilon^{Don.}_{i,d,f}|\text{Controls}\right) = \text{Cov}\left(I_{-r(d),f,t}\frac{I_{d,-c(f),t}}{I_{\cdot,-c(f),t}}, \epsilon^{Sim.}_{d,f}|\text{Controls}\right) = 0, \quad \forall t = 1880 \cdots 2010, \quad (7)$$

where $\epsilon^{Don.}_{d,f}$ and $\epsilon^{Sim.}_{d,f}$ are the residuals in equations(4) and (5). We require that any unobservable factors that make residents in d (e.g. Stockton) feel more generous or more similar towards f (e.g. the Philippines) are uncorrelated with the coincidental timing of the interaction between push and pull factors going back to 1880 (e.g. the coincidence of a large influx of Filipinos to non-West Coast counties in the 1930's and the attractiveness of Stockton to non-Asian migrants in the 1930's).

Discussion. It is important to note that the IV estimates of both $\beta^{Don.}$ in equation (4) and $\beta^{Sim.}$ in equation (5) only exploit variations in the predicted likelihood of contact at the county × country level and not actual contact at the individual level. For instance, if we found a positive OLS estimate of $\beta^{Sim.}$ in equation (5) at the individual level, it could merely reflect the reverse causality tendency of people to befriend someone they feel similar to. This is not what we do. Instead, we identify plausibly exogenous historical shocks which predict, *at the county level*, the likelihood of contact with people from a specific foreign origin. This predicted likelihood of contact only varies at the county × country level with the historical push-pull immigration shocks from Burchardi et al. (2019), controlling for county and country fixed effects – our first stage equation (6). The information from our survey on contact determines the first stage regression coefficients, the γ_t's in equation (6); but the variation in predicted contact, over and beyond county and country fixed effects, comes solely from historical shocks. We then test whether residents in county d (charitable donors) where we predict a high likelihood of contact with foreign origin f tend to send charitable donations to f when it is devastated by a natural disaster – the IV estimate of $\beta^{Don.}$ in equation (4); and whether other residents in county

d (surveyed respondents) where we predict a high likelihood of contact with foreign origin f tend to feel similar to people from that origin f – the IV estimate of $\beta^{Sim.}$ in equation (5).

4 Results

We now present our results. In our experimental setting (section 4.1), we show that a neutral information treatment induces respondents to form more favorable attitudes towards immigration after they witness the ordeal of unauthorized migrants crossing the Southern border, a shock to $E_{i,migrants}$ in equation (2); and that the same information treatment induces respondents to perceive themselves as more similar to unauthorized migrants, a shock to $S_{i,migrants}$ in equation (2). We then show results similar to this fully controlled experimental environment, in observational data (section 4.2). In a specific county d where we predict that residents are more likely to have close contacts with a specific foreign origin, due to plausibly exogenous historical migrations shocks, more charitable donations flow to that foreign origin f when it suffers from a natural disaster, a shock to $E_{d,f}$ in equation (2); and the same higher predicted likelihood of contact in county d induces its residents to perceive themselves as more similar to people from foreign origin f, a shock to $S_{d,f}$ in equation (2).

4.1 Experimental results

Information, Carne y Arena, and attitudes. We first exploit the controlled environment of the virtual reality immersive experience Carne y Arena to quantify the empathetic response of subjects. The results are presented in figure 1 (see table 2 for additional statistics). Carne y Arena on its own improves attitudes towards immigration by 32% of a std. dev. (top estimate in figure 1, p-value $<$ 0.01). Our main finding is that the impact of Carne y Arena is magnified by our information treatment: attitudes improve by 70% of a std. dev. if respondents received information *before* witnessing Carne y Arena (bottom estimate in figure 1, p-value $<$ 0.01), significantly more than the 32% improvement from Carne y Arena alone. Importantly, this magnification does not operate if information comes *after* Carne y Arena: attitudes improve by 36% (middle estimate in figure 1, p-value $<$ 0.01), statistically indistinguishable from the 32% improvement from Carne y Arena alone. The fact that information only magnifies the impact of Carne y Arena if it comes before (inducing subjects to feel similar to migrants, and living the experience of the protagonists of Carne y Arena as if they were 'in their shoes') but not after (subjects cannot 're-live' the immersive experience) shows this magnification is not due to the informational content of our information treatment having a direct impact on attitudes.

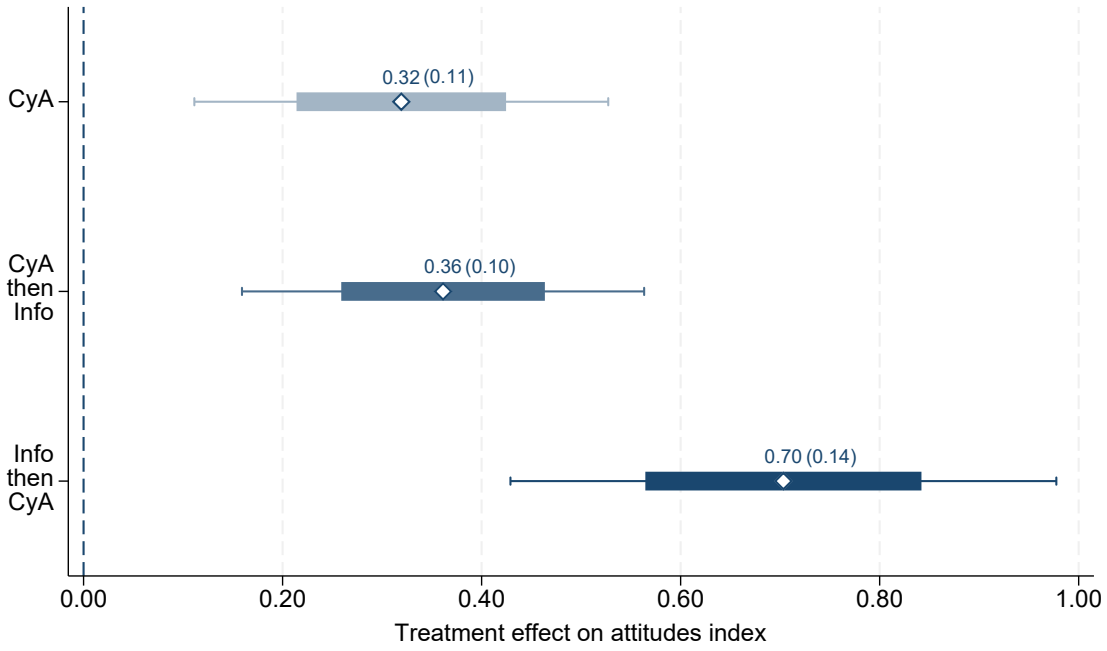

FIGURE 1: INFORMATION, CARNE Y ARENA, AND ATTITUDES

Notes: This figure shows the effect of various treatments on attitudes, expressed as a percentage of a std. dev. of attitudes for the control group, β in equation (3). The thick lines represent $\pm$ one standard error from the point estimates, and the whiskers 95% confidence intervals. Each estimate corresponds to a different regression. The control group is always 'Baseline'. The treatment groups are, respectively: for the first (top) estimate, 'CyA'; for the second (middle), 'CyA *then* Info'; for the third (bottom), 'Info *then* CyA' (see table 1 for details of the experimental protocol). Interpretation ('Info *then* CyA,' bottom estimate): the attitudes of respondents exposed to the information *then* Carne y Arena treatments, in that order, are 70% (s.e. 14) of a std. dev. higher than the attitudes of respondents not exposed to any treatment (control group). See table 2 for additional statistics.

Information and perceived similarity. We then show that the same information on unauthorized immigration experimentally enhances perceived similarity with that specific group – unauthorized migrants to the US. The results are presented in figure 2 (see table 3 for additional statistics). We measure the treatment effect of our information package on perceived similarity. We use either no regression weights, weights to match the demographic attributes of the US population, or weights to match those of visitors to Carne y Arena (using gender, ethnicity, political leaning, and age). For all specifications, we find a large, positive, and significant impact of our information treatment on perceived similarity. The perceived similarity of a respondent exposed to information is 20% (p-value < 0.01) of a std. dev. higher than that of a respondent not exposed to information (Carne y Arena weights, bottom of figure 2). While our information package is carefully designed to be neutral (see section 2.1), it has a positive impact on perceived similarity. We interpret this result as suggesting that simply showing that unauthorized migrants have universal aspirations – they care about the education of their children, contribute to the welfare system, flee from violence, etc – and possibly correcting negative stereotypes about migrants (Bursztyn and Yang, 2022), reveals similarity at a deep level.

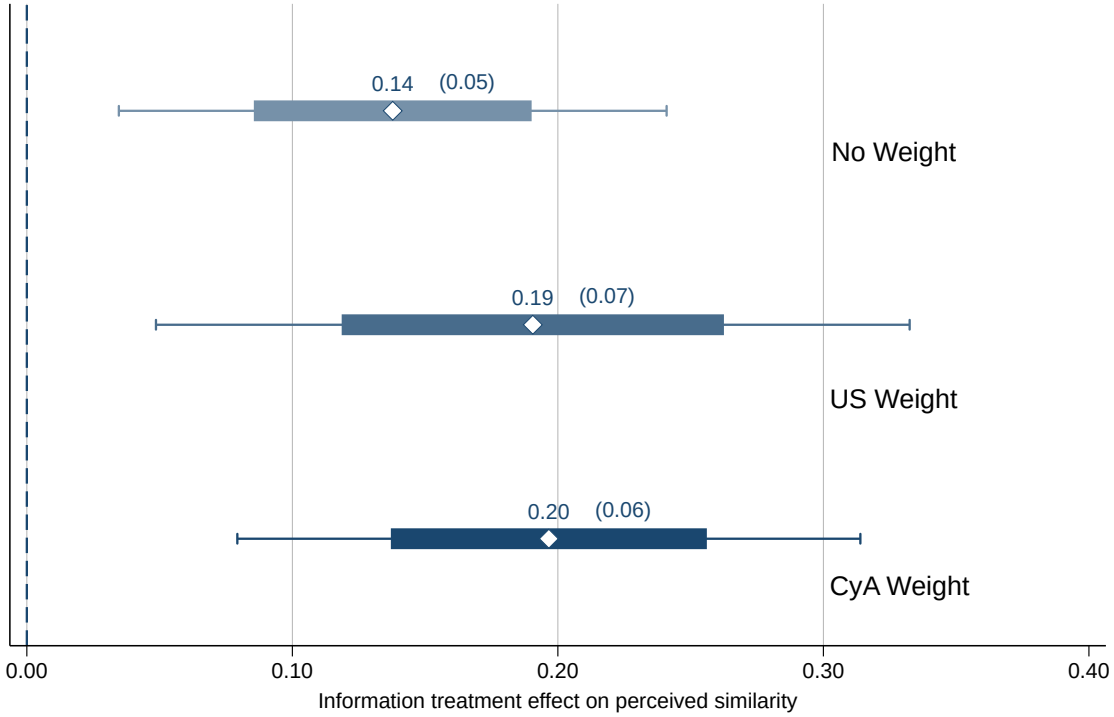

FIGURE 2: INFORMATION AND PERCEIVED SIMILARITY

Notes: This figure shows the effect of our information treatment on perceived similarity, expressed as a percentage of a std. dev. of perceived similarity for the control group, β in equation (3). The thick lines represent $\pm$ one standard error from the point estimates, and the whiskers 95% confidence intervals. The top estimate ('No Weight') uses unweighted observations; the middle estimate ('US Weight') re-weights observations to match the demographics of the US population; the bottom estimate ('CyA Weight') re-weights observations to match the demographics of visitors to Carne y Arena. Interpretation ('CyA weights,' bottom estimate): with weights matching the Carne y Arena demographics, perceived similarity is 20% (s.e. 6) of a std. dev. higher for respondents exposed to our information package (treatment group) compared to respondents not exposed (control group). See table 3 for additional statistics.

Our findings suggest that subjects experience the struggles of unauthorized migrants as if they were 'in their shoes,' and they are willing to take actions to help them (figure 1), if they have previously been experimentally induced to perceive them as similar by our information treatment (figure 2).

4.2 Observational results

We find analogous results in observational data. These results bolster our confidence that our experimental findings hold beyond the strict confines of our selected group of museum-goers, and have real-world consequences. It does however require to re-define several aspects of our procedure. First, we can no longer appeal to the strict exogeneity of a randomized protocol; instead, we use a rigorous identification strategy to isolate quasi-exogenous variations (see section 3.2). Second, instead of respondents witnessing the struggles of unauthorized migrants in the Carne y Arena virtual reality immersive experience, we use the random occurrence of natural disasters; the 'others' are now people from Haiti, Japan, or the Philippines, instead of unauthorized migrants. Third, instead of a measure

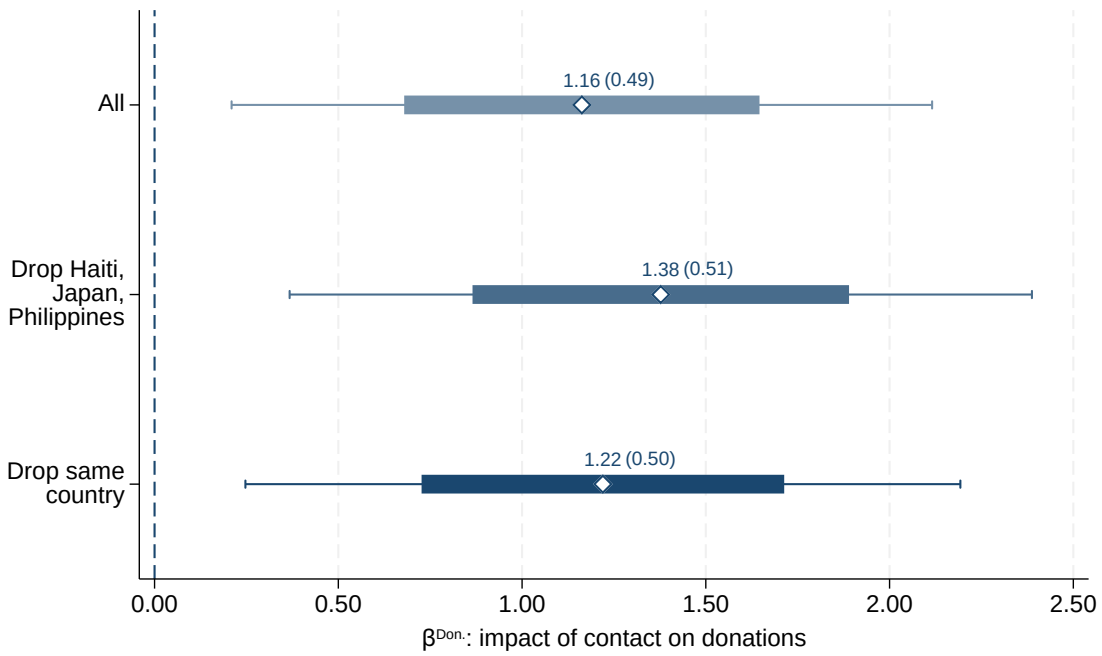

FIGURE 3: CONTACT, NATURAL DISASTERS, AND DONATIONS

Notes: This figure shows the effect of predicted contact with people of a given foreign origin on charitable donations to that origin, the IV estimate of $\beta^{Don.}$ in equation (4), controlling for country and county fixed effects, and where contact is instrumented using equation (6). The thick lines represent ± one standard deviation from the point estimates, and the whiskers to 95% confidence intervals. The top estimate ('All') uses data from all respondents; the middle estimate ('Drop Haiti, Japan, Philippines') drops all answers about contact for respondents from Haiti, Japan, or the Philippines; the bottom estimate ('Drop same country') drops answers about contact with country f for respondents from country f. Interpretation (dropping same country responses, bottom estimate): a 1 p.p. increase in the predicted likelihood that residents in county d have close contacts with people from country f increases the number of charitable donations from d to f by approximately 1.22% (s.e. 0.50). See table 4 for additional statistics.

of attitudes towards immigration to quantify the empathy response of experimental subjects, we use real-life charitable donations to foreign countries. Finally, instead of using our information package to induce changes in perceived similarity, we use the naturally occurring variation of inter-group contact between residents in different US counties and people of different foreign origins.

Contact, natural disasters, and generosity. We first use the random occurrence of natural disasters to quantify the empathy response of residents across US counties. Our measure of the empathy response to natural disasters (in Haiti, Japan, or the Philippines) is computed from real-life charitable donations to foreign countries. We measure the magnification of inter-group contact on the empathy response to witnessing the struggles of others hit by a natural disaster.

The results are presented in figure 3 (see table 4 for additional statistics). We show, for various specifications of equation (4), estimates of the coefficient $\beta^{Don.}$, the impact of contact on charitable donations, where contact is instrumented using equation (6). Respondents who reside in counties where we predict that they are more likely to be in contact with a specific foreign country are more

likely to send charitable donations to that same foreign country when it is hit by a natural disaster. Across all specifications, we find a large, positive, and significant impact of contact on charitable donations. For a specific domestic county, a 1 p.p. increase in the predicted likelihood of contact with people from a specific foreign origin (Haiti, Japan, or the Philippines) induces an increase in the number of charitable donations from this county to that foreign country of approximately 1.2% (bottom estimate of figure 3, dropping same country donations and contact, p-value < 0.01).[34]

As the majority of donations occur in the immediate aftermath of a natural disaster and very few in other periods,[35] our measure of charitable donations corresponds to measuring a treatment effect: the difference between donations when foreign country f is hit by a natural disaster (treatment) versus is not (control, approximately zero donations). Our coefficient of interest, $\beta^{Don.}$, measures the difference in donations between counties where local residents are likely to be in contact with people from country f versus not likely. In other words, $\beta^{Don.}$ corresponds to the difference between a treatment that combines contact with a natural disaster (high predicted contact counties) versus a treatment with only a natural disaster (low predicted contact counties). It is the observational analog to the difference between the experimental treatment effect of Information *then* Carne y Arena versus that of Carne y Arena alone (bottom minus top estimate in figure 1: $0.70 - 0.32 > 0$).

Contact and perceived similarity. We then show that the same plausibly exogenous increase in contact with a specific foreign (Haiti, Japan, or the Philippines) induces a higher perceived similarity to that country: residents in counties where they are more likely to be in contact with people from say Haiti, for plausibly exogenous reasons, perceive themselves as more similar to Haitians. As in our experimental setting, showing that contact induces variation in perceived similarity is key to understanding the empathy response to witnessing the struggles of victims of natural disasters.

Before presenting our formal results, we observe from our descriptive statistics (appendix table A1) that perceived similarity is correlated with contact. Respondents have contacts with Japanese-Americans the most (43%), then with Filipino-Americans (28%), and then with Haitian-Americans (13%); and they feel most similar to Japanese-Americans (+19% of a std. dev. relative to the mean), then to Filipino-Americans (+3%), and then to Haitian-Americans (-22%). This correlation is suggestive of a link between contact and perceived similarity for the US as a whole, but it will of course be fully absorbed by foreign country fixed effects in our formal analysis, which we present next.

The formal results are presented in figure 4 (see table 5 for additional statistics). We show, for various specifications of equation (5), estimates of the coefficient $\beta^{Sim.}$, the impact of contact on

[34] We use the words 'approximately 1.2%' with a slight abuse of notations: the inverse hyperbolic sine function (IHS) approximates, but is not equal to, the natural logarithm function.

[35] See the distribution of donations over the period 2010-17 in appendix figure A1.

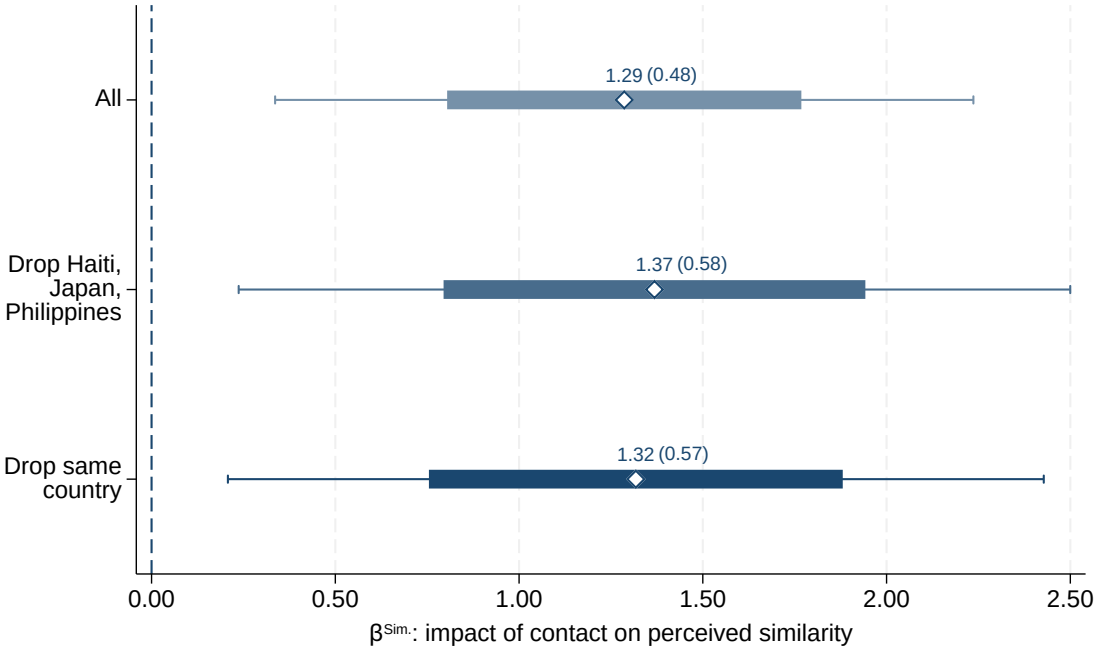

FIGURE 4: CONTACT AND PERCEIVED SIMILARITY

Notes: This figure shows the effect of predicted contact with people of a given foreign origin (Haiti, Japan, or the Philippines) on perceived similarity to that foreign origin, the IV estimate of $\beta^{Sim.}$ in equation (5), controlling for country and county fixed effects, and where contact is instrumented using equation (6). The thick lines represent ± one standard deviation from the point estimates, and the whiskers to 95% confidence intervals. The top estimate ('All') uses data from all respondents; the middle estimate ('Drop Haiti, Japan, Philippines') drops all answers from all respondents from Haiti, Japan, or the Philippines; the bottom estimate ('Drop same country') drops answers about country f for respondents from country f. Interpretation (dropping same country responses, bottom estimate): a 1 p.p. increase in the predicted likelihood that a resident in county d has close contacts with people from foreign country f increases their perceived similarity to f by 1.32% (s.e. 0.57) of a std. dev. See table 5 for additional statistics.

perceived similarity, where contact is instrumented using equation (6). Respondents who reside in counties where we predict that they are more likely to be in contact with a specific foreign origin country perceive themselves as more similar to people from that same foreign country.[36] Across all specifications, we find a large, positive, and significant impact of contact on perceived similarity. A 1 p.p. increase in the predicted likelihood of contact with people from a specific foreign origin (Haiti, Japan, or the Philippines) induces an increase in perceived similarity to that same foreign country of 1.3% of a std. dev. (bottom estimate of figure 4, dropping same country responses, p-value < 0.01). The first-stage F-statistics are not very large (4.9 to 5.7), so we compute p-values from weak IV-robust inference (Andrews et al., 2007; Sun, 2018), which are below 0.01 in all specifications.

The impact of inter-group contact on perceived similarity, $\beta^{Sim.}$ in figure 4, is the observational analog to the experimental information treatment effect on perceived similarity in figure 2.

Our results complement those in Bursztyn et al. (2024), which show that residents in counties with

[36] While the observations in equation (5) are at the individual level, the instruments we use to identify plausibly exogenous variation in contact in equation (6) vary only at the county × country level. See section 3.2.

exogenously more Arab-Muslim residents are more likely to be in contact with Arab-Muslims, more likely to donate to Arab-Muslim countries, and less likely to hold negative misperceptions of Islam or support policies and candidates hostile to Arab-Muslims. We propose a novel explanation for those findings: contact induces a higher perceived similarity with specific foreign origins. We also confirm the impact of contact on charitable donations in a setting with *several* foreign origins, which allows us to control for county fixed effects. Finally, we recognize that our instruments may also affect other variables beyond inter-group contact, for instance, the local news coverage of foreign events, which may affect charitable donations and perceived similarity over and beyond the impact of contact.

4.3 Interpretation and discussion

It is important to understand both the strengths and the limitations of our analysis.

We derive two main results: (i) policy relevant interventions magnify empathy, and (ii) the same interventions increase perceived similarity. We derive results (i) and (ii) for two distinct policy interventions, neutral information provision and inter-group contact, using two distinct empirical designs.

Our first main result is (i): conventional policy interventions magnify empathy. In a fully controlled field experiment, we show that providing neutral information on unauthorized immigration – our first policy intervention– magnifies the positive impact of Carne y Arena on attitudes towards immigration; the impact more than doubles (figure 1). In observational data, we show that inter-group contact – our second policy intervention – magnifies the empathetic response to witnessing the struggles of foreign victims of natural disasters: a one p.p. increase in the likelihood of being in contact with a foreign origin group increases charitable donations to that foreign origin by approximately 1.3% (figure 3). Guided by concepts from cognitive science, we conjecture that the reason why those two policy interventions magnify empathy is because they increase perceived similarity to out-groups, so that subjects live the struggles of those out-groups as if they were 'in their shoes' (our conceptual framework in section 1). To confirm this conjecture, we derive our second main result (ii): the same policy interventions increase perceived similarity. In a large representative survey ($n = 1,505$), our neutral information package on unauthorized immigration increases the perceived similarity to unauthorized immigrants (figure 2). In a separate large representative survey ($n = 2,400$), inter-group contact with foreign origin groups increases perceived similarity to those foreign origin groups (figure 4).

We now go transparently over the strengths and limitations of our analysis.

First, in our experimental setting at Carne y Arena, we use neutral information provision to shift perceived similarity, instead of directly manipulating the perceived similarity of visitors. We believe this is a strength. Our neutral information provision treatment can easily be replicated, scaled up,

adapted to different settings beyond unauthorized immigration, and is on its own of interest to policy makers. We further note that, as part of our agreement with the owners of Carne y Arena (the creator Alejandro González Iñárritu and the production studio Legendary Entertainment), we were not allowed to directly manipulate Carne y Arena visitors. We recognize however that we cannot definitively rule out that unobserved confounding factors may drive both the response of perceived similarity and of attitudes to our information treatment: while our main inference would be the same, information affects both perceived similarity and empathy, the mechanism may be different.

Second, in our experimental setting, we measure the information treatment effect on perceived similarity on a different sample than Carne y Arena visitors. This is a limitation. While we show that neutral information shifts perceived similarity on a much larger and representative sample, offering a blueprint for other large-scale tests of similar hypotheses (e.g. whether neutral information on other out-groups affects perceived similarity to those groups, for instance religious or sexual minorities), we cannot affirm that our information treatment has the same effect on perceived similarity for the smaller sample of Carne y Arena visitors. Unfortunately, the Carne y Arena exhibit is no longer running, preventing us from performing additional tests.

Third, in our observational setting on charitable donations, we test whether inter-group contact shifts perceived similarity instead of directly manipulating the perceived similarity of donors. We believe this is a strength. Inter-group contact occurs naturally, on a scale larger than any experiment could rival, and it is intimately related to migrations (both intra- and international) which are of policy interest on their own. We also note that the two charities which graciously agreed to share their data with us did not grant use permission to access private information on their donors and directly run experiments on them. Again, we believe this is a strength. Our analysis considers real-life donors, endogenously deciding to donate part of their income following naturally occurring disasters.

Fourth, in our observational setting, we use different samples for charitable donations (donors) and for inter-group contact (large representative sample of online respondents). The reason is, again, that we were not granted the permission to access private information on donors by the charities we teamed up with. This constraint forces us to run our analysis at the level of US county $\times$ foreign country cells, not at the individual level. This is a limitation because we are not able to directly link individual donors to inter-group contact. However, we believe this is also a strength because it allows us to deploy a more rigorous identification strategy (Burchardi et al., 2019) which exploits historical shocks at the level of county $\times$ country cells.

5 Additional results and robustness checks

***Multilateral* perceived similarity.** We have shown that perceived similarity is key to understanding the mechanics of empathy. We showcase below the versatility of our measure of perceived similarity, and its potential as a powerful tool to study social perceptions.

We extend our analysis of perceived similarity between a respondent and a given foreign country, a *bilateral* measure, to a *multilateral* measure. We apply a simple multidimensional scaling method to represent in a two-dimensional space the positions of Haiti, Japan, and the Philippines, relative to the respondents.[37] This approach is similar to the 'spatial' mental representation of memories in neuroscience (see for instance Pantelis et al., 2008).

We apply this method to compare, across political affiliations, the mental representations of those three countries in a two-dimensional space. We partition respondents into five political groups: very conservative, conservative, moderate, liberal, and very liberal. For each group, we represent in two-dimensional space the positions of Haiti, Japan, and the Philippines, relative to the (average) position of the respondents. The resulting spatial representations are presented in figure 5. A clear picture emerges. Liberal respondents live in a 'smaller world' than conservative respondents: not only do liberals perceive themselves as more similar to all foreign origins, but they also perceive those foreign origins to be more similar to each other than conservatives. Interestingly, the increased perceived similarity among liberals compared to conservatives is strongest for countries that all respondents, liberals and conservatives, perceive as less similar (Haiti and, to a lesser degree, the Philippines). For Japan, which is perceived as the most similar, perceptions differ much less between liberals and conservatives. This may be due to the fact that Japan is a wealthy country, and therefore perceived as more similar to anyone in the US, a wealthy country too.

Those results suggest that, in the same way that experiences can interfere with memory retrieval if they are associated with similar mental representations (Bordalo et al., 2023; Pantelis et al., 2008), contact with one group could interfere with contact with another group if those two groups are perceived as similar to each other.[38] Those results also suggest a complementary explanation for the

[37] For each respondent i, we measure their perceived similarity to Japan, Haiti, and the Philippines on a scale from 0 to 5 (section 2). We use those similarity measures to define the distance between i and each foreign country f: $Distance_{i,f} \equiv 5 - Similarity_{i,f}$. We then assume that i and f can be represented by their (x, y) coordinates in a 2-dimensional space, s.t.

$$Distance_{i,f} = \sqrt{(x_i - x_f)^2 + (y_i - x_f)^2}.$$

With at least three respondents who live in the same "topography," i.e. for whom Haiti, Japan, and the Philippines have the same coordinates, we can solve for those coordinates, up to a translation and a rotation. Intuitively, if some respondents perceive both Haiti and the Philippines to be similar and Japan to be dissimilar, while others perceive Japan to be similar and both Haiti and the Philippines to be dissimilar, we infer that Haiti and the Philippines are close to each other, while Japan is far. We apply this method separately for respondents partitioned into five political groups.

[38] Our ability to control for county fixed effects, ensures that our results in section 4.2 are not affected by this potential

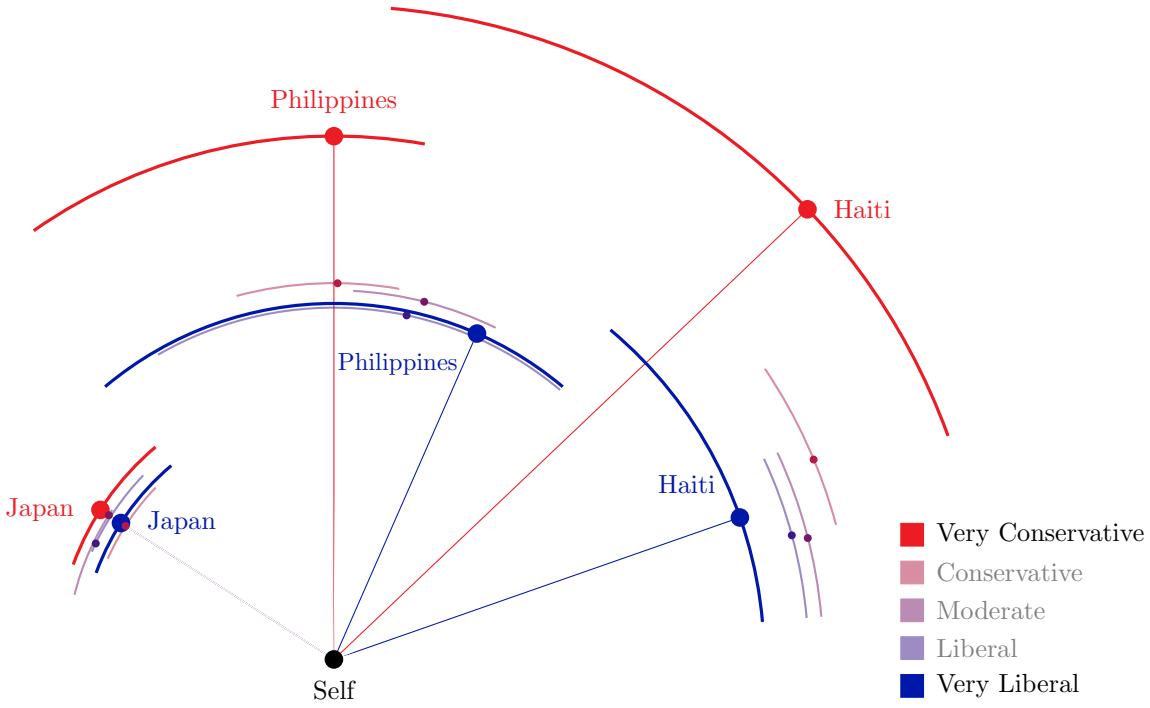

FIGURE 5: MULTILATERAL PERCEIVED SIMILARITY

Notes: The figure presents a two-dimensional spatial representation of the relative positions of respondents (self), Haiti, Japan, and the Philippines, according to our perceived similarity measure. We partition the set of respondents into five political groups according to their stated political ideology: 'very conservative,' 'conservative,' 'moderate,' 'liberal,' and 'very liberal.' For each group separately, we perform a two-dimensional scaling exercise, where we define the distance between respondent i and country f as $Distance_{i,f} = 5 - Similarity_{i,f}$. The bilateral distances within each group are presented in appendix table A6. 'Self' is the centroid of all respondents within each group. We arbitrarily normalize the direction towards Japan for all groups (Northwest).

findings in Enke et al. (2022) who show universalists allocate their altruism over short and long social distances more uniformly than communitarians: universalists may simply perceive all social distances to be shorter. Figure 5 shows this is the case for liberals' perceptions of foreign origin groups.

Overall figure 5 paints a more subtle picture of the role played by perceived similarity than our purely bilateral analysis. We leave a deeper exploration of these more complex interactions across multiple groups and the analysis of the topography of social inter-group connections for future research.

Experimental setting: heterogeneity. We find no systematic evidence that our results are driven by a composition effect of different groups responding differently to our treatments. For both experiments, we split the sample of respondents between types of roughly equal sizes: women versus men, Hispanics versus non-Hispanics, foreign versus US-born, and liberals versus conservatives. The results are presented in A7 (on site Carne y Arena experiment) and appendix tables A8 (online experiment on information and perceived similarity). For both experiments and all groups, the treatment effects are not statistically different between types. This is true even though subjects across groups have

interference: the relative similarities to *other* foreign origins are always absorbed by county fixed effects.

different ex ante attitudes: for instance, subjects who self-report being more conservative have less favorable attitudes towards immigration than subjects who self-report being more liberal.

We note two interesting patterns, although neither is strongly significant. First, all treatment effects are weaker for Hispanic and foreign-born subjects (the majority of whom are of Hispanic origin) than for non-Hispanics and US-born subjects. This is not surprising: one may expect some Hispanic subjects to have a better knowledge of the reality of immigration in the US (the information treatment), and some direct or indirect knowledge of the ordeal of crossing the Southern border (the Carne y Arena treatment), which would explain why the treatment effects are weaker for this group.

More interestingly, we find that more conservative subjects respond differently than more liberal subjects. Conservatives see a smaller improvement in their attitudes towards immigration after Carne y Arena (+26% for conservatives versus +39% for liberals, appendix table A7 column 1), but a larger increase in perceived similarity after our information treatment (+23% for conservatives versus +17% for liberals, appendix table A8 column 3). Our conceptual framework (section 1) suggests that the larger increase in perceived similarity for conservatives (larger S-shock) should magnify the impact of Carne y Arena on their attitudes (larger S-shock $\times$ E-shock). We find suggestive evidence in support of this prediction: the improvement in attitudes after our information *then* Carne y Arena treatment is similar for conservatives and liberals (+58% for conservatives and +63% for liberals, appendix table A7 column 3). It suggests the larger information-induced shift in perceived similarity for conservatives undoes the initial difference in perceived similarity and closes the gap in empathetic responses.

For conservatives, who hold ex ante less favorable attitudes towards immigration, our neutral information treatment has a strong impact on perceived similarity, possibly because it is viewed as neutral or non-ideological. Once those conservative subjects have been induced to perceive unauthorized migrants as similar to themselves, they have a strong empathetic response to the Carne y Arena immersive experience. By contrast, Carne y Arena alone may be perceived by conservative subjects as too manipulative, or they may lack the ability to empathize strongly with the protagonists of Carne y Arena. Our findings suggest a possible policy intervention to gain support for reform in favor of a vulnerable outgroup from members of the ingroup who hold ex ante hostile views: simply providing neutral information that reveals some universal traits shared with the outgroup enhances empathy.

We recognize, however, that those results are only suggestive. While the political views of subjects in our online experiment are broadly aligned with the overall US population, the sample of museum-goers is heavily skewed towards liberal views.[39]

[39] For instance, only 6% of Carne y Arena subjects report having voted for Donald Trump in 2020 (33 out of 537 who report they voted for either Trump or Biden), compared to 46.8% of voters for the US as a whole.

Experimental setting: persistence. Carne y Arena has a large and persistent impact on attitudes towards immigration. Appendix table A9 shows that two months after visiting the exhibit, the attitudes of respondents who have *only* seen the exhibit but for whom we have never measured attitudes ('Long run' group) are 41% of a std. dev. larger than for those who have not yet been through the exhibit ('Baseline' group on site). The size and persistence of the impact of the immersive experience of Carne y Arena on attitudes is worth noting. For instance, Kalla and Broockman (2023), who use a similar measure of attitudes towards immigration, find that the largest and most effective treatment (through discourse rather than virtual reality) moves attitudes by 10-15% of a std. dev. after 1.5-4.5 months, substantially below the 41% long-run impact of Carne y Arena. We conjecture that the strength and persistence of Carne y Arena may be due to a strong memory imprint and vivid memory retrieval, in part because memories from emotional events tend to be durable (Phelps, 2004; Kensinger and Ford, 2020), and in part because sensory memories, especially when associated with negative valence, tend to be more vivid and less likely to fade (Bowen et al., 2018; Cooper et al., 2019).

The long-run impact of Carne y Arena on attitudes is similar for respondents who have *only* seen the exhibit (+41%, p-value = 0.06, for the 'Long run' group two months later versus the 'Baseline' group on site), and respondents who have seen Carne y Arena but may also have been exposed to other treatments and for whom we may have measured attitudes on site (+41%, p-value < 0.01, for the combined 'Long run'-'Baseline'-'CyA'-'Info'-'Cya *then* Info'-'Info *then* CyA' groups two months later versus the 'Baseline' group on site). This suggests that while the impact of Carne y Arena is large and durable, the additional impact of information may be transient and more quickly forgotten. However, with a follow-up rate of around 25%, we lack the statistical power to distinguish the long-term effects of different treatments orderings, with only about 30 respondents per treatment arm.

Experimental setting: anchoring. We reject in appendix table A10 that the persistence of the effect of Carne y Arena is due to anchoring, the tendency of a respondent to remember their stated attitudes and not change them. We compare the long-term attitudes of subjects exposed to the exact same Carne y Arena treatment on site, in one group to whom we have asked about their attitudes on site ('CyA' group, possible anchoring) and in one group to whom we have not asked about attitudes ('Long Run' group, no anchoring). Two months later, their attitudes are statistically indistinguishable (p-value = 0.51 for a test of equality of long-term attitudes between the 'CyA' and 'Long run' groups).

Experimental setting: experimenter effect. The long-run impact of Carne y Arena allows us to rule out a subtle form of experimenter effect. On site, despite our best efforts to design our experiment in a way that neutralizes the possibility of such an effect (the randomization is only done

on the ordering of the treatments, respondents are only aware that our study is about the vague notion of "the Power of Art," respondents are in a quiet and secluded space, etc.), it remains possible that respondents feel under pressure from the (remote) presence of the research team when answering questions. This pressure is not there two months later, at home. We find approximately the same treatment effect of Carne y Arena at home and on site (+41% at home versus +32% on site), which suggests our initial results are not driven by some implicit pressure from the experimenter on site.

Experimental setting: information treatment effect on attitudes. In our conceptual framework (section 1), shocks to perceived similarity do not affect empathy absent an emotional event – the S and E interaction in equation (2). We designed our information treatment to be neutral, i.e. to mimic a strict S shock. Accordingly, as our conceptual framework suggests, our information treatment has no impact on attitudes in our online experimental survey (0.028, p-value = 0.59, in column 1 in appendix table A11); its only impact is on perceived similarity. However, we find contrasting evidence in our on site experiment, where the information treatment has a positive and significant impact on attitudes (0.404, p-value < 0.01, in column 2). We interpret, through the lens of our conceptual framework, this positive effect on site as suggestive that for our selected sample of museum-goers, visiting Carne y Arena entails a form of background-noise emotional event, perhaps because most visitors are anticipating an emotionally intense virtual reality piece on the experience of unauthorized migrants.

Experimental setting: attention. An alternative interpretation for why attitudes increase more when respondents receive information *before* they witness Carne y Arena (+70%) versus *after* (+38%) would be that, after Carne y Arena, respondents may be in an emotional state which could impede their ability to process information. We find no evidence that Carne y Arena affects the attention respondents pay to information. We directly measure the ability of respondents to retain information and find no significant differences in this ability across treatment arms in table A12. In particular, the ability to retain information is not stronger when information is received *before* ('Info *then* CyA') versus *after* Carne y Arena ('CyA *then* Info'). We find no evidence either that our various treatments have a differential impact on the attention paid to different *types* of information, where we pre-assign the 12 exhibits on immigration to three categories: 'positive,' 'negative,' and 'emotional' information. The ability of respondents to retain specific types of information is not significantly affected by the Carne y Arena, nor does it vary significantly between information types. This confirms that our information treatment is equally strong in all treatment arms: subjects in all groups pay similar attention to information. It also rules out a more subtle effect: Carne y Arena does not selectively affect the attention subjects pay to different types of information. This contrasts with the evidence, for

instance, from Malmendier and Nagel (2011) who find, in a very different context, that life experiences affect how individuals process information and what type of information they pay attention to.

Experimental setting: alternative sub-samples. Appendix table A13 confirms our experimental results are similar in both Carne y Arena sites, Dallas and Omaha. Panel A replicates our results for the combined sample. Panel B presents results for the subset of Dallas visitors only, and panel C for Omaha visitors only. Even though the demographic characteristics of visitors in Dallas and Omaha differ (for instance, visitors are more likely to be Hispanic or foreign-born in Dallas than in Omaha, see appendix table A2), estimates are similar for the sub-samples and the combined sample.

Experimental setting: inference and permutation tests. Our experimental samples are relatively small, so that inference based on asymptotic theory may not be valid. We use alternative finite sample methods to compute p-values for the tests of significance of our coefficients. In addition to using robust standard errors for our baseline inference, we also compute wild bootstrap p-values (Wu, 1986; Cameron et al., 2008), as well as p-values computed using a permutation test (Imbens and Rubin, 2015; Young, 2019). For all coefficients, and for all methods, p-values are below 0.01.

Observational setting: inference. In our baseline specification (tables 4 and 5) we compute standard errors using 2-way clustering at the county and country level, the level at which our instrument varies. Table A14 explores alternative measures of standard errors. Across different measures, standard errors do not vary much, and our baseline (two-way clustering) lies in the middle of the distribution. For the impact of contact on charitable donations, our baseline (0.496) lies between the standard error clustered at the county level (0.610, largest) and the robust standard error (0.374, smallest). For the impact of contact on perceived similarity, our baseline (0.566) lies between the robust standard error (0.744, largest) and the standard error clustered at the country level (0.528, smallest).

Observational setting: OLS vs IV. Appendix tables A15 reports OLS and IV estimates of $\beta^{Don.}$ in equation (4) and $\beta^{Sim.}$ in equation (5). Panel A replicates our baseline IV estimates (from column 3 in tables 4 and 5), and panel B the corresponding OLS estimates. The OLS estimates are positive, but smaller than our baseline IV estimates. We conjecture that this may be due in large part to measurement error in contact at the county level, which our instruments may partly correct.

Observational setting: alternative measures of contact. Appendix table A16 decomposes the impact of contact on charitable donations and perceived similarity, using the individual components of the contact question in our survey. Column 4 replicates our baseline estimates (column 3 in tables 4

and 5), where we assign a value of 1 if a respondent answers yes to any of our contact questions (have a friend, a neighbor, or a co-worker from a given foreign origin) and 0 otherwise. Columns 1-3 use each question individually. Panel A suggests that the impact of contact on charitable donations is primarily driven by contact with co-workers (elasticity 1.88, p-value < 0.05), although the other measures of contact also have a positive impact on donations (elasticities 0.56 and 0.60), but we lack sufficient power (p-values > 0.1 for both). Panel B on the other hand shows that each individual measure of contact has a positive impact on perceived similarity of the same magnitude as the combined index, though more significant for friends and neighbors (elasticities 1.84 and 2.39 respectively, p-values < 0.01 for both) than for co-workers (elasticity 1.61, p-values > 0.1).

Observational setting: duration of residency. Appendix table A17 shows that the impact of contact on perceived similarity is not driven by respondents who have resided in their current US county for short versus long periods. Column 4 replicates our baseline estimate (column 3 in table 5). In columns 1-3, we run separate regressions for respondents who have been residents in their county for a short period (less than 10 years, column 1), a medium period (10 to 30 years, column 2), and a long period (more than 30 years, column 3). This split of our sample by duration roughly corresponds to equal-sized terciles, with around 2,000 observations for each (about 700 respondents). Although we lose power for each sub-sample, and our estimates are not statistically significant at conventional levels, the coefficients are positive and have similar magnitudes for long and short durations of residence. This suggests that our results are not driven by respondents who may have endogenously decided not to move away from a specific county because they like its ethnic composition (long-duration residents). It also suggests that contact starts having an impact on perceived similarity within less than 10 years.

Observational setting: individual controls. Finally, appendix table A18 shows that our results are robust to controlling for individual characteristics. We can run this test for our survey on perceived similarity, for which we collect individual demographic information, but not for our charitable donations data, which is anonymized and contains no personal information on donors. Column 1 replicates our baseline estimate with county and country fixed effect (column 3 in table 5). Columns 2 to 5 control for individual characteristics one at a time, and column 6 for all characteristics jointly. The point estimates do not vary across specifications beyond the third decimal point (elasticities from 1.317 to 1.319 versus 1.318 for our baseline, p-values < 0.05 for all). Column 7 goes one step further and controls directly for individual fixed effects. This means we solely exploit variation within individuals, who live in counties where predicted contact with individual foreign origins (Haiti, Japan, the Philippines) is high or low, and who perceive those foreign origins as similar or not. Even in this highly

saturated specification, we still find the same strong and significant impact of contact on perceived similarity as in specifications with only individual controls (elasticity 1.303, p-value < 0.05).

Conclusion

Using a series of field experiments and observational data, we study the mechanics of empathy. We show that whether a person will have a strong empathetic response or not after witnessing the struggles of others depends on whether they perceive those others as similar to them. Perceiving others as similar enables them to live others' experience of struggles as if they were 'in their shoes.'

We propose a measure of perceived similarity which can be applied to any pre-defined groups of others: we implement it for unauthorized migrants in the US and people of specific foreign origins (Haiti, Japan, and the Philippines). In a controlled field experiment, subjects exposed to statistical information about unauthorized immigrants to the US have a stronger empathetic response to witnessing the ordeal of unauthorized migrants crossing the Southern border in a virtual reality immersive experience: they are more likely to donate to charities helping migrants and form more positive political attitudes towards immigration. We also show that the same information package induces respondents to feel similar to unauthorized migrants. In observational data, residents in counties where they are likely to be in contact with specific foreign origin groups (from Haiti, Japan, or the Philippines) feel more similar to these groups and have a stronger empathetic response to witnessing those foreign origins devastated by natural disasters: they send more charitable donations to those foreign countries.

Taken together, our results suggest a novel mechanism through which political and private attitudes can be affected: information provision and inter-group contact can improve a person's ability to put themselves in the shoes of others. In particular, we show that neutral information, not designed to be persuasive or manipulative, can alter how similar subjects perceive others, and, in turn, unleash their empathy, even when subjects are initially more hostile to the other group.

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Main Tables

Table 2: Information, Carne y Arena, and Attitudes

	(1) Attitudes	(2) Attitudes	(3) Attitudes
CyA	0.319***		
	(0.106)		
CyA *then* Info		0.361***	
		(0.103)	
Info *then* CyA			0.703***
			(0.139)
Constant	-0.000	-0.000	-0.000
	(0.077)	(0.078)	(0.073)
p-value Robust S.E.	< 0.01	< 0.01	< 0.01
p-value Wild Bootstrap	< 0.01	< 0.01	< 0.01
p-value Permutation test	< 0.01	< 0.01	< 0.01
Observations	347	326	238

Notes: This table shows estimates of various specifications of equation (3), corresponding to figure 1. The dependent variable is an index of attitudes in favor of migrants, normalized to mean zero and std. dev. one for the control group. The control group is always 'Baseline.' The treatment groups are: 'CyA' in column 1; 'CyA *then* Info' in column 2; 'Info *then* CyA' in column 3 (see protocol in table 1). Robust standard errors are in parentheses. *p*-values using robust standard errors, wild bootstrap (Wu, 1986), and a permutation test (Young, 2019). *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Information and Perceived Similarity

	(1) No weight	(2) Representative weights	(3) CyA weights
Information	0.138***	0.191***	0.197***
	(0.053)	(0.072)	(0.060)
Constant	0.000	-0.088*	-0.028
	(0.037)	(0.051)	(0.044)
p-value Robust S.E.	< 0.01	< 0.01	< 0.01
p-value Wild Bootstrap	< 0.01	0.014	< 0.01
p-value Permutation test	< 0.01	< 0.01	< 0.01
Observations	1505	1502	1502

Notes: This table shows estimates of various specifications of equation (3), corresponding to figure 2. The dependent variable is a measure of perceived similarity to unauthorized migrants, normalized to mean zero and std. dev. one for the 'Control' group. Observations are un-weighted in column 1; weighted to match the demographics of the US population in column 2; weighted to match the demographics of visitors to Carne y Arena in column 3 (see protocol in table 1). Robust standard errors in parentheses. *p*-values are computed using robust standard errors, wild bootstrap (Wu, 1986), and a permutation test (Young, 2019). *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Contact, Natural Disasters, and Donations

	(1) All	(2) Drop Haiti, Japan, Philippines	(3) Drop same country
Contact	1.163**	1.377***	1.220**
	(0.486)	(0.515)	(0.496)
Observations	6364	6179	6223
First stage *F-statistic*	5.679	4.900	5.314
Weak IV-robust *p-value*	< 0.01	< 0.01	< 0.01

Notes: This table corresponds to figure 3. It shows various IV estimates of the effect of contact with people of a given foreign origin on charitable donations to that origin, controlling for country and county fixed effects, $\beta^{Don.}$ in equation (4). The $I_{-r(d),f,t} \times I_{d,-c(f),t}/I_{\cdot,-c(f),t}$'s are our excluded instruments for the first-stage equation (6). Column 1 uses data from all respondents. Column 2 drops all answers from all respondents from Haiti, Japan, or the Philippines. Column 3 drops answers about country f for respondents from f. Standard errors, in parentheses, are clustered at the country and county level. We also report weak IV-robust p-values (Andrews et al., 2007). *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Contact and Perceived Similarity

	(1) All	(2) Drop Haiti, Japan, Philippines	(3) Drop same country
Contact	1.286***	1.368**	1.318**
	(0.484)	(0.577)	(0.566)
Observations	6364	6179	6223
First-stage *F-statistic*	5.679	4.900	5.314
Weak IV-robust *p-value*	< 0.01	< 0.01	< 0.01

Notes: This table corresponds to figure 4. It shows various IV estimates of the effect of contact with people of a given foreign origin on perceived similarity to that foreign origin, controlling for country and county fixed effects, $\beta^{Sim.}$ in equation (5). The $I_{-r(d),f,t} \times I_{d,-c(f),t}/I_{\cdot,-c(f),t}$'s are our excluded instruments for the first-stage equation (6). Column 1 uses data from all respondents. Column 2 drops all answers from all respondents from Haiti, Japan, or the Philippines. Column 3 drops answers about country f for respondents from country f. Standard errors, in parentheses, are clustered at the country and county level. We also report weak IV-robust p-values (Andrews et al., 2007). *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix

"In Their Shoes"

Marianne Andries,

Leonardo Bursztyn

Thomas Chaney

Milena Djourelova

In this online appendix, we present additional statistics and results (appendix section A), and the complete surveys we administered, on the Carne y Arena experiment (appendix section B), on perceived similarity (appendix section C), and on contact to foreign origin groups (appendix section D).

A Additional Tables and Figures

Appendix Table A1: Descriptive Statistics

	Obs.	Mean	Std. Dev.	Median	Min.	Max.
Panel A: Carne y Arena						
Share correct answers	1,261	0.383	0.338	0.417	0.000	1.000
Attitude index:						
On site	656	0.297	0.936	0.548	-3.952	1.720
At home	197	0.408	0.806	0.526	-3.287	1.720
Panel B: Experimental survey on information and perceived similarity						
Perceived similarity	1,505	0.069	1.022	-0.050	-3.379	2.169
Share correct answers	755	0.745	0.185	0.750	0.083	1.000
Attitude index	1,505	0.014	1.000	0.172	-2.082	2.043
Panel C: Charitable donations and historical migrations (county d-country f level)						
IHS-transformed number of donations from d to f	12,978	1.967	2.216	0.881	0.000	8.002
Number of immigrants from d to f at t (in 1,000's)	88,370	0.041	0.720	0.000	0.000	95.964
Panel D: Online survey on perceived similarity to Haiti, Japan, and the Philippines						
Perceived similarity:						
All	7,173	0.000	1.000	0.357	-2.327	2.146
Haiti	2,391	-0.223	1.006	-0.538	-2.327	2.146
Japan	2,391	0.196	1.010	0.357	-2.327	2.146
Philippines	2,391	0.026	0.938	0.357	-2.327	2.146
Contact:						
All	7,173	0.283	0.450	0.000	0.000	1.000
Haiti	2,391	0.134	0.341	0.000	0.000	1.000
Japan	2,391	0.435	0.496	0.000	0.000	1.000
Philippines	2,391	0.280	0.449	0.000	0.000	1.000

Notes: The table presents summary statistics for all datasets used in the main analyses, except for the demographic information in the on site and online surveys (described in appendix tables A4-A5). Note that the attitude index on site (panel A), and perceived similarity and the attitude index online (panel B), are normalized to mean zero and standard deviation one for their respective control groups; but as we have induced higher indices in the other treatment groups, the means are higher than zero, and the standard deviations different from one. By contrast, perceived similarity with foreign origins (panel D) is normalized for the entire population, so its mean is zero and standard deviation one by construction.

Appendix Table A2: Balance Test, Carne y Arena

	Baseline	CyA	Info	CyA *then* Info	Info *then* CyA	Long run	Test
Panel A: Dallas							
	83 (30.9%)	99 (36.8%)		87 (32.3%)			
Gender							
Male	31 (37.3%)	44 (44.4%)		35 (40.2%)			0.618
Female	52 (62.7%)	55 (55.6%)		52 (59.8%)			
Birthplace							
US born	43 (51.8%)	45 (45.5%)		44 (50.6%)			0.655
Foreign	40 (48.2%)	54 (54.5%)		43 (49.4%)			
Ethnicity							
Non-Hispanic	55 (66.3%)	56 (56.6%)		47 (54.0%)			0.231
Hispanic	28 (33.7%)	40 (46.0%)		43 (43.4%)			
Ideology							
Conservative	30 (36.1%)	38 (38.4%)		34 (39.1%)			0.919
Liberal	53 (63.9%)	61 (61.6%)		53 (60.9%)			
Panel B: Omaha							
	82 (18.3%)	83 (18.5%)	85 (18.9%)	74 (16.5%)	63 (14.0%)	62 (13.8%)	
Gender							
Male	34 (42.5%)	33 (40.2%)	27 (32.9%)	18 (24.7%)	31 (50.0%)	19 (31.1%)	0.032
Female	46 (57.5%)	49 (59.8%)	55 (67.1%)	55 (75.3%)	31 (50.0%)	42 (68.9%)	
Birthplace							
US born	52 (63.4%)	64 (77.1%)	68 (80.0%)	51 (68.9%)	45 (71.4%)	43 (69.4%)	0.197
Foreign	30 (36.6%)	19 (22.9%)	17 (20.0%)	23 (31.1%)	18 (28.6%)	19 (30.6%)	
Ethnicity							
Non-Hispanic	64 (78.0%)	72 (86.7%)	73 (85.9%)	64 (86.5%)	51 (81.0%)	55 (88.7%)	0.455
Hispanic	18 (22.0%)	11 (13.3%)	12 (14.1%)	10 (13.5%)	12 (19.0%)	7 (11.3%)	
Ideology							
Conservative	30 (36.6%)	34 (41.0%)	28 (32.9%)	27 (36.5%)	15 (23.8%)	15 (24.2%)	0.167
Liberal	52 (63.4%)	49 (59.0%)	57 (67.1%)	47 (63.5%)	48 (76.2%)	47 (75.8%)	

Notes: This table shows the demographic composition (number of respondents and shares in %) for the Carne y Arena experimental subjects, and the p-values from a Pearson test of equality of those demographic shares between treatment arms.

Appendix Table A3: Balance Test, Carne y Arena Follow up Participants

	Baseline (on site)	Long-run (follow-up)	All (follow-up)	Test
Panel A: Dallas				
	83 (55.0%)	68 (45.0%)		
Gender				
Male	31 (37.3%)	16 (23.9%)		0.077
Female	52 (62.7%)	51 (76.1%)		
Bithplace				
US born	43 (51.8%)	24 (47.1%)		0.594
Foreign	40 (48.2%)	27 (52.9%)		
Ethnicity				
Non-Hispanic	55 (66.3%)	35 (68.6%)		0.777
Hispanic	28 (33.7%)	16 (31.4%)		
Ideology				
Conservative	30 (36.1%)	22 (43.1%)		0.420
Liberal	53 (63.9%)	29 (56.9%)		
Panel B: Omaha				
	82 (32.4%)	22 (8.7%)	149 (58.9%)	
Gender				
Male	34 (42.5%)	5 (22.7%)	52 (35.4%)	0.210
Female	46 (57.5%)	17 (77.3%)	95 (64.6%)	
Birthplace				
US born	52 (63.4%)	17 (77.3%)	110 (73.8%)	0.195
Foreign	30 (36.6%)	5 (22.7%)	39 (26.2%)	
Ethnicity				
Non-Hispanic	64 (78.0%)	19 (86.4%)	122 (81.9%)	0.622
Hispanic	18 (22.0%)	3 (13.6%)	27 (18.1%)	
Ideology				
Conservative	30 (36.6%)	6 (27.3%)	43 (28.9%)	0.439
Liberal	52 (63.4%)	16 (72.7%)	106 (71.1%)	

Notes: This table shows the demographic composition (number of respondents and shares in %) for the follow-up surveys of the Carne y Arena experimental subjects, and the p-values from a Pearson test of equality of those demographic shares between treatment arms.

Appendix Table A4: Balance Test, Information and Perceived Similarity

	Control	Treatment	Total	Test
	750 (49.8%)	755 (50.2%)	1,505 (100.0%)	
Gender				
Male	314 (43.1%)	313 (42.1%)	627 (42.6%)	0.697
Female	415 (56.9%)	431 (57.9%)	846 (57.4%)	
Birthplace				
US born	593 (79.1%)	596 (78.9%)	1,189 (79.0%)	0.952
Foreign	157 (20.9%)	159 (21.1%)	316 (21.0%)	
Ethnicity				
Non-Hispanic	698 (93.1%)	692 (91.7%)	1,390 (92.4%)	0.303
Hispanic	52 (6.9%)	63 (8.3%)	115 (7.6%)	
Ideology				
Conservative	303 (40.4%)	317 (42.0%)	620 (41.2%)	0.532
Liberal	447 (59.6%)	438 (58.0%)	885 (58.8%)	

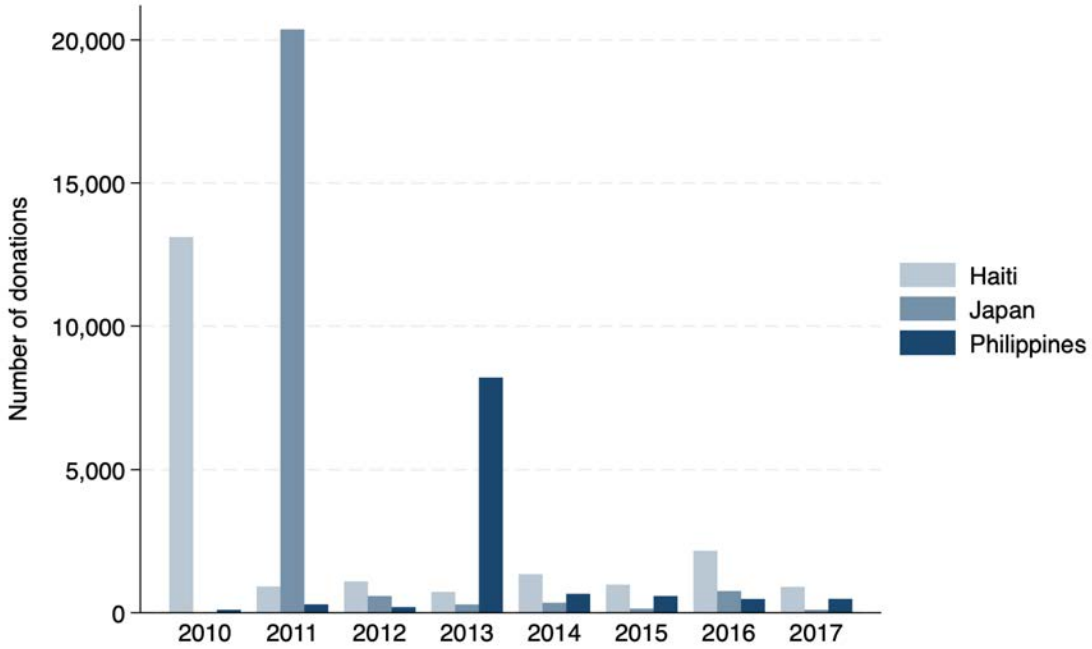
Notes: This table shows the demographic composition (number of respondents and shares in %) for the online experimental subjects on perceived similarity, and the p-values from a Pearson test of equality of those demographic shares between treatment arms.

Appendix Table A5: Descriptive statistics, Information and Perceived Similarity

	Summary	CCES shares
	7,173	
Gender		
Female	3,531 (50.3%)	56.7%
Male	3,489 (49.7%)	43.3%
Birthplace		
Foreign	1,551 (21.6%)	17%
US born	5,622 (78.4%)	83%
Ethnicity		
Hispanic	483 (6.7%)	2.8%
Non-Hispanic	6,690 (93.3%)	97.2%
Ideology		
Liberal	3,981 (55.5%)	33.6%
Conservative	3,192 (44.5%)	66.4%

Notes: This table presents descriptive statistics for our online survey on perceived similarity, and compares the demographic composition (number of respondents and shares in %) of our survey respondents to that of the US as a whole, using data from the Cooperative Congressional Election Study (CCES). Note that 153 respondents listed their gender as "other," so that female and male responses do not add up to the total number of responses.

Appendix Figure A1: Total number of donations to each country per year



Notes: This figure shows the total number of charitable donations, from all US counties combined, towards each of the three countries in our sample (Haiti, Japan, and the Philippines), over our sample period, 2010-17. The main events are: for Haiti the 2010 earthquake and subsequent cholera epidemic; for Japan the 2011 Tohoku earthquake, tsunami, and nuclear disaster; and for the Philippines the 2013 Bohol earthquake and super typhoon Yolanda.

Appendix Table A6: 2-dimensional representation of perceived distances

	Perceived bilateral distances:		
Panel A:	very conservative respondents		
	Japan	Philippines	Haiti
Self	0.367	0.692	0.865
Japan		0.583	1.017
Philippines			0.634
Panel B:	conservative respondents		
	Japan	Philippines	Haiti
Self	0.328	0.498	0.688
Japan		0.426	0.915
Philippines			0.672
Panel C:	moderate respondents		
	Japan	Philippines	Haiti
Self	0.354	0.488	0.647
Japan		0.504	0.925
Philippines			0.596
Panel D:	liberal respondents		
	Japan	Philippines	Haiti
Self	0.351	0.465	0.628
Japan		0.510	0.921
Philippines			0.587
Panel E:	very liberal respondents		
	Japan	Philippines	Haiti
Self	0.335	0.471	0.569
Japan		0.534	0.819
Philippines			0.425

Notes: We partition respondents to our survey on contact and similarity into five political groups ('very conservative,' 'conservative,' 'moderate,' 'liberal,' and 'very liberal') according to their answer to the question "On policy matters, where do you see yourself on the liberal/conservative spectrum?" Using our measure of perceived similarity between respondent i and foreign country f (from 0 to 5), we define $Distance_{i,f} = 5 - Similarity_{i,f}$. For each group separately, we perform a two-dimensional scaling exercise, and recover the positions of each respondent, Haiti, Japan, and the Philippines. For each group, the table presents the matrix of bilateral distances between the centroid of all respondents ('Self'), Haiti, Japan, and the Philippines. We omit the diagonal ($Distance_{k,k} = 0$ by construction), and the lower triangle ($Distance_{k,l} = Distance_{l,k}$). We use those bilateral distances to construct figure 5.

Appendix Table A7: Information, Carne y Arena, and Attitudes, Heterogeneity

	(1) CyA		(2) CyA *then* Info		(3) Info *then* CyA	
Baseline:	0.319***	(0.106)	0.361***	(0.103)	0.703***	(0.139)
Women:	0.324**	(0.138)	0.325**	(0.130)	0.928***	(0.208)
Men:	0.379**	(0.165)	0.428**	(0.172)	0.550**	(0.222)
p-value	0.800		0.631		0.217	
Hispanic:	0.252	(0.196)	0.293	(0.188)	0.603*	(0.326)
non-Hispanic:	0.338***	(0.124)	0.375***	(0.121)	0.752***	(0.164)
p-value	0.711		0.717		0.685	
Foreign:	0.045	(0.162)	0.139	(0.156)	0.420	(0.263)
Native:	0.524***	(0.136)	0.529***	(0.132)	0.886***	(0.180)
p-value	0.024		0.058		0.146	
Liberal:	0.386***	(0.129)	0.437***	(0.124)	0.633***	(0.172)
Conservative:	0.258	(0.165)	0.258	(0.161)	0.582**	(0.272)
p-value	0.542		0.380		0.876	

Notes: This table shows estimates of various specifications of equation (3), exploring the heterogeneity of the various treatment effects on attitudes in favor of migrants across types. The control group is always 'Baseline.' The treatment groups are: 'CyA' in column 1; 'CyA *then* Info' in column 2; 'Info *then* CyA' in column 3. The top panel (Baseline) reproduces the results from table 2 for comparison. The other panels present separate regressions for each type: female versus male respondents, Hispanic versus non-Hispanic respondents, foreign versus US-born respondents, and liberal versus conservative respondents. The p-values are from a test of equality of the treatment effects between types. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A8: Information and Perceived Similarity, Heterogeneity

	(1) No weights		(2) US weights		(3) CyA weights	
Baseline:	0.138***	(0.053)	0.191***	(0.072)	0.197***	(0.060)
Female:	0.172**	(0.069)	0.216**	(0.107)	0.208***	(0.078)
Male:	0.089	(0.081)	0.168*	(0.098)	0.175**	(0.089)
p-value	0.434		0.740		0.781	
Hispanic:	0.456**	(0.191)	0.473***	(0.180)	0.455**	(0.181)
non-Hispanic:	0.113**	(0.055)	0.117	(0.078)	0.125**	(0.057)
p-value	0.084		0.070		0.081	
Foreign:	0.189	(0.115)	0.287*	(0.147)	0.338**	(0.133)
Native:	0.124**	(0.059)	0.156*	(0.083)	0.144**	(0.065)
p-value	0.618		0.440		0.192	
Liberal:	0.122*	(0.068)	0.162	(0.100)	0.169**	(0.075)
Conservative:	0.168**	(0.082)	0.197**	(0.092)	0.232**	(0.099)
p-value	0.667		0.797		0.614	

Notes: This table shows estimates of various specifications of equation (3), exploring the heterogeneity of the information treatment effect on perceived similarity across types. Observations are un-weighted in column 1; weighted to match the demographics of the US population in column 2; weighted to match the demographics of visitors to Carne y Arena in column 3. The top panel (Baseline) reproduces the results from table 3 for comparison. The other panels present separate regressions for each type: female versus male respondents, Hispanic versus non-Hispanic respondents, foreign versus US-born respondents, and liberal versus conservative respondents. The p-values are from a test of equality of the treatment effects between types. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A9: Persistence of Carne y Arena

	(1) only CyA	(2) CyA
Long run *vs* short run	0.410*	0.408***
	(0.221)	(0.095)
Observations	187	362
p-value Robust S.E.	0.065	< 0.01
p-value Wild Bootstrap	0.039	< 0.01
p-value Permutation test	0.060	< 0.01

Notes: This table shows estimates of the treatment effect of Carne y Arena on long-term (2 months) attitudes in favor of migrants. The dependent variable is the attitude index, measured either in a follow-up survey (treatment group) or on site (control group). The control group in both columns is 'baseline,' for which we measure attitudes on site. The treatment group in column 1 is 'Long run', for which we measure attitudes in a follow-up survey two months after the Carne y Arena visit. The treatment group for column 2 combines 'long run,' 'CyA,' 'Info,' 'CyA *then* Info,' and 'Info *then* CyA,' for which we measure attitudes in a follow-up survey two months after the Carne y Arena visit (see protocol in table 1). Robust standard errors are in parentheses. *p*-values are computed using robust standard errors, wild bootstrap (Wu, 1986), and a permutation test (Young, 2019). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A10: Anchoring

	(1) Follow-up attitudes
CyA *vs* Long-run	-0.171
	(0.255)
Observations	64
p-value Robust S.E.	0.506
p-value Wild Bootstrap	0.479
p-value Permutation test	0.600

Notes: This table tests for the presence of anchoring of attitudes in the long term (2 months). The dependent variable is the attitude index, measured in a follow-up survey 2 months after the visit to Carne y Arena. The control group is 'Long run' and the treatment group is 'CyA' (see protocol in table 1). Robust standard errors are in parentheses. *p*-values are computed using robust standard errors, wild bootstrap (Wu, 1986), and a permutation test (Young, 2019). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A11: Information and Attitudes, On Site vs Online

	(1)	(2)
	Attitudes	
	Online	On site
Info	0.028	0.404***
	(0.052)	(0.127)
Observations	1505	250
p-value Robust S.E.	0.588	< 0.01
p-value Wild Bootstrap	0.589	< 0.01
p-value Permutation test	0.440	< 0.01

Notes: This table shows estimates of various specifications of equation (3). The dependent variable is the attitude index, measured either online (column 1) or on site (column 2). The control group is 'Control' in column 1 and 'Baseline' in column 2. The treatment group is 'Treatment' in column 1 and 'Info' in column 2 (see protocol in table 1). Robust standard errors are in parentheses. *p*-values are computed using robust standard errors, wild bootstrap (Wu, 1986), and a permutation test (Young, 2019). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A12: Treatment Effect of CyA on Information Retention

	(1) All Information	(2) Negative Information	(3) Positive Information	(4) Emotional Information
CyA	0.021	0.009	0.030	0.024
	(0.031)	(0.042)	(0.035)	(0.039)
Dallas	0.084**	0.132***	0.065	0.055
	(0.036)	(0.049)	(0.041)	(0.045)
Constant	0.546***	0.586***	0.526***	0.527***
	(0.018)	(0.023)	(0.020)	(0.022)
p-value Robust S.E.	0.505	0.835	0.393	0.541
p-value Wild Bootstrap	0.489	0.821	0.386	0.524
p-value Permutation test	0.520	0.720	0.580	0.620
Observations	405	405	405	405

Notes: This table shows estimates of various specifications of equation (3), where we estimate the treatment effect of Carne y Arena on the ability of subjects to retain information, controlling for differences between Dallas and Omaha subjects. The dependent variable is the share of correct answers on all information in column 1 (12 questions), 'negative' information in column 2 (4 questions), 'positive' information in column 3 (4 questions), and 'emotional' information in column 4 (4 questions). Robust standard errors are in parentheses. *p*-values are computed using robust standard errors, wild bootstrap (Wu, 1986), and a permutation test (Young, 2019). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A13: Information, Carne y Arena, and Attitudes, Dallas vs Omaha

	(1) CyA	(2) CyA *then* Info	(3) Info *then* CyA	(4) Info
Panel A: Dallas and Omaha combined				
Treatment	0.319***	0.361***	0.703***	0.404***
	(0.106)	(0.103)	(0.139)	(0.127)
Observations	347	326	228	250
p-value Robust S.E.	< 0.01	< 0.01	< 0.01	< 0.01
p-value Wild Bootstrap	< 0.01	< 0.01	< 0.01	< 0.01
p-value Permutation test	< 0.01	< 0.01	< 0.01	< 0.01
Panel B: only Dallas				
Treatment	0.344**	0.456***		
	(0.138)	(0.134)		
Observations	182	170		
p-value Robust S.E.	0.014	< 0.01		
p-value Wild Bootstrap	0.019	< 0.01		
p-value Permutation test	< 0.01	< 0.01		
Panel C: only Omaha				
Treatment	0.290*	0.250	0.703***	0.404***
	(0.163)	(0.158)	(0.152)	(0.143)
Observations	165	156	145	167
p-value Robust S.E.	0.078	0.115	< 0.01	< 0.01
p-value Wild Bootstrap	0.061	0.104	< 0.01	< 0.01
p-value Permutation test	0.020	0.140	< 0.01	< 0.01

Notes: This table shows estimates of various specifications of equation (3) for Dallas and Omaha subjects combined (panel A), for Dallas subjects only (panel B), and for Omaha subjects only. For comparison, panel A is an exact reproduction of table 2 (columns 1-3) and appendix table A11 (information treatment on site, column 4). The dependent variable is an index of attitudes in favor of migrants. The control group is always 'Baseline.' The treatment groups are: 'CyA' in column 1; 'Info' in column 2; 'CyA *then* Info' in column 3; 'Info *then* CyA' in column 4 (see protocol in table 1). Robust standard errors are in parentheses. *p*-values are computed using robust standard errors, wild bootstrap (Wu, 1986), and a permutation test (Young, 2019). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A14: Perceived Similarity and Donations, Alternative Std. Errors

	(1) Perceived Similarity	(2) IHS(# Donations)
Contact	1.318**	1.220**
Robust SE	(0.744)	(0.374)
Clustered on county level	(0.648)	(0.610)
Clustered on country level	(0.528)	(0.445)
Clustered on country-county level	(0.566)	(0.496)

Notes: This table shows various alternative measures of standard errors for our main estimates, the impact of contact on perceived similarity (table 5 column 3) and on charitable donations (table 4 column 3): robust standard errors, standard errors clustered at the domestic county level, clustered at the foreign country level, and two-way clustered at the county and country level (our baseline). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A15: Perceived Similarity and Donations, OLS vs IV

	(1) Perceived Similarity	(2) IHS(# Donations)
Panel A: IV		
Contact	1.318**	1.220**
	(0.566)	(0.496)
Observations	6223	6223
First-stage F-statistic	5.314	5.314
Weak IV-robust p-value	< 0.01	< 0.01
Panel B: OLS		
Contact	0.128***	0.005
	(0.029)	(0.010)
Observations	6997	6223

Notes: The table presents IV (panel A) and OLS (panel B) estimates of the impact of contact on perceived similarity, equation (5) in column 1, and the impact of contact on charitable donations, equation (4) in column 2, controlling for county and country fixed effects in both panels and where contact in panel A is instrumented using the first stage equation (6). Standard errors, in parentheses, are clustered at the country and county level. We report in panel A weak IV-robust p-values (Andrews et al., 2007). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A16: Alternative Measures of Contact

	(1) Friends	(2) Neighbor	(3) Co-workers	(4) Any
Panel A:	IHS(# Donations)			
Contact	0.555	0.599	1.877**	1.220**
	(0.669)	(0.676)	(0.802)	(0.496)
Observations	6223	6223	6223	6223
First-stage F-statistic	6.436	9.293	4.466	5.314
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01
Panel B:	Perceived Similarity			
Contact	1.838***	2.392***	1.610	1.318**
	(0.639)	(0.778)	(1.062)	(0.566)
Observations	6223	6223	6223	6223
First-stage F-statistic	6.436	9.293	4.466	5.314
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01

Notes: The table presents IV estimates of the impact of contact on charitable donations, equation (4) in panel A, and the impact of contact on perceived similarity, equation (5) in panel B, where contact is instrumented using the first stage equation (6), for different measures of contact: friends in column 1, neighbors in column 2, coworkers in column 3, and at any of those three categories of contact in column 4 (our baselines in tables 4 and 5, column 3). Standard errors, in parentheses, are clustered at the country and county level. We also report weak IV-robust p-values (Andrews et al., 2007). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A17: Alternative Durations of Residency

	(1)	(2)	(3)	(4)
	Perceived Similarity			
Duration of residency	Short	Medium	Long	All
Contact	0.749	0.279	0.731*	1.318**
	(0.796)	(0.484)	(0.411)	(0.566)
Observations	2028	2232	1963	6223
First-stage *F-statistic*	1.836	4.572	5.679	5.314
Weak IV-robust *p*-value	< 0.01	0.066	< 0.01	< 0.01

Notes: The table presents IV estimates of the impact of contact on perceived similarity, equation (5), where contact is instrumented using the first stage equation (6), for respondents who have lived in their current county of residence for different durations: less than 10 years (column 1), 10 to 30 years (column 2), more than 30 years (column 3), or any duration (column 4, which reproduces our baseline from table 5 column 3). Standard errors, in parentheses, are clustered at the country and county level. We also report weak IV-robust *p*-values (Andrews et al., 2007). *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A18: Contact and Perceived Similarity, Individual Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Perceived Similarity						
Contact	1.318**	1.318**	1.313**	1.319**	1.318**	1.317**	1.303**
	(0.566)	(0.566)	(0.566)	(0.566)	(0.566)	(0.567)	(0.653)
Observations	6223	6223	6223	6223	6223	6223	6223
Fist-stage F-statistic	5.314	5.317	5.317	5.295	5.311	5.295	4.173
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	0.034	< 0.01
Individual controls:							
Gender	·	✓	·	·	·	✓	·
Ethnicity	·	·	✓	·	·	✓	·
Birthplace	·	·	·	✓	·	✓	·
Ideology	·	·	·	·	✓	✓	·
Individual FE	·	·	·	·	·	·	✓

Notes: The table presents IV estimates of the impact of contact on perceived similarity, equation (5), where contact is instrumented using the first stage equation (6), using various combinations of individual controls: none in column 1 (our baseline from table 5 column 3), gender in column 2, ethnicity in column 3, foreign or domestic birthplace in column 4, liberal or conservative ideology in column 5, all controls together in column 6, and controlling for individual fixed effects in column 6. Standard errors, in parentheses, are clustered at the country and county level. We also report weak IV-robust p-values (Andrews et al., 2007). *** p<0.01, ** p<0.05, * p<0.1.

B Carne y Arena Survey Questionnaire

The printout version of the Carne y Arena questionnaire below corresponds to a respondent assigned to our control group ('Baseline') at Kaneko in Omaha, Nebraska (the questionaire at Fair Park in Dallas, Texas, is identical but for the last page). They answer questions about their attitudes towards immigration first, receive the information treatment second, and visit Carne y Arena third. Across respondents, the ordering of those three blocks is randomized. See table 1 for further details on the experimental protocol.

Email

Please enter your email address

Demographic Block

What is your gender?

- Male
- Female
- Other

In what year were you born?

What was your TOTAL household income, before taxes, last year?

- $0-$9,999
- $10,000-$14,999
- $15,000-$19,999
- $20,000-$29,999
- $30,000-$39,999
- $40,000-$49,999
- $50,000-$69,999
- $70,000-$89,999
- $90,000-$109,999
- $110,000-$149,999
- $150,000-$199,999
- $200,000+

Please indicate your marital status

- Single
- Married
- Legally separated or divorced
- Widowed

How many children do you have?

- I do not have children
- 1
- 2
- 3
- 4

- ○ 5 or more

What racial or ethnic group best describes you?

- ○ White
- ○ Black or African-American
- ○ Hispanic or Latino
- ○ Asian or Asian-American
- ○ Native American
- ○ Middle Eastern
- ○ Mixed Race
- ○ Other

Were you born in the United States?

- ○ Yes
- ○ No

Where were you born?

Were both of your parents born in the United States?

- Yes
- No

Where was your father born?

Where was your mother born?

What is your ZIP code?

Which category best describes your highest level of education?

- Eighth Grade or less
- Some High School

- ○ High School degree / GED
- ○ Some College
- ○ 2-year College Degree
- ○ 4-year College Degree
- ○ Master's Degree
- ○ Doctoral Degree; Professional Degree (JD, MD, MBA)

What is your current employment status?

- ○ Full-time employee
- ○ Part-time employee
- ○ Self-employed or small business owner
- ○ Unemployed and looking for work
- ○ Student
- ○ Not currently working and not looking for work
- ○ Retiree

What is your current occupation?

Even if you are not currently working, what was latest

occupation?

On policy matters, where do you see yourself on the liberal/conservative spectrum?

- Very liberal
- Liberal
- Moderate
- Conservative
- Very conservative

In politics, as of today, do you consider yourself a Republican, a Democrat or an independent?

- Republican
- Democrat
- Independent

Did you vote in the last presidential election?

- Yes

- ○ No

In the last presidential election, you supported:

- ○ Joe Biden
- ○ Donald Trump
- ○ Other

Even if you did NOT vote, please indicate the candidate that you were most likely to have voted for or who represents your views most closely

- ○ Joe Biden
- ○ Donald Trump
- ○ Other

How often do you visit art exhibitions/events?

- ○ Very frequently
- ○ Frequently
- ○ Occasionally
- ○ Rarely
- ○ Never

Have you experienced Virtual Reality before?

- ○ Yes
- ○ No

Attention check

How many states are there in the U.S.?

- ○ 5
- ○ 50
- ○ 100
- ○ 10

Views on immigration

Consider the policy proposals listed below. Which ones do you think the U.S. government should implement?

Select all that apply.

- ☐ Increase the number of asylum seekers admitted to the U.S.

- [ ] Eliminate the estate tex.
- [ ] Shift from a more family-based to a more merit-based immigration system.
- [ ] Raise the federal minimum wage to $15 an hour.
- [ ] Cap carbon emissions to combat climate change.
- [ ] Allow employers to decline coverage of abortions in insurance plans.
- [ ] Remove barriers to domestic oil and gas drilling.
- [ ] Amend federal laws to prohibit discrimination on the basis of gender identity and sexual orientation.
- [ ] Increase the number of border patrols on the US-Mexican border.
- [ ] Pass the DREAM Act, granting resident status to unauthorized immigrants who entered the US as minors.
- [ ] None of the above.

How would you rank the policies you selected in terms of priority of the U.S. government (where 1 indicates highest priority)?

Drag and drop the items to the desired order. Skip if you only chose 1 item.

» Pass the DREAM Act, granting resident status to unauthorized immigrants who entered the US as minors.

» Amend federal laws to prohibit discrimination on the basis of gender identity and sexual orientation.

» Raise the federal minimum wage to $15 an hour.

» Cap carbon emissions to combat climate change.

» Increase the number of border patrols on the US-Mexican border.

» Eliminate the estate tex.

» Increase the number of asylum seekers admitted to the U.S.

» Remove barriers to domestic oil and gas drilling.

» Shift from a more family-based to a more merit-based immigration system.

» Allow employers to decline coverage of abortions in insurance plans.

» None of the above.

Which of the following policies best represents your views on unauthorized immigration?

- ○ All unauthorized immigrants should be granted full U.S. citizenship, without any conditions.
- ○ All unauthorized immigrants should be given a pathway to earn U.S. citizenship.
- ○ All unauthorized immigrants brought here as children should be given a pathway to earn U.S. citizenship.

- ○ No unauthorized immigrant should be given a pathway to earn U.S. citizenship.
- ○ All unauthorized immigrants should be deported.

We would like to make a donation of $2 on your behalf to a good cause.

Which of the following charities would you like to donate to?

- ○ The **Natural Resources Defense Council** (NRDC):
  Works to safeguard the earth -- its people, its plants and animals, and the natural systems on which all life depends.
- ○ The **Humane Society of the U.S.** (HSUS):
  Works to end the cruelest practices toward all animals, care for animals in crisis and build a stronger animal protection movement.
- ○ The **Refugee and Immigrant Center for Education and Legal Services** (RAICES):
  A nonprofit agency that promotes justice by providing free and low-cost legal services to underserved immigrant children, families, and refugees.

Financial incentive

On the next page, we will show you information related to immigration and border security. You will later be asked to complete a quiz on this information.

If you answer correctly to more than 70% of quiz questions, you will be entered into a lottery for a **$100 Amazon gift card**.

Information Treatment

In this section we will show you information related to the current situation on the Southwest border and the number and characteristics of unauthorized immigrants living in the U.S.

The statistical sources for this information, and the years it refers to, are:

Border apprehensions: **U.S. Customs and Border Protection**, Department of Homeland Security (fiscal years 2015 to 2020).

Number and characteristics of unauthorized immigrants living in the U.S.: **U.S. Census Bureau** and **Migration Policy Institute** (2015 to 2019).

Crime rates in Texas: **Texas Department of Public Safety** (2015 to 2019).

Cross-country living standards: **World Bank** (2020).

Cross-country crime rates: **United Nations Office on Drugs and Crime** (2018).

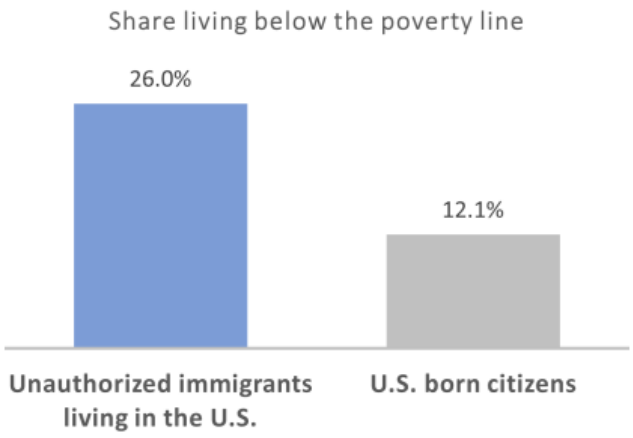
In the fiscal year 2020, U.S. Customs and Border Protection apprehended a total of **400,651 people** on the Southwest border.

In the fiscal year 2020, U.S. Customs and Border Protection seized **287,000 pounds** of drugs on the Southwest border.

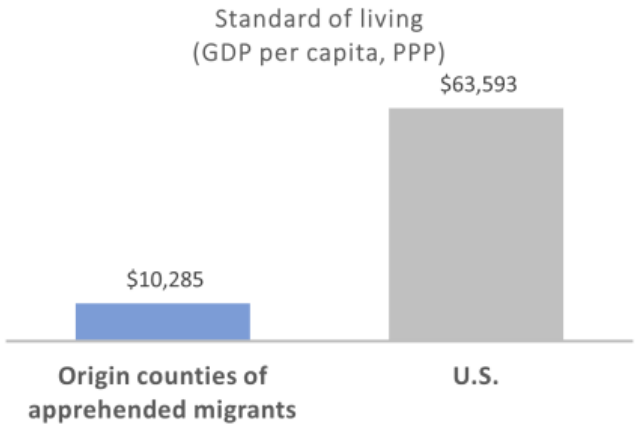
The number of unauthorized immigrants living in the U.S. is about **11 million**.

Unauthorized immigrants living in the U.S. are **2.5 times more likely** than U.S. born citizens to live below the poverty

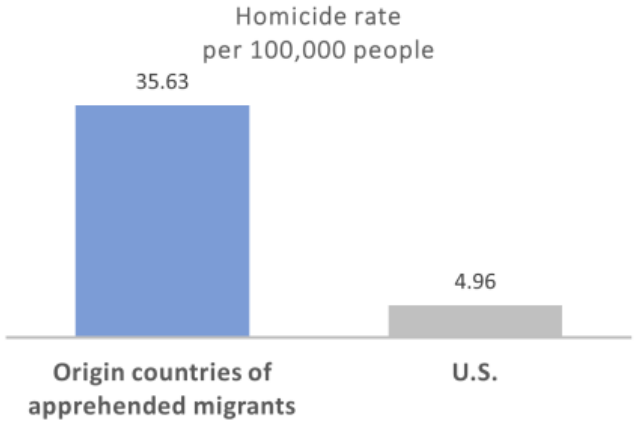
line.



The average standard of living in the top four origin countries of migrants apprehended on the Southwest border is **6 times lower** than that in the U.S.



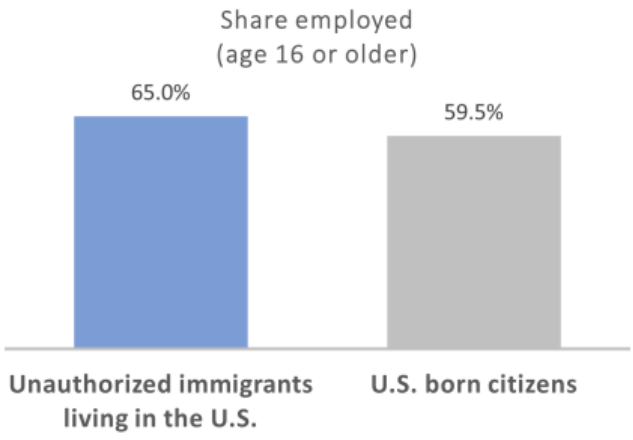
The average homicide rate in the top four origin countries of migrants apprehended on the Southwest border is **7 times higher** than that in the U.S.



In the fiscal year 2020, U.S. Customs and Border Protection apprehended **30,557** unaccompanied children under the age of 18 on the Southwest border.

In the fiscal years 2015 to 2020, U.S. Customs and Border Protection recorded **1,455** deaths on the Southwest border.

Unauthorized immigrants living in the U.S. are **as likely** as U.S. born citizens to be employed.

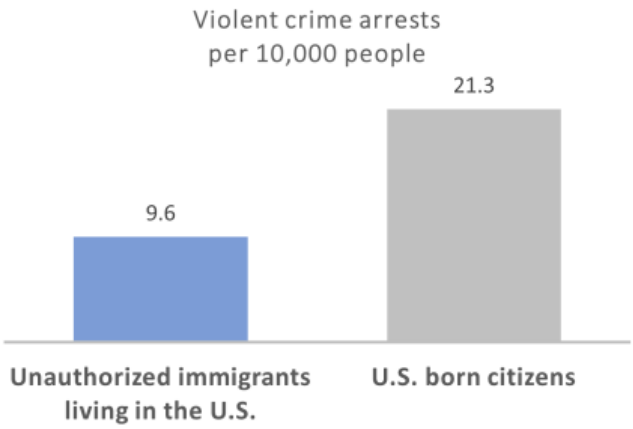


Unauthorized immigrants living in the U.S. have the following rights and obligations:

They are **required to file tax returns and pay taxes** on income earned in the U.S.

They are **NOT eligible** to receive Social Security benefits.

In Texas, unauthorized immigrants are **55% less likely** than U.S. born citizens to be arrested for a violent crime.



About **93%** of unauthorized immigrant children aged 13 to 17 who live in the U.S. are enrolled in high school.

Information Questions

Please answer the following questions based on the information you saw.

In the fiscal year 2020, how many people were apprehended by U.S. Customs and Border Protection on the Southwest border?

- ○ Between 180,000 and 300,000

- ○ Between 340,000 and 460,000
- ○ Between 660,000 and 780,000

In the fiscal year 2020, what was the volume of drugs seized by U.S. Customs and Border Protection on the Southwest border?

- ○ Between 260,000 lbs and 350,000 lbs
- ○ Between 10,000 lbs and 100,000 lbs
- ○ Between 130,000 lbs and 220,000 lbs

What is the number of unauthorized immigrants living in the U.S.?

- ○ Between 9 and 13 million
- ○ Between 18 and 21 million
- ○ Between 5 and 8 million

Which of the following statements is correct?

- ○ Unauthorized immigrants are
  2 times more likely than U.S. born citizens to live below the poverty line.
- ○ Unauthorized immigrants are
  3 times more likely than U.S. born citizens to live below the poverty line.

- Unauthorized immigrants are 4 times more likely than U.S. born citizens to live below the poverty line.

Which of the following statements is correct?

- The standard of living in the origin countries of apprehended migrants is about the same as that in the U.S.
- The standard of living in the origin countries of apprehended migrants is 10 times lower than that in the U.S.
- The standard of living in the origin countries of apprehended migrants is 6 times lower than that in the U.S.

Which of the following statements is correct?

- The homicide rate in the origin countries of apprehended migrants is 11 times higher than that in the U.S.
- The homicide rate in the origin countries of apprehended migrants is 7 times higher than that in the U.S.
- The homicide rate in the origin countries of apprehended migrants is 3 times higher than that in the U.S.

In the fiscal year 2020, how many unaccompanied children under the age of 18 were apprehended by U.S. Customs and Border Protection on the Southwest border?

- ○ Between 26,000 and 35,000
- ○ Between 50,000 and 59,000
- ○ Between 2,000 and 11,000

How many deaths did U.S. Customs and Border Protection record on the Southwest border in the fiscal years 2015 to 2020?

- ○ Between 2,500 and 2,900
- ○ Between 1,300 and 1,700
- ○ Between 1,900 and 2,300

Which of the following statements is correct?

- ○ U.S. born citizens are
  2 times more likely than unauthorized immigrants to be employed.
- ○ Unauthorized immigrants are
  2 times more likely than U.S. born citizens to be employed.
- ○ Unauthorized immigrants are
  as likely as U.S. born citizens to be employed.

Are unauthorized immigrants eligible to receive Social Security benefits?

- ○ Yes
- ○ No

Are unauthorized immigrants required to file tax returns and pay taxes on income earned in the U.S.?

- ○ Yes
- ○ No

Which of the following statements is correct?

- ○ In Texas, unauthorized immigrants are 1.5 times more likely than U.S. born citizens to be arrested for a violent crime.
- ○ In Texas, unauthorized immigrants are 2 times more likely than U.S. born citizens to be arrested for a violent crime.
- ○ In Texas, unauthorized immigrants are 55% less likely than U.S. born citizens to be arrested for a violent crime.

What share of unauthorized immigrant children of age 13 to 17 are enrolled in high school?

- ○ Between 30% and 40%
- ○ Between 70% and 80%
- ○ Over 90%

How sure are you about your answers to the above questions?

- ○ Very sure
- ○ Sure
- ○ Somewhat sure
- ○ Unsure
- ○ Very unsure

Intermediate Carne y Arena Page

You have **completed Part 1**.

You can now enter the Carne y Arena exhibit!

Please remember to **come back for Part 2** after the exhibit.

You can now continue to Part 2.

Emotional Response to Carne y Arena

How would you describe your "Carne y Arena" experience in a few words / sentences?

How strong was your emotional reaction to "Carne y Arena", on a scale from 1 (neutral) to 10 (very strong)?

How would you rate the artistic value of the "Carne y Arena" experience, on a scale from 1 to 10?

End Page: Went to Carne y Arena

We thank you for participating in this study!

As a token of our appreciation, we offer you a chance to receive a **FREE annual membership** to KANEKO.

In about 2 weeks, we will send you an email with a chance to receive a free membership, and we will invite you to answer a few additional questions. Please check your email.

C Similarity Survey Questionnaire

The printout version of the contact and perceived similarity questionnaire below corresponds to a respondent assigned to our treatment group ('Treatment'). They receive our information treatment first and then answer questions about their perceived similarity to an unauthorized migrant from Mexico. Respondents in the control group receive a similar survey but without the information treatment. See table 1 for further details on the experimental protocol.

Consent

University of Chicago Online Consent Form for Research Participation

Study Number: IRB22-0551
Study Title: Emotion
Researchers: Marianne Andries, Leonardo Bursztyn, Thomas Chaney, Milena Djourelova
Collaborating Institutions: University of Southern California

Description: We are researchers at the University of Chicago and the University of Southern California doing a research study about the power of art. Participation should take up between 5 and 10min in total. Your participation is voluntary.

Incentives: You may be offered a chance to win a **$100 Amazon gift**. The giftcard will be sent to the winner by email, within two months from completing the survey.

Risks and Benefits: Your participation in this study does not involve any risk to you beyond that of everyday life. Taking part in this research study may not only benefit you

personally by giving you an opportunity to visit a groundbreaking art installation -- it may also help us learn new things that could help others.

Confidentiality: Identifiable data will never be shared outside the research team. All personally identifying information collected about you will be removed or changed before files are shared with other researchers or results are made public, and destroyed once it is no longer needed for the study.

Contacts & Questions: If you have questions or concerns about the study, you can contact the researchers at mdjourelova@uchicago.edu (Milena Djourelova). If you have any questions about your rights as a participant in this research, feel you have been harmed, or wish to discuss other study-related concerns with someone who is not part of the research team, you can contact the University of Chicago Social & Behavioral Sciences Institutional Review Board (IRB) Office by phone at (773) 702-2915, or by email at sbs-irb@uchicago.edu.

Consent: Participation is voluntary. Refusal to participate or withdrawing from the research will involve no penalty or loss of benefits to which you might otherwise be entitled. By clicking ``agree" below, you confirm that you have read the consent form, are at least 18 years old, and agree to

participate in the research. Please print or save a copy of this page for your records. If you do not agree to participate, please exit the survey by closing this link.

○ Agree

Prolific ID

What is your Prolific ID?
Please note that this response should auto-fill with the correct ID

${e://Field/PROLIFIC_PID}

Demographic Block

What is your gender?

○ Male

- ○ Female
- ○ Other

In what year were you born?

What was your TOTAL household income, before taxes, last year?

- ○ $0-$9,999
- ○ $10,000-$14,999
- ○ $15,000-$19,999
- ○ $20,000-$29,999
- ○ $30,000-$39,999
- ○ $40,000-$49,999
- ○ $50,000-$69,999
- ○ $70,000-$89,999
- ○ $90,000-$109,999
- ○ $110,000-$149,999
- ○ $150,000-$199,999
- ○ $200,000+

Please indicate your marital status

- ○ Single
- ○ Married
- ○ Legally separated or divorced

- ○ Widowed

How many children do you have?

- ○ I do not have children
- ○ 1
- ○ 2
- ○ 3
- ○ 4
- ○ 5 or more

What racial or ethnic group best describes you?

- ○ White
- ○ Black or African-American
- ○ Hispanic or Latino
- ○ Asian or Asian-American
- ○ Native American
- ○ Middle Eastern
- ○ Mixed Race
- ○ Other

Were you born in the United States?

- ○ Yes
- ○ No

Where were you born?

Were both of your parents born in the United States?

- ○ Yes
- ○ No

Where was your father born?

Where was your mother born?

What is your ZIP code?

Which category best describes your highest level of education?

- Eighth Grade or less
- Some High School
- High School degree / GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree
- Doctoral Degree; Professional Degree (JD, MD, MBA)

What is your current employment status?

- Full-time employee
- Part-time employee
- Self-employed or small business owner
- Unemployed and looking for work
- Student
- Not currently working and not looking for work

- ○ Retiree

What is your current occupation?

Even if you are not currently working, what was latest occupation?

On policy matters, where do you see yourself on the liberal/conservative spectrum?

- ○ Very liberal
- ○ Liberal
- ○ Moderate
- ○ Conservative
- ○ Very conservative

In politics, as of today, do you consider yourself a Republican, a Democrat or an independent?

- ○ Republican
- ○ Democrat
- ○ Independent

How often do you visit art exhibitions/events?

- ○ Very frequently
- ○ Frequently
- ○ Occasionally
- ○ Rarely
- ○ Never

Have you experienced Virtual Reality before?

- ○ Yes
- ○ No

Attention check

How many states are there in the U.S.?

- ○ 10
- ○ 5
- ○ 100
- ○ 50

Financial incentive

On the next page, we will show you information related to immigration and border security. You will later be asked to complete a quiz on this information.

If you answer correctly to more than 70% of quiz questions, you will be entered into a lottery for a **$100 Amazon gift card**.

Information Treatment

In this section we will show you information related to the current situation on the Southwest border and the number and characteristics of unauthorized immigrants living in the

U.S.

The statistical sources for this information, and the years it refers to, are:

Border apprehensions: **U.S. Customs and Border Protection**, Department of Homeland Security (fiscal years 2015 to 2020).

Number and characteristics of unauthorized immigrants living in the U.S.: **U.S. Census Bureau** and **Migration Policy Institute** (2015 to 2019).

Crime rates in Texas: **Texas Department of Public Safety** (2015 to 2019).

Cross-country living standards: **World Bank** (2020).

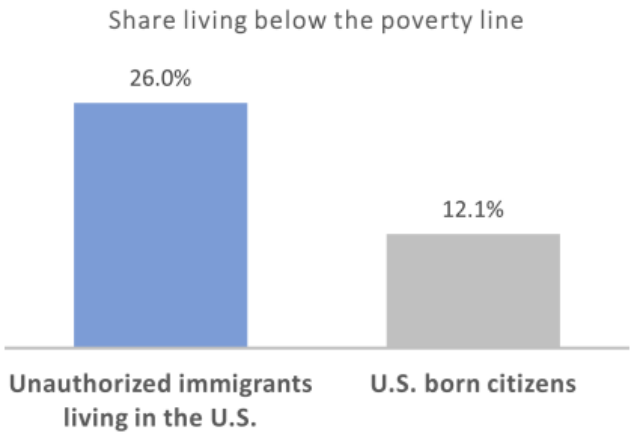
Cross-country crime rates: **United Nations Office on Drugs and Crime** (2018).

In the fiscal year 2020, U.S. Customs and Border Protection apprehended a total of **400,651 people** on the Southwest border.

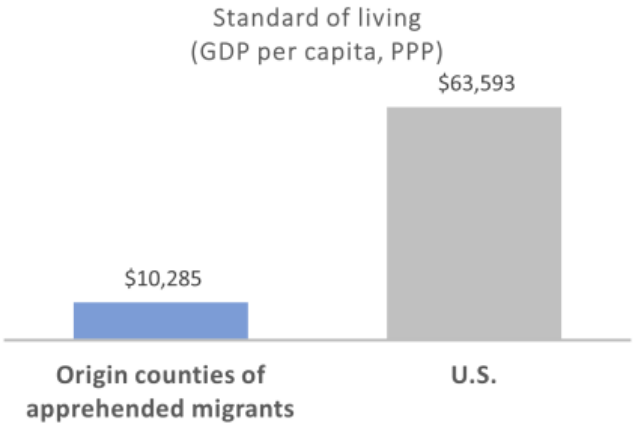
In the fiscal year 2020, U.S. Customs and Border Protection seized **287,000 pounds** of drugs on the Southwest border.

The number of unauthorized immigrants living in the U.S. is about **11 million**.

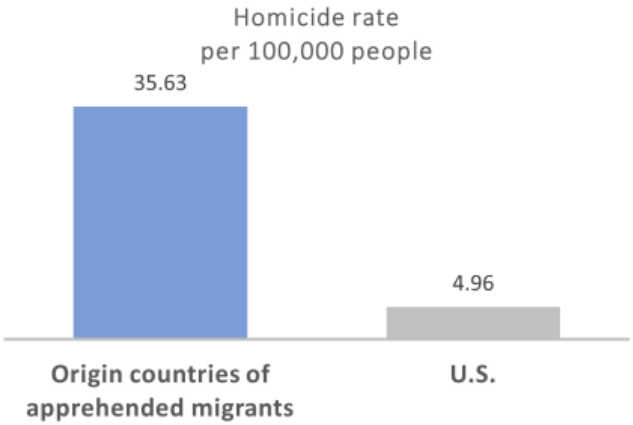
Unauthorized immigrants living in the U.S. are **2.5 times more likely** than U.S. born citizens to live below the poverty line.



The average standard of living in the top four origin countries of migrants apprehended on the Southwest border is **6 times lower** than that in the U.S.



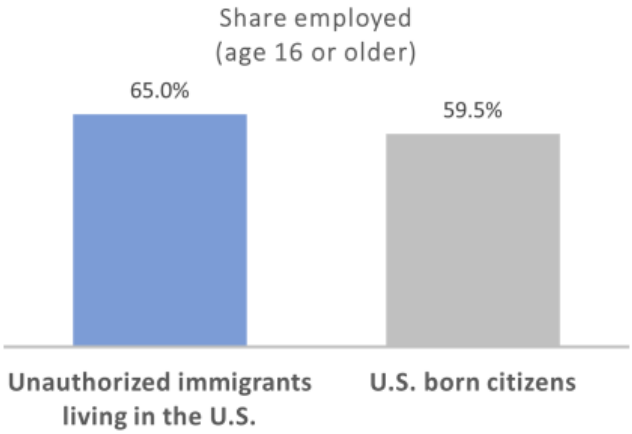
The average homicide rate in the top four origin countries of migrants apprehended on the Southwest border is **7 times higher** than that in the U.S.



In the fiscal year 2020, U.S. Customs and Border Protection apprehended **30,557** unaccompanied children under the age of 18 on the Southwest border.

In the fiscal years 2015 to 2020, U.S. Customs and Border Protection recorded **1,455** deaths on the Southwest border.

Unauthorized immigrants living in the U.S. are **as likely** as U.S. born citizens to be employed.

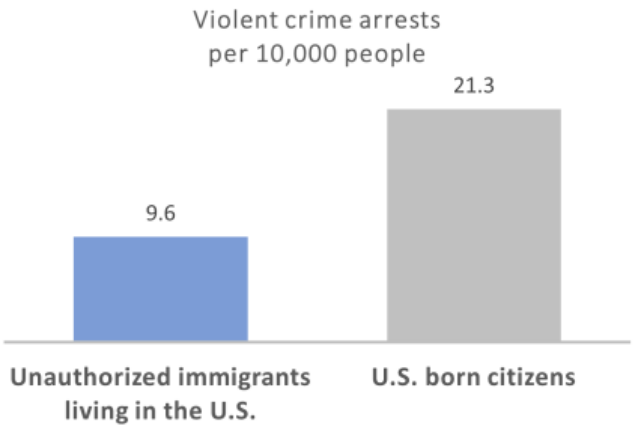


Unauthorized immigrants living in the U.S. have the following rights and obligations:

They are **required to file tax returns and pay taxes** on income earned in the U.S.

They are **NOT eligible** to receive Social Security benefits.

In Texas, unauthorized immigrants are **55% less likely** than U.S. born citizens to be arrested for a violent crime.



About **93%** of unauthorized immigrant children aged 13 to 17 who live in the U.S. are enrolled in high school.

Information Questions

Please answer the following questions based on the information you saw.

In the fiscal year 2020, how many people were apprehended by U.S. Customs and Border Protection on the Southwest border?

- ○ Between 20,000 and 140,000
- ○ Between 340,000 and 460,000
- ○ Between 660,000 and 780,000

In the fiscal year 2020, what was the volume of drugs seized by U.S. Customs and Border Protection on the Southwest border?

- ○ Between 260,000 lbs and 350,000 lbs
- ○ Between 10,000 lbs and 100,000 lbs
- ○ Between 130,000 lbs and 220,000 lbs

What is the number of unauthorized immigrants living in the U.S.?

- ○ Between 9 and 13 million
- ○ Between 18 and 21 million

- ○ Between 5 and 8 million

Which of the following statements is correct?

- ○ Unauthorized immigrants are 2 times more likely than U.S. born citizens to live below the poverty line.
- ○ Unauthorized immigrants are 4 times more likely than U.S. born citizens to live below the poverty line.
- ○ Unauthorized immigrants are as likely as U.S. born citizens to live below the poverty line.

Which of the following statements is correct?

- ○ The standard of living in the origin countries of apprehended migrants is about the same as that in the U.S.
- ○ The standard of living in the origin countries of apprehended migrants is 10 times lower than that in the U.S.
- ○ The standard of living in the origin countries of apprehended migrants is 6 times lower than that in the U.S.

Which of the following statements is correct?

- ○ The homicide rate in the origin countries of apprehended migrants is 11 times higher than that in the U.S.

- ○ The homicide rate in the origin countries of apprehended migrants is 7 times higher than that in the U.S.
- ○ The homicide rate in the origin countries of apprehended migrants is about the same as that in the U.S.

In the fiscal year 2020, how many unaccompanied children under the age of 18 were apprehended by U.S. Customs and Border Protection on the Southwest border?

- ○ Between 26,000 and 35,000
- ○ Between 14,000 and 23,000
- ○ Between 2,000 and 11,000

How many deaths did U.S. Customs and Border Protection record on the Southwest border in the fiscal years 2015 to 2020?

- ○ Between 1,900 and 2,300
- ○ Between 1,300 and 1,700
- ○ Between 700 and 1,100

Which of the following statements is correct?

- Unauthorized immigrants are
  4 times more likely than U.S. born citizens to be employed.
- Unauthorized immigrants are
  2 times more likely than U.S. born citizens to be employed.
- Unauthorized immigrants are
  as likely as U.S. born citizens to be employed.

Are unauthorized immigrants eligible to receive Social Security benefits?

- Yes
- No

Are unauthorized immigrants required to file tax returns and pay taxes on income earned in the U.S.?

- Yes
- No

Which of the following statements is correct?

- In Texas, unauthorized immigrants are
  2 times more likely than U.S. born citizens to be arrested for a violent crime.
- In Texas, unauthorized immigrants are
  75% less likely than U.S. born citizens to be arrested for a violent crime.

- ○ In Texas, unauthorized immigrants are 55% less likely than U.S. born citizens to be arrested for a violent crime.

What share of unauthorized immigrant children of age 13 to 17 are enrolled in high school?

- ○ Between 50% and 60%
- ○ Between 30% and 40%
- ○ Over 90%

How sure are you about your answers to the above questions?

- ○ Very sure
- ○ Sure
- ○ Somewhat sure
- ○ Unsure
- ○ Very unsure

Big 5 Quiz

Next, we would like to ask you some questions about your personality.

Below you will see a number of statements, each of which starts with "I see myself as someone who." For each statement, please indicate how much you agree with this.

I see myself as someone who:

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Tends to be quiet.	○	○	○	○	○
... Is compassionate, has a soft heart.	○	○	○	○	○
... Tends to be disorganized.	○	○	○	○	○
... Worries a lot.	○	○	○	○	○
... Is fascinated by art, music, or literature.	○	○	○	○	○

I see myself as someone who:

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Is dominant, acts as a leader.	○	○	○	○	○
... Is sometimes rude to others.	○	○	○	○	○
... Has difficulty getting started on tasks.	○	○	○	○	○

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Tends to feel depressed, blue.	○	○	○	○	○
... Has little interest in abstract ideas.	○	○	○	○	○

I see myself as someone who:

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Is full of energy.	○	○	○	○	○
... Assumes the best about people.	○	○	○	○	○
... Is reliable, can always be counted on.	○	○	○	○	○
... Is emotionally stable, not easily upset.	○	○	○	○	○
... Is original, comes up with new ideas.	○	○	○	○	○

Perceived similarity

We asked an **unauthorized immigrant** from Mexico to fill out the same personality quiz and computed their

standardized scores on five major personality traits.

How do you think your personality traits compare to theirs?

If your guess is correct, you will be entered into a lottery for a **$100 Amazon gift card**.

- ○ You have **0 out of 5** traits in common.
- ○ You have **1 out of 5** traits in common.
- ○ You have **2 out of 5** traits in common.
- ○ You have **3 out of 5** traits in common.
- ○ You have **4 out of 5** traits in common
- ○ You have **5 out of 5** traits in common.

Views on immigration

Consider the policy proposals listed below. Which ones do you think the U.S. government should implement?

Select all that apply.

- ☐ Amend federal laws to prohibit discrimination on the basis of gender identity and sexual orientation.
- ☐ Shift from a more family-based to a more merit-based immigration system.
- ☐ Increase the number of border patrols on the US-Mexican border.
- ☐ Remove barriers to domestic oil and gas drilling.

- [ ] Allow employers to decline coverage of abortions in insurance plans.
- [ ] Eliminate the estate tex.
- [ ] Increase the number of asylum seekers admitted to the U.S.
- [ ] Pass the DREAM Act, granting resident status to unauthorized immigrants who entered the US as minors.
- [ ] Cap carbon emissions to combat climate change.
- [ ] Raise the federal minimum wage to $15 an hour.
- [ ] None of the above.

How would you rank the policies you selected in terms of priority of the U.S. government (where 1 indicates highest priority)?

Drag and drop the items to the desired order. Skip if you only chose 1 item.

» Pass the DREAM Act, granting resident status to unauthorized immigrants who entered the US as minors.

» Amend federal laws to prohibit discrimination on the basis of gender identity and sexual orientation.

» Raise the federal minimum wage to $15 an hour.

» Cap carbon emissions to combat climate change.

» Increase the number of border patrols on the US-Mexican border.

» Eliminate the estate tex.

» Increase the number of asylum seekers admitted to the U.S.

» Remove barriers to domestic oil and gas drilling.

» Shift from a more family-based to a more merit-based immigration system.

» Allow employers to decline coverage of abortions in insurance plans.

» None of the above.

Which of the following policies best represents your views on unauthorized immigration?

- ○ All unauthorized immigrants should be granted full U.S. citizenship, without any conditions.
- ○ All unauthorized immigrants should be given a pathway to earn U.S. citizenship.
- ○ All unauthorized immigrants brought here as children should be given a pathway to earn U.S. citizenship.
- ○ No unauthorized immigrant should be given a pathway to earn U.S. citizenship.
- ○ All unauthorized immigrants should be deported.

We would like to make a donation of $2 on your behalf to a good cause.

Which of the following charities would you like to donate to?

- The **Natural Resources Defense Council** (NRDC):
  Works to safeguard the earth -- its people, its plants and animals, and the natural systems on which all life depends.
- The **Refugee and Immigrant Center for Education and Legal Services** (RAICES):
  A nonprofit agency that promotes justice by providing free and low-cost legal services to underserved immigrant children, families, and refugees.
- The **Humane Society of the U.S.** (HSUS):
  Works to end the cruelest practices toward all animals, care for animals in crisis and build a stronger animal protection movement.

End

You have completed the survey. Thank you for your participation!

D Contact Survey Questionnaire

Across respondents, for questions on both perceived similarity and contact, the ordering of specific foreign origins is randomized. In the printout below, the respondent answers questions about Japan first, Haiti second, and the Philippines third. Other respondents have different country orderings.

Prolific ID

What is your Prolific ID?
Please note that this response should auto-fill with the correct ID

${e://Field/PROLIFIC_PID}

Demographic Block

What is your gender?

- Male
- Female
- Other

In what year were you born?

What was your TOTAL household income, before taxes, last year?

- $0-$9,999
- $10,000-$14,999
- $15,000-$19,999
- $20,000-$29,999
- $30,000-$39,999
- $40,000-$49,999
- $50,000-$69,999
- $70,000-$89,999
- $90,000-$109,999
- $110,000-$149,999
- $150,000-$199,999
- $200,000+

Please indicate your marital status

- Single
- Married
- Legally separated or divorced
- Widowed

How many children do you have?

- I do not have children
- 1
- 2
- 3
- 4

- ○ 5 or more

What racial or ethnic group best describes you?

- ○ White
- ○ Black or African-American
- ○ Hispanic or Latino
- ○ Asian or Asian-American
- ○ Native American
- ○ Middle Eastern
- ○ Mixed Race
- ○ Other

Were you born in the United States?

- ○ Yes
- ○ No

Where were you born?

Were both of your parents born in the United States?

- Yes
- No

Where was your father born?

Where was your mother born?

Which category best describes your highest level of education?

- Eighth Grade or less
- Some High School
- High School degree / GED
- Some College
- 2-year College Degree
- 4-year College Degree
- Master's Degree

- ○ Doctoral Degree; Professional Degree (JD, MD, MBA)

What is your current employment status?

- ○ Full-time employee
- ○ Part-time employee
- ○ Self-employed or small business owner
- ○ Unemployed and looking for work
- ○ Student
- ○ Not currently working and not looking for work
- ○ Retiree

What is your current occupation?

Even if you are not currently working, what was latest occupation?

On policy matters, where do you see yourself on the liberal/conservative spectrum?

- ○ Very liberal
- ○ Liberal
- ○ Moderate
- ○ Conservative
- ○ Very conservative

In politics, as of today, do you consider yourself a Republican, a Democrat or an independent?

- ○ Republican
- ○ Democrat
- ○ Independent

Attention check

How many states are there in the U.S.?

- ○ 5
- ○ 100
- ○ 50
- ○ 10

County

What is the FIPS code of your current county of residence?

If you are unsure, here is one way to look up your FIPS code:

Use your zip code or town/ city to look up your FIPS code on this page: https://www.zipinfo.com/search/zipcode.htm (check the box "county name and FIPS code" on the top left).

Your FIPS code will be a 5-digit number, possibly starting with 0. **Please note that your FIPS code is not your ZIP code! Please ensure that your FIPS code is correct. If it does not match your device location, we may be forced to terminate your survey.**

For how many years have you lived in this county?

- ○ Just moved in the last year
- ○ 1-5 years

- ○ 5-10 years
- ○ 10-20 years
- ○ 20-30 years
- ○ 30+ years

Big 5 Quiz

Next, we would like to ask you some questions about your personality.

Below you will see a number of statements, each of which starts with "I see myself as someone who." For each statement, please indicate how much you agree with this.

I see myself as someone who:

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Tends to be quiet.	○	○	○	○	○
... Is compassionate, has a soft heart.	○	○	○	○	○
... Tends to be disorganized.	○	○	○	○	○
... Worries a lot.	○	○	○	○	○

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Is fascinated by art, music, or literature.	○	○	○	○	○

I see myself as someone who:

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Is dominant, acts as a leader.	○	○	○	○	○
... Is sometimes rude to others.	○	○	○	○	○
... Has difficulty getting started on tasks.	○	○	○	○	○
... Tends to feel depressed, blue.	○	○	○	○	○
... Has little interest in abstract ideas.	○	○	○	○	○

I see myself as someone who:

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Is full of energy.	○	○	○	○	○
... Assumes the best about people.	○	○	○	○	○

	Disagree strongly	Disagree a little	Neutral; no opinion	Agree a little	Agree strongly
... Is reliable, can always be counted on.	○	○	○	○	○
... Is emotionally stable, not easily upset.	○	○	○	○	○
... Is original, comes up with new ideas.	○	○	○	○	○

Perceived_Similarity

We asked **immigrants from 3 different countries** to fill out the same personality quiz and computed their standardized scores on five major personality traits.

Next, we will ask you how you think your personality traits compare to theirs.

If your guess is correct, you will be entered into a lottery for a **$100 Amazon gift card**.

How do you think your personality traits compare to the traits of people **from Japan**?

- You have **0 out of 5** traits in common.
- You have **1 out of 5** traits in common.
- You have **2 out of 5** traits in common.
- You have **3 out of 5** traits in common.
- You have **4 out of 5** traits in common.
- You have **5 out of 5** traits in common.

How do you think your personality traits compare to the traits of people **from the Philippines**?

- You have **0 out of 5** traits in common.
- You have **1 out of 5** traits in common.
- You have **2 out of 5** traits in common.
- You have **3 out of 5** traits in common.
- You have **4 out of 5** traits in common.
- You have **5 out of 5** traits in common.

How do you think your personality traits compare to the traits of people **from Haiti**?

- ○ You have **0 out of 5** traits in common.
- ○ You have **1 out of 5** traits in common.
- ○ You have **2 out of 5** traits in common.
- ○ You have **3 out of 5** traits in common.
- ○ You have **4 out of 5** traits in common.
- ○ You have **5 out of 5** traits in common.

Contact

We would now like to ask about your close friends and family members, neighbors, workplace acquaintances, and others with whom you regularly interact (i.e. speak with at least once a month).

For each of the groups below, please check the box if a member of that group is in the respective category of people you interact with.

	Close friends and family members	Neighbors	Workplace acquaintances	Others with whom I regularly interact	Service or hospitality workers	No interactions
Japanese Americans	☐	☐	☐	☐	☐	☐

	Close friends and family members	Neighbors	Workplace acquaintances	Others with whom I regularly interact	Service or hospitality workers	No interactions
Filipino Americans	☐	☐	☐	☐	☐	☐

	Close friends and family members	Neighbors	Workplace acquaintances	Others with whom I regularly interact	Service or hospitality workers	No interactions
Haitian Americans	☐	☐	☐	☐	☐	☐

End

You have completed the survey. Thank you for your participation!

Powered by Qualtrics