The Immigrant Next Door*

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Abstract

We study how the presence of individuals of a given foreign descent shapes natives' attitudes and behavior toward that group. Using individualized donations data from large charitable organizations, we show that the long-term presence of a given foreign ancestry in a US county leads to more generous behavior specifically toward that group's ancestral country. To shed light on mechanisms, we focus on attitudes and behavior toward Arab-Muslims, combining several existing large-scale surveys, cross-county data on implicit prejudice, and a newly-collected national survey. We show that greater Arab-Muslim populations: (*i*) decrease both natives' explicit and implicit prejudice against Arab-Muslims, (*ii*) reduce natives' support for policies and political candidates hostile toward Arab-Muslims, (*iii*) lead to more personal contact between natives and Arab-Muslim individuals, and (*iv*) increase natives' knowledge of Arab-Muslims and Islam in general.

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1 Introduction

Many countries face growing challenges surrounding backlash against the presence of "non-natives". As hypothesized by Allport (1954), and as empirically demonstrated in more recent work (e.g. Lowe, 2020), the effects of specific forms of contact on attitudes and behavior depend heavily on the nature of interaction. Summing across all of the different forms of interaction that naturally occur between immigrants and natives, what is the aggregate effect on natives' beliefs and behavior?

In this paper, we show that the decades-long presence of immigrant groups induces more positive behavior and attitudes toward those groups. We combine several sources of data to measure the presence of, generosity towards, and prejudice against foreign-origin groups in the United States. In particular, we measure *presence* using variation in the number of residents of a US county who claim ancestry from a given foreign origin, and we measure *generosity* towards specific foreign countries using individualized data from two large charitable organizations, both of which channel donations from American donors to a large number of disaster-struck foreign countries in South America, Africa, Asia, and Oceania. Turning to mechanisms, we measure *attitudes* toward a specific foreign-origin group of particular relevance to the policy debate, Arab-Muslims, using the Implicit Association Test, survey data on explicitly stated warmth, voting for presidential candidate Donald Trump, and support for Trump's proposed Muslim Ban in 2016. Finally, we measure actual *contact* with and *knowledge* about Arab-Muslims through a large-scale custom survey. In sum, we find that exposure to descendants of a given group increases natives' generosity towards that group, lowers prejudice against that group, and increases personal contact with and knowledge about that group.

We make three main contributions. First, we quantify the aggregate effect of the decades-long presence of foreign migrant groups on natives' attitudes and behavior. Our estimates are large: for instance, they suggest that in the absence of a Haitian diaspora in the United States, for the average US county, the number of donations from white Americans to Haiti following the devastating 2010 earthquake would have decreased by 51.3%. Second, our empirical setting allows us to consider the effects of exposure to a large number of distinct outgroups, increasing the external validity of our findings beyond a single specific outgroup and enabling us to flexibly control for unobservable US county-specific or foreign country-specific confounders. Third, we combine information on actual behavior towards foreign origin groups (revealed preferences), on explicit attitudes (stated preferences), and on implicit bias (implicit preferences), shedding light on the mechanisms through which long-term presence affects generosity and prejudice.

We now turn to a more detailed description of our methodology and results. To identify the causal impact of exposure to foreign-origin groups on natives' beliefs about and behavior towards them at the aggregate (US county) level, we adopt the approach from Burchardi, Chaney and Hassan (2019). We isolate quasi-random variation in the ancestral composition of present-day US counties stemming exclusively from the interaction of two forces: *(i)* time-series variation in the relative attractiveness of different destination counties within the United States to the average migrant arriving at the time and *(ii)* the staggered arrival of migrants from different countries. In addition, we leverage the dyadic structure of our charitable donations data to control for any county- and country-specific unobservables by including county and country fixed effects, ensuring that our estimates are not confounded by county-specific differences in attitudes and behaviors toward foreigners in general or country-specific differences in the propensity to attract donations.

We find that a larger local population with ancestry from a given foreign country substantially increases donations from European-ancestry residents to that foreign country. This estimated effect of exposure operates on both the extensive and intensive margins of donations and is economically significant: a one percent increase in foreign ancestry increases the number of donations by approximately 0.1%, and the dollar value of donations by approximately 0.3%. We show evidence this effect operates not just at the county level, but also at the aggregate (commuting zone and state) level. Horseracing the effect of exposure to first-generation immigrants against the effects of exposure to foreign ancestry, which includes second- and higher-generation immigrants, we find evidence that: on the margin, exposure to people of a given foreign ancestry, but who were born in the United States, has a positive and significant effect on donations to their ancestral country; whereas additional exposure to foreign born immigrants has a null effect on donations.

Even though these results condition on county fixed effects and quasi-random variation in the ancestral composition of US counties, different types of "natives" might still selectively move within the United States to avoid living near descendants of migrants from specific origins. If such "selective white flight" were large enough in magnitude, it could bias our estimated effects of contact. Using thirty years of detailed Census data on internal migration, we show that none of our results are attributable to such endogenous sorting of the native population. On average, white Americans do not react to the presence of descendants of foreign migrants from a given country by moving to counties with smaller populations of that ancestral group, nor does this null effect mask significant heterogeneity by subgroup. We conclude that the effect of ancestry on donations is indeed causal.

To investigate mechanisms, we focus on a single foreign-origin group, Arab-Muslims, for which we have detailed cross-county data on natives' behavior and attitudes. We first replicate our results on charitable giving limiting the sample to Arab countries: greater exposure to residents of Arab-Muslim ancestry significantly increases donations towards Arab-Muslim countries.[1] This exposure also leads to more positive attitudes: white, non-Muslim respondents in counties with (exogenously) larger populations of Arab ancestry are less implicitly and explicitly prejudiced against Arab-Muslims. At the same time, the presence of Arabs does not appear to affect attitudes toward non-Arab, non-Muslim minority groups. These effects on attitudes carry over into measures of political choices: non-Muslim white residents in counties with (exogenously) larger Arab-Muslim ancestry were less supportive of Donald Trump's "Muslim Ban" and, in 2016, were less likely to vote for Donald Trump.

Finally, we present the results of a large-scale custom survey designed to shed light on two potential channels through which exposure to Arab-Muslims might affect natives' beliefs and behavior: first, that a greater Arab-Muslim population increases direct, personal interaction between non-Muslim white residents and Arab-Muslims; and second, that a greater Arab-Muslim population increases knowledge of Arab-Muslims and reduces the extent to which non-Muslim whites hold negative stereotypes about Islam. We find that an (exogenously) larger Arab-Muslim population in a respondent's county substantially increases the probability that the respondent has an Arab-Muslim friend, neighbor, or workplace acquaintance. A larger Arab-Muslim population also substantially increases respondents' knowledge of Arab-Muslims and Islam in general and decreases the extent to which they associate Islam with violence or prejudice against women.

Taking the evidence together, we conclude that natives' greater charitable donations toward a foreign-origin group's ancestral country, their more positive explicit and implicit attitudes toward that group, their lower support for policies and candidates hostile toward that group, and their greater contact with and knowledge of that group are driven by that group's long-term presence. The long-term presence of minority foreign groups, summing up over all types of day-to-day interactions with natives, induces more favorable behavior and attitudes towards them.

Related literature Our paper contributes to a large literature studying the effect of intergroup contact on attitudes and discrimination, building on the seminal work by Allport (1954). Given the selection issues inherent to most observational designs studying contact, much of this literature relies on randomized experiments.[2] Other papers exploit natural experiments, such as the random assignment

[1] Although the focus on a single group precludes including county fixed effects, we carry out a range of exercises to verify that our instrument remains conditionally exogenous to county-level confounders. In particular, we show that, for all countries, the inclusion of county-level fixed effects does not substantially change our main (dyadic) estimates, suggesting that any potential bias resulting from correlation between county-specific unobservables and our instrument is small. We also show that an exogenously greater Arab-Muslim population does not significantly affect any of a range of placebo outcomes relating to other foreign-origin groups.

[2] See Pettigrew and Tropp (2006) and Paluck et al. (2018) for meta-analyses of this literature. Experiments studying the effects of long-run contact on adults, rather than children, are especially scarce: Paluck et al. (2018) find that, at the time of writing, there were no randomized studies that show the effects of interracial and interethnic contact on adults over the age of 25, and there were only three such studies that quantify the effects more than a single day after

of roommates or classmates (Boisjoly et al., 2006; Rao, 2019; Carrell et al., 2019; Corno et al., 2019; Scacco and Warren, 2018; Billings et al., 2021), the random composition of military bootcamp cohorts (Dahl et al., 2020; Finseraas and Kotsadam, 2017) or the random assignment of location for military or missionary deployment (Bagues and Roth, 2020; Crawfurd, 2020).

One important theme in this literature is persistence. Some studies (Schindler and Westcott, 2021; Bazzi et al., 2019; Bagues and Roth, 2020) find that the effects of contact persist over long periods, while others (Dahl et al., 2020; Enos, 2014) find that effects fade out quickly. Recent work (Lowe, 2020; Mousa, 2020; Bazzi et al., 2019) has also documented considerable heterogeneity: contact may lead to more positive social preferences in some contexts while having no effects or even negative effects in others.[3] Given these disparate findings, a crucial question concerns the *aggregate effect* of ancestral presence: summing up over all types of naturally-occurring interactions over the course of decades, how does intergroup exposure shape beliefs and prejudices, and translate into real-world behavior? Our data and identification strategy allow us to identify such causal effect on a comprehensive range of outcomes in the most natural possible setting – day-to-day interaction over decades.

Our paper also complements a growing body of work on the relationship between immigration, political attitudes, and voting behavior. Some work finds that higher immigration leads to greater support for right-wing parties,[4] while other work has found evidence in the opposite direction:[5] for instance, Calderon et al. (2022) find that the Second Great Migration of African-Americans to the US North increased whites' support for the civil rights movement, while Tabellini (2020) shows increased immigration to US counties caused higher support for anti-immigration legislation, the election of more conservative legislators, and lower redistribution, despite the economic benefits generated for non-immigrants. Steinmayr (2021) finds evidence of both positive and negative effects: the far right vote share is increased by short-term exposure to refugees, but decreased by sustained contact.

Our work complements these results in multiple respects. We isolate the direct effect of exposure to out-groups on implicit and explicit attitudes and altruistic behavior towards these groups, thus shedding light on underlying mechanisms;[6] we examine the effects of the presence of dozens of different

treatment.

[3]For example, while Lowe (2020) and Mousa (2020) find that cooperative contact leads to more positive social behavior, Lowe (2020) finds that adversarial contact has the opposite effect, and Mousa (2020) finds that this more positive behavior is limited to specific contexts. Bazzi et al. (2019) exploit a population resettlement program to identify the long-run effects of intergroup contact on national integration in Indonesia, and find that the program leads to greater integration in fractionalized communities with many small groups, but has the opposite effect in polarized areas with a few large groups.

[4]See, for example, Barone et al., 2016; Halla et al., 2017; Dustmann et al., 2019; Brunner and Kuhn, 2018; Becker and Fetzer, 2016; Colussi et al., 2016.

[5]See, for example, Dill, 2013; Vertier et al., 2022; Achard et al., 2022.

[6]Recent contributions have used Implicit Association Test (IAT) scores as a *predictor* of biased behaviors (Glover et al., 2017; Carlana, 2019); we instead use these scores as an *outcome* and provide evidence that implicit bias can be shaped by exposure to out-groups, complementing recent work in other contexts (Lowes et al., 2015, 2017; Schindler and

foreign ancestral groups over the period of decades, allowing us to control flexibly for county- and country-level confounders; and we offer evidence of mechanisms through which the long-term presence of individuals of foreign descent affect behavior and generosity.[7] Thus, we contribute to the extensive literature on cultural persistence and change by showing that the local presence of foreign groups changes long-term attitudes toward them (Alesina et al., 2013; Giuliano and Nunn, 2017).

The remainder of this paper proceeds as follows. Section 2 describes our data. Section 3 presents our results on donations to foreign countries and probes the robustness of our results. Section 4 explores heterogeneity and, through a detailed examination of attitudes toward Arab-Muslims, sheds light on the mechanisms underlying the effect of exposure. Section 5 concludes.

2 Data

We collect several series of data broadly corresponding to measures of presence, generosity, and prejudice, with summary statistics provided in Appendix Table A1 and a more detailed description provided in Appendix Section B.2. Throughout the analysis, we denote domestic US counties by d and foreign countries by f. In analyses with county-country-quarter level data, our variables are generically defined as $X^t_{d,f}$, denoting outcome X for country f, at time t, in US county d. In analyses with individual-level data (all of which are cross-sectional and specifically pertain to Arab-Muslims), our variables are generically defined as $X_{i,d}$, denoting the outcome X of individual i residing in county d.

2.1 Presence: Historical Migrations and Ancestry

To quantify the presence of members of a given ethnicity, we collect data on the historical ancestral composition of US counties. We conjecture that a person living in county d with a larger community with ancestry from country f has a stronger exposure to that community (a conjecture we corroborate empirically in Section 4). As discussed in Appendix B.1, we follow Burchardi et al. (2019) and extract information on immigration and ancestry from the individual files of the Integrated Public Use Microdata Series (IPUMS) samples of the 1880, 1900, 1910, 1920, 1930, 1970, 1980, and 1990 waves of the US Census and from the 2006-2010 five-year sample of the American Community Survey (ACS).

Our key measure of historical immigration is $I^t_{f,d}$: the number of immigrants who were born in

Westcott, 2021).

[7] Fouka et al. (2022) finds that the Great Migration, which led millions of African-Americans to migrate out of the rural South, improved white residents' views of immigrants and facilitated social integration of European immigrant groups. Similarly, Fouka and Tabellini (2022) find that Mexican immigration improves white residents' attitudes and behavior towards Black Americans. More generally, our results relate to the discussion in Myrdal et al. (1944) about the importance of information transmission in changing whites' attitudes toward minorities.

foreign country f, who live in domestic county d at time t, and who immigrated to the US between $t-1$ and t (the interval between two consecutive Census waves). Our stock ancestry variable, $Ancestry^{t}_{f,d}$, corresponds to the number of respondents in d at t who report ancestry from f; that is, this stock includes both US-born individuals with ancestry from country f and first-generation immigrants from country f. Our empirical strategy isolates quasi-random variation in this variable. Appendix Table A2 displays the number of individuals with ancestry from a given country and the peak arrival time for all countries in our dataset. In total, 11.2% of individuals report ancestry from one of the 44 countries in our donations dataset. Appendix Figure A1 plots the fraction of individuals in each county who claim ancestry from a foreign country in our dataset (Panel A) and the fraction of individuals in each county who claim ancestry from an Arab-Muslim country in our dataset (Panel B).

2.2 Generosity: Charitable Donations

To measure generosity towards foreign countries, we collect data on charitable donations towards foreign causes from two major charitable organizations, to which we refer as Charity 1 and Charity 2.[8] While both organizations occasionally donate to US-based causes, they primarily channel donations from US donors towards foreign non-governmental organizations. We focus solely on donations to specific foreign countries, the vast majority of which occur immediately after a natural or man-made disaster in that country. After removing donors whom we are unable to match to a unique county of residence, we are left with 80,556 individual donations spanning 2004 to 2017 for Charity 1 and 715,663 individual donations spanning 2010 to 2017 for Charity 2. For each donation, the organizations know the name of the donor, the date of the donation, the foreign destination of the donation, and, for Charity 2 only, the dollar amount of the donation. Appendix Figure A2 maps the distribution of donors across US counties and the worldwide distribution of the receiving countries. Donations come from all parts of the US; recipient countries are primarily in Africa, South Asia, and Latin America.

We pool donations across Charity 1 and Charity 2 and restrict our sample to the 44 recipient countries in both datasets, for all of which we have ancestry data from the census. To identify the likely ancestral country of origin of donors, we contract with NamSor, an organization which uses machine learning techniques on historical Census data to classify names by ethnicity, gender, and religion. In our main specification, we restrict the sample to donors matched to European countries to approximate a population of white "natives."[9] Given that no recipient country in our dataset is

[8] Charity 1 requested anonymity. Charity 2 is GlobalGiving (https://www.globalgiving.org), "a nonprofit that has served disaster-impacted communities around the world since 2004, mainly by raising money from U.S. donors to drive locally led responses to natural or man-made disasters."

[9] In particular, we restrict to donors matched to countries classified as European by the International Organization for Standardization. We validate the accuracy of this classification in Appendix Section C.1. Because the classification algorithm is trained to predict the ethnic origin of the name, not the current country of residence, only respondents with

in Europe, this restriction also ensures that our results are not driven by the natural tendency of individuals to donate to their own ancestral country. We then aggregate donations at the domestic county $d \times$ foreign country $f \times$ quarter t level.

2.3 Implicit and Explicit Prejudice: IATs and Stated Warmth

We draw data on implicit and explicit prejudice against Arab-Muslims from two sources. The first source is Project Implicit, a platform through which respondents can complete Implicit Association Tests (IATs) quantifying subconscious prejudice against different groups. IAT scores are generally regarded as difficult to manipulate (Egloff and Schmukle, 2002), and a number of studies have correlated these scores with real-world psychological responses and economic decision-making (Bertrand et al., 2005; Carlana, 2019; Glover et al., 2017). We use data from all Arab-Muslim, Asian, and Race IATs taken before January 1, 2021. Subjects taking the IAT answer additional questions, including a measure of explicitly-stated attitudes ("warmth") toward the group in question. Subjects also report their demographic characteristics and indicate their reason for taking the test. In order to assuage concerns about respondents endogenously selecting into taking the IAT, we classify respondents taking the test due to "Assignment for work" or "Assignment for school" as "forced respondents" and conduct our primary analyses with the 107,083 white, non-Muslim forced respondents to the Arab-Muslim IAT. To ensure that our estimates generalize to a representative sample, we turn to Nationscape, a large-scale survey, representative of the US population, administered by the Democracy Fund Voter Study Group and fielded between 2019 and 2020. In this survey, respondents explicitly state their favorability toward Muslims. We again restrict the sample to white, non-Muslim respondents. For comparability, we normalize all measures — implicit prejudice against Arab-Muslims (Project Implicit), warmth toward Arab-Muslims (Project Implicit), favorability toward Muslims (Nationscape) — to mean zero and standard deviation one, with higher values representing more positive attitudes.

2.4 Political Choice: Muslim Ban Support and Trump Voting

We assess how exposure to Arab-Muslims shapes political choice by analyzing two distinct outcomes from the Cooperative Congressional Election Study (CCES), a widely-used representative and stratified survey tracking public opinion and political attitudes. First, we examine the effect of exposure to individuals of Arab-Muslim ancestry on support for the "Muslim Ban," proposed by Donald Trump during his 2016 presidential campaign and first implemented in January 2017.[10] As our second mea-

names associated with Native American nations are matched to the United States, while most Americans are matched to European countries.

[10]Executive Order 13769, "Protecting the Nation From Foreign Terrorist Entry Into the United States," severely restricted travel from Iran, Iraq, Libya, Somalia, Sudan, Syria, and Yemen. The order did not target all Arab countries

sure of political choice, we study voting behavior in the 2016 US Presidential elections. Aside from his calls for a Muslim Ban, Trump's campaign rhetoric often singled out Arab-Muslims, suggesting that Islam was incompatible with American values and portraying Muslims as terrorists.[11] We thus in part attribute increases in Republican support between 2012 and 2016 to hostility toward Arab-Muslims. Both CCES and Nationscape include questions eliciting respondents' support for the Muslim Ban and 2016 voting behavior. As before, we limit to white, non-Muslim respondents.

2.5 Contact and Mechanisms: Reported Contact and Knowledge

To further understand the mechanisms through which exposure to Arab-Muslims shapes beliefs, we fielded a large-scale survey between December 30, 2020 and January 2, 2021 in cooperation with Luc.id, a consumer research company widely used in the social sciences (e.g. Burzstyn et al. 2023; Fetzer et al. 2020). We restrict our sample to white, non-Muslim respondents who were born in the US and who report that they are not of Arab descent. Our resulting sample ($n = 5,031$) is broadly representative of the targeted population in terms of age, gender, income, Hispanic ethnicity, and education (Appendix Table A4). We include the survey questionnaire in Appendix D.

The core of our survey elicits respondents' *contact* with Arab-Muslims and their *knowledge* of Arab-Muslims and Islam in general. To measure contact, we ask respondents to indicate whether they have interacted with Arab-Muslims in any of three capacities: as friends, as neighbors, and as workplace acquaintances. To measure knowledge of Arab-Muslims, we ask three questions. First, we ask respondents to select the correct definition of Ramadan among one correct and three incorrect definitions. Second, we ask respondents to identify the five pillars of Islam among a number of possible choices; respondents receive one point for each correct answer they highlight and for each incorrect answer they do not highlight. Finally, we ask respondents to indicate the percentage of the US population which is Muslim, and we measure accuracy as the (negative) of the absolute value of the difference between their guess and the correct percentage (1.1 percent).

(e.g. the United Arab Emirates and Saudi Arabia were exempted). Although it was not officially a ban on Muslims, Trump's repeated comments on the campaign trail — and the fact that all countries on the list were Muslim-majority — caused it to be widely interpreted as such.

[11] For example, Trump suggested that he might implement a national database of American Muslims and that he would be open to surveilling or closing mosques. See, for example, Why Trump's Proposed Targeting of Muslims Would Be Unconstitutional *American Civil Liberties Union*, Nov 22, 2016.

3 Effect of the Presence of Foreign Ancestries on Natives' Donations

We begin by examining the effects of the presence of foreign descent groups on natives' propensity to donate to those groups' ancestral countries. This analysis allows us to exploit the dyadic structure of our donations dataset — that is, the fact that we observe donation flows originating from many different counties and going to many different countries — by including a rich set of fixed effects.

3.1 Econometric Specification

In our primary analyses, we measure county d's exposure to foreign ancestral group f as the inverse hyperbolic sine of the number of residents in domestic county d who claim ancestry from a foreign country f, $IHS\left(\text{Ancestry}_{d,f}\right)$.[12] This functional form places an emphasis on the *absolute size* of the community with ancestry from f. For example, a large enough population with ancestry from a given origin country may support grocery stores, restaurants, cultural events and centers, etc. As we discuss in Section 3.6, our conclusions remain unchanged if we instead consider the *share* of the population in county d with ancestry from f.

Our outcome variable is the IHS-transformed number of donations from residents in county d to country f in period t. Our specifications take the form

$$IHS\left(\#\text{Donations}_{d,f}^{t}\right) = \beta IHS\left(\text{Ancestry}_{d,f}^{t}\right) + \delta_d \times \delta_t + \delta_f \times \delta_t + \text{Controls}_{d,f}^{t} + \epsilon_{d,f}^{t}, \tag{1}$$

where δ_d, δ_f, and δ_t denote fixed effects for domestic county d, foreign country f, and quarter t. The coefficient of interest from Equation (1), β, approximates the elasticity of donations with respect to ancestry.

The fixed effects included in Equation (1) address a number of important challenges to identification. For example, any systematic differences between counties in overall generosity or tolerance towards foreigners, even if they vary over time, are absorbed in the interaction of county and time fixed effects. Similarly, the interactions $\delta_f \times \delta_t$ absorb any systematic differences in how liked or disliked certain foreign countries are across the US as a whole.

Nevertheless, there remain two main challenges to identifying β. First, unobserved factors may affect both the existing stock of ancestry from a given foreign country and the propensity of local residents to donate specifically to that country, creating a spurious correlation between ancestry and donations. For instance, it is possible that Arab-Muslims endogenously prefer settlement in US coun-

[12] The inverse hyperbolic sine (IHS), defined as $IHS(x) = \ln\left(x + \sqrt{x^2+1}\right)$, approximates the natural logarithm function, but is well defined at zero.

ties that are and always have been more (or less) tolerant towards Arab migrants than towards other origins. Second, even after isolating exogenous variation in foreign ancestry, it is still possible that different types of natives sort across counties to live near to their preferred foreign minority — selective white flight. We address each of these concerns in turn.

3.2 Isolating Exogenous Variations in Foreign Ancestry

To address the first concern, we construct instruments for the present-day distribution of foreign ancestry across US counties by combining data from the long history of foreign migrations to the US with a simple model of international migration, following closely the approach first developed by Burchardi et al. (2019).[13] Our instruments purposefully exclude any determinant of migration that could correlate with the endogenous response of foreign migrants to natives' attitudes towards specific foreign groups, such as prejudice, hostility, or generosity toward specific groups.

In this model, the historical allocation of foreign migrants across domestic counties is governed by three forces. First, during times when more migrants arrive from a given foreign origin f, more migrants from f will settle in *all* domestic counties, all else equal. We label this first source of variation a 'push factor,' which varies across foreign origins f and over time t. Second, we assume that upon their arrival in the US, a migrant from f is more likely to settle in d if they can find better economic opportunities there. We proxy for the attractiveness of county d at time t for migrants arriving from *any* foreign origin using the fraction of foreign migrants, irrespective of their origin, who settle in d at time t. We label this second source of variation an 'economic pull factor,' which varies across domestic counties d and over time t. Third, we assume that upon their arrival in the US, a migrant from f is also more likely to settle in d if it hosts a large preexisting community from f. We label this third source of variation a 'social pull factor.'

Combining all three elements, we predict that many migrants from f will settle in d at time t if many migrants from f arrive in the US at t, *and* d is attractive to migrants from any foreign country at t, *and* d hosts a large preexisting stock with ancestry from f. Finally, we use the fact that the preexisting stock of ancestries at any time is itself inherited from previous migration waves in earlier periods. Iterating our model forward then allows us to isolate (exogenous) variation in the distribution of ancestries which results purely from the historical interaction of economic push and pull factors.

To exclude the possibility that our push and pull factors are contaminated by any remaining county-country specific factors, when predicting ancestry from f in d, we leave out from the push

[13] Variants of this approach have since been employed by Burchardi et al. (2020) and Arkolakis et al. (2020), among others. As discussed in Burchardi et al. (2019), the approach combines a leave-out approach (e.g. Bartik, 1991), adapted to two dimensions, with a push-pull model (e.g. Card, 2001; Boustan, 2010).

factor migrants from f settling in the Census region (Northeast, South, West, or Midwest) where county d is located, and from the economic pull factor migrants from the same continent as f.[14]

As Burchardi et al. (2019) show, the first-stage expression for the contemporaneous stock of residents in domestic county d with ancestry from foreign country f at time t can be written as

$$IHS\left(\text{Ancestry}_{d,f}^{t}\right) = \sum_{s=1880}^{t} \gamma_s I_{f,-r(d)}^{s} \frac{I_{-c(f),d}^{s}}{I_{-c(f)}^{s}} + \gamma \cdot \text{PCs}_{d,f}^{t} + \text{Controls}_{d,f}^{t} + \eta_{d,f}^{t}, \quad (2)$$

where $\text{Controls}_{d,f}^{t}$ includes the full set of controls and fixed effects in (1). $I_{f,-r(d)}^{s}$ is our push factor, the total number of migrants arriving from country f in period s, excluding those who settle in d's region $(-r(d))$; $I_{-c(f),d}^{s}/I_{-c(f)}^{s}$ is our economic pull factor, the fraction of all migrants arriving in the US in period s who settle in county d, excluding migrants from f's continent $(-c(f))$. The vector $\text{PCs}_{d,f}^{t}$ are principal components summarizing the information contained in higher order interactions of push and pull factors.[15]

To understand how the push-pull and higher-order interaction terms affect contemporaneous ancestry, it is easiest to consider a stylized historical example. In the 1920s, there was a large influx of Mexican migrants to the US following the Mexican Revolution: a large "push" from Mexico. At the same time, due to the newly booming automobile industry, Detroit was attracting large numbers of migrants from all origins: a large "economic pull" for Detroit. The push-pull interaction thus induced a large stock of Mexican ancestry in Detroit starting in 1920 (Mexico push 1920 × Detroit pull 1920). As immigration from Mexico again increased in the 1980s, the "social pull" factor led to large inflows of Mexican migrants, even though Detroit was no longer an attractive place for migrants in general (Mexico push 1980 × Mexico push 1920 × Detroit pull 1920). And the next wave of Mexican migrants in the 1990s was again in part attracted to Detroit due to the large Mexican ancestry inherited from both 1920 and 1980 (Mexico push 1990 × Mexico push 1980 × Mexico push 1920 × Detroit pull 1920). As a result, Detroit has a large Mexican community in 2010 inherited from at least three waves. In Equation (2), the first wave corresponds to the push-pull term $\gamma_{1920} I_{Mexico,not\,Midwest}^{1920} \frac{I_{not\,Latin\,America,Detroit}^{1920}}{I_{not\,Latin\,America}^{1920}}$; the next two waves are summarized in the principal components.

The push-pull interaction terms in Equation (2) — $I_{f,-r(d)}^{s} \frac{I_{-c(f),d}^{s}}{I_{-c(f)}^{s}}$ for $s = 1880 \ldots 2010$ and $\text{PCs}_{d,f}^{t}$ — are the excluded instruments we use in every IV specification of our main estimating equations.

[14] We explore various alternative leave-out strategies as robustness checks and obtain similar results (see Section 3.6).

[15] Formally, for all $\{d, f\}$ pairs, there are 758 higher-order terms: $I_{f,-r(d)}^{s}(I_{-c(f),d}^{s}/I_{-c(f)}^{s}) \prod_{u=s+1}^{t_0} I_{f,-r(d)}^{u}, \forall\ (s, t_0)$ s.t. $1880 \leq s < t_0 \leq t$. The vector Principal $\text{Components}_{d,f}^{t}$ corresponds to the five largest principal components, which jointly capture over 99% of the total variation among higher-order terms.

Our identifying assumption is

$$\text{Cov}\left(I^s_{f,-r(d)}\frac{I^s_{-c(f),d}}{I^s_{-c(f)}}, \epsilon^t_{d,f}\middle|\text{controls}\right) = 0, \forall s \leq t, \tag{3}$$

where $\epsilon^t_{d,f}$ are the residuals from Equation (1). We require that any unobservable factor that makes residents in a county d more or less generous toward people with ancestry from f post-2005, $\epsilon^t_{d,f}$ in (1), is conditionally uncorrelated with the coincidental interaction push- and pull factors going back to 1880.

To return to our stylized example, we observe in 2010 many charitable donations from Detroit residents who are not of Mexican descent to Mexico, even controlling for the fact that Detroit residents may be more generous towards *all* foreign countries – the Detroit × quarter fixed effect $\delta_d \times \delta_t$ in (1) — and that Mexico may be a preferred destination for donations from *all* US donors — the Mexico fixed effect $\delta_f \times \delta_t$ in (1). Our first stage predicts a large population of Mexican ancestry in 2010 in Detroit because many Mexicans happened to migrate to the US in 1920 (excluding the Midwest) – precisely at the time when Detroit was attracting a large share of foreign migrants in 1920 (excluding Latin Americans). Our identifying assumption requires that this interaction of the timing of large Mexican out-migrations and large Detroit in-migrations in 1920 affects disproportionate generosity towards Mexico (relative to causes in other countries) among white (non-Mexican) Detroiters in 2010 only through its effect on Mexican settlement in Detroit, and not through any other channel.

Appendix Figure A3 presents the first-stage coefficients. Following Burchardi et al. (2019), to facilitate the interpretation of coefficients as the marginal effect of migrations in that period, we sequentially orthogonalize each instrument with respect to the previous instruments. Reassuringly, all but one of the terms are positive (with the 2000 term marginally negative). We find similar results for the Arab-Muslim sample in Appendix Figure A4.

3.3 Main Results

Table 1 presents estimates of Equation (1), restricting the sample to donors with European-origin names. The outcome is the IHS-transformed number of donations from county d to country f. Column 1 presents estimates with only quarter × destination country fixed effects. Column 2 adds controls for the logged distance between country f and county d, the associated latitude difference, and a set of demographic controls as of 2000 (the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area, alongside population density, the unemployment rate, and log income). Column 3 adds quarter × state fixed effects, and Column 4 replaces the county-level demographic controls with quarter × county fixed effects.

Our preferred estimate in Column 4 (0.107, s.e.=0.043) implies that a one unit increase in the IHS of ancestry from country f (approximately half a standard deviation) increases the IHS of the number of donations to f by 0.107 (approximately two-thirds of a standard deviation).[16] We present this result graphically in Figure 1, in which we plot (binned) predicted ancestry against (binned) IHS-transformed donations, where both are residualized by the set of controls included in Column 4 of Table 1. Interpreting the IHS transformation as an approximation of the natural logarithm, the estimated elasticity of the number of donations to f with respect to the size of the ancestral group from f is 0.1: a 1% increase in the local population with ancestry from a given country increases the number of donations from donors with European names towards that country by 0.1%. The remaining columns show this effect on donations operates at both the extensive and intensive margins: a one unit increase in the IHS of ancestry from country f increases the (linear) probability that any residents with European names in the county donate to country f by 4.7% and increases the dollar amount of donations by 0.329% (Charity 2 only). The first-stage F-statistics tend to be large, but we nonetheless provide p-values from weak IV-robust inference (based on Conditional Likelihood Ratio tests, following Andrews 2016; Sun 2018).

To put these magnitudes in perspective, consider a counterfactual state where there is no Haitian diaspora in the United States. A literal interpretation of our results suggests that, for the average US county, the number of donations from white donors flowing to Haiti after the devastating 2010 earthquake would decrease by 51.3%, and the dollar value of donations by 87.4%. Note this is a reduction in charitable donations *specifically* directed at Haiti, not of the overall level of generosity towards foreign countries.

Importantly, as our preferred specifications include county and country fixed effects, the impact of foreign ancestry is specific to each immigrant group and arises even after we control for any cross-county differences in overall generosity: the presence of a *specific* immigrant group over a period of years or decades increases generosity specifically toward that group's ancestral country, relative to all other recipient countries.

OLS versus IV To probe the robustness of our instrumental variable strategy, it is useful to first examine the OLS estimates in Panel B. As we move from Column 1 to 4 (adding more and more controls), the OLS estimate drops by more than two thirds and becomes statistically indistinguishable from zero in the most stringent specification with quarter $\times$ county fixed effects (Column 4). These

[16] Consistent with Burchardi et al. (2019), the F-statistics on the excluded instruments are well above critical levels throughout (330.6 in Column 4), showing that the first stage has sufficient power across all of these variations.

large changes in the OLS coefficient suggest that some of the positive correlation between donations and ancestry in Column 1 is likely explained by the fact that counties with more residents of foreign ancestry are wealthier or more generous towards all foreign causes, or by the fact that some foreign causes are more popular with donors throughout the United States than others. As we control for more and more of these factors, the OLS coefficient drops dramatically.

By contrast, the corresponding IV estimates remain in a tight range between 0.139 (s.e.=0.028) in Column 1 and 0.107 (s.e.=0.043) in Column 4, as we add more and more stringent controls – in particular 150,768 interacted quarter × county fixed effects when going from Column 3 to 4. This stability suggests that our instruments successfully isolate exogenous variations in ancestry that is orthogonal to such confounding factors across counties and countries.

Moreover, the OLS estimates (Panel B) tend to be about an order of magnitude smaller than the IV estimates (Panel A). One obvious reason for this pattern is measurement error – recalled ancestry is notoriously noisy (Duncan and Trejo, 2017), and our instruments, based on realized historical migrations, should remove measurement errors induced by such recall bias. In addition to measurement error in ancestry, however, smaller OLS estimates are also consistent with migrants endogenously choosing where to settle. In particular, one of the d-f-specific confounding factors our instruments remove is the possibility that migrants from a given country may choose to locate in US counties in which their human capital matches local job opportunities. Such selection could drive them towards US counties that experience import competition from their home country, even in the absence of migration. That is, endogenous selection may drive migrants from a given country towards US counties where native residents are ex-ante less generous specifically toward that country, and thus lead to a negative bias in the OLS coefficient, as we empirically observe. (This type of bias in the raw within-county variation is particularly plausible after controlling for county fixed effects, which absorb any variation in residents' general attitude towards foreign causes). We also show below (Section 3.6) our estimates are robust to a range of other possible concerns.

3.4 Ruling Out "Selective White Flight"

Although our identification strategy rules out endogeneity concerns relating to the selection of immigrants into counties that are disproportionately generous toward their ancestral country, it does not address the potential selection of white *natives*: in- and out-migration in response to exogenous changes in counties' ancestral composition. While any tendency of natives to avoid immigrant groups in *general* will not bias our estimates due to the inclusion of county fixed effects, *differential* selection — "selective white flight" — may lead to a bias. For example, if white, non-Mexican Detroiters who specifically dislike Mexicans (but not other minorities) leave Detroit as the Mexican community grows and move to places with small Mexican communities, while white, non-Mexican residents from elsewhere who specifically like Mexicans move to Detroit, then Detroit would display spuriously positive attitudes and generosity toward Mexicans.

We systematically test for such selective white flight by constructing a $d \times f$ specific index designed to capture whether white natives who move out of d (e.g. Detroit) have a tendency to settle in places with larger or smaller communities with ancestry from f (e.g. Mexico) relative to its national average:

$$\text{WhiteFlightIndex}_{d,f}^{t} = \sum_{d'} \frac{Out_{d,d'}^{t}}{Out_{d,\cdot}^{t}} \frac{Ancestry_{d',f}^{t}/Ancestry_{d'}^{t}}{\mathbb{E}\left[Ancestry_{d'',f}^{t}/Ancestry_{d''}^{t}|f\right]}, \tag{4}$$

where $Out_{d,d'}^{t}/Out_{d,\cdot}^{t}$ is the share of white natives from d who move to d' in period t; $Ancestry_{d',f}^{t}/Ancestry_{d'}^{t}$ is the population share in d' with ancestry from f; and $\mathbb{E}\left[Ancestry_{d'',f}^{t}/Ancestry_{d''}^{t}|f\right]$ is the average population share with ancestry from f across all US counties. The index thus takes a low value if white residents leaving d move to counties with a disproportionately small ethnic enclave from f. For instance, for $d = Detroit$ and $f = Mexico$, this index takes a low value if a large share of white movers from Detroit choose domestic locations where Mexican ancestry is small relative to its national average. We construct this index for moves by white Americans between 1970 and 2000, using all available data from the 1980, 1990, and 2000 Censuses.

Table 2 shows estimated effects of IHS-transformed ancestral population on the IHS-transformed white flight index:

$$IHS\left(\text{WhiteFlightIndex}_{d,f}^{t}\right) = \beta IHS\left(\text{Ancestry}_{d,f}^{t}\right) + \delta_t + \delta_d + \delta_f + \text{Controls}_{d,f}^{t} + \epsilon_{d,f}^{t}, \tag{5}$$

where we again instrument for ancestry using Equation (2). The table shows no evidence of selective white flight, which would manifest as an economically significant negative coefficient on ancestry: if d hosts a large community from f –$IHS(\text{Ancestry}_{d,f}^{t})$ large, movers from d would move to places with a *small* population from f –$IHS(\text{WhiteFlightIndex}_{d,f}^{t})$ small. If anything, the estimate in Column 1 (conditional on time and country fixed effects) is marginally positive – the opposite of selective white flight. Once we add county fixed effects in Column 2, the estimated coefficient becomes a precisely estimated zero ($\beta = -0.009$, s.e.=0.007). To investigate whether this null average effect masks heterogeneity, we construct our index separately for married and unmarried individuals, male and female individuals, individuals with and without a four-year college degree, individuals above and below median age, and individuals with above and below median income. As shown in Panel B, we find no evidence of significant heterogeneity across any of the five subgroups. In other words, we find no evidence for the kind of selective white flight that could bias our results.[17]

3.5 Local vs. Aggregate Effects

The county × quarter and country × quarter fixed effects in our baseline specification rule out a wide range of possible confounding factors that would make residents in some counties more generous than in others, or more generous towards some foreign countries than others. As a result of including them, however, our estimates speak only to the *relative* effect of ancestry: white residents of treated counties donate more, relative to their overall level of generosity towards foreign countries. This leaves open the possibility of crowding out: more donations from d may come at the expense of fewer donations from elsewhere. Table 3 suggests there is no such crowding out. Column 1 replicates our baseline county-level regression ($\beta = 0.107$, s.e.=0.043). To measure the absolute effect, Column 2 omits both county × quarter and country × quarter fixed effects ($\beta = 0.066$, s.e.=0.015), and Column 3 omits the county × quarter fixed effect ($\beta = 0.132$, s.e.=0.033). In both cases, the effect remains positive and highly significant, suggesting that the presence of foreign ancestry in county d positively affects the *absolute* number of donations from d rather than solely reducing the number of donations from d to other destinations.

Consistent with this positive absolute effect, Column 4 and 5 further show positive estimates at higher levels of spatial aggregation. When we aggregate our data at the commuting zone level in Column 4 and at the state level in Column 5, we find that, if anything, coefficients increase in magnitude as we increase the level of geographical aggregation ($\beta = 0.219$, s.e.=0.104 for commuting zones and $\beta = 0.534$, s.e.=0.225 for states). In Column 6, we aggregate *destinations* rather than *origins* to calculate the effect of a greater ancestral presence from all countries in a given continent on donations to all countries in that continent, and we again estimate a positive and highly significant effect ($\beta = 0.341$, s.e.=0.113). We conclude that the presence of descendants of foreign migrants has a positive *aggregate* impact on the natives' generosity.

3.6 Additional Robustness Checks

We now briefly summarize additional robustness checks contained in the Online Appendix.

Alternative instruments Appendix Table A5 shows our results remain virtually unchanged if we alter the construction of our instruments to allow for a range of potential challenges to our identifying

[17] Our analysis does not allow us to speak to the extent of *within-county* selective white flight. Since our primary effect of interest is at the county level, such white flight would not bias our estimates, but it may mask important heterogeneity: for example, our estimates may be driven by the natives who choose not to move away from ethnic enclaves. That we also find little treatment effect heterogeneity between large and small counties, or sparse and dense counties, is suggestive evidence that this is unlikely to be the case. We discuss heterogeneity in greater depth in Section 4.2.5.

assumptions. In our standard specification, we measure the "pull" factor (the county's attractiveness to the average migrant) using the number of migrants arriving in the county from other continents than f. Leaving out migrants arriving from the same continent insulates our instruments from any d-f specific confounding factors that may also affect migrants from (similar) neighboring countries. In Column 1, we measure the pull factor using only *European* migrants, that is, using only the choices made by migrants arriving from countries that are not in our donations sample. In Column 2, instead of leaving out migrants from any country f' in the same continent as f, we remove instead migrants from any country f' that historically has tended to send migrants to the United States at the same time.[18] Finally, in Column 3, we repeat the same robustness exercise for the calculation of our push factor, where instead of leaving out migrants from from f arriving in any d' in the same census region as d, we leave out any d' that historically tended to receive foreign migrants at the same time as d. The fact that all of these specifications yield almost identical results bolsters our confidence that they indeed isolate quasi-random variation in the ancestral composition of US counties.

In Appendix Table A6, we show that our results are virtually identical whether or not we include as excluded instruments the principal components summarizing the information contained in the higher order interactions of push and pull factors. Additionally, Appendix Table A7 shows our results remain stable even when we use only variation in migrations dating back more than 50 years for identification. Successively dropping the instruments corresponding to the interactions from most recent decades, the coefficient remains stable; only when we drop instruments corresponding to all decades after 1930 does the coefficient of interest lose statistical significance, but it nevertheless retains two thirds of its original size.

Family ties A key step in our analysis is to isolate donations from Americans who are themselves not descendants of migrants from the country receiving donations. Because none of the recipient countries in our dataset are European, in our standard specification, we restrict our sample to donors with European names. In Appendix Table A8, we impose alternative restrictions. Column 2 limits the sample to donors whose names likely originate from continents other than that of the recipient country, yielding an almost identical estimate ($\beta = 0.110$, s.e.=0.045). Column 3 instead limits the sample to donors with names from *countries* other than the recipient country, and we again find a similar estimate ($\beta = 0.116$, s.e.=0.048). Finally, we include all donors — including those whose names originate from the recipient country — in Column 4. As expected, the coefficient is higher ($\beta = 0.157$, s.e.=0.077), reflecting the natural tendency of people to donate to their ancestral country.

[18] Specifically, for every pair $\{f, f'\}$ of countries, we compute the correlation between migration from f and f', $corr\left(I^s_{f,d}, I^s_{f',d} | f, f'\right)$. If this correlation is above a 0.5 threshold and is statistically significant at the 5% level or below, we exclude f' from the construction of the pull factor for f.

One potential concern is that our primary, and most restrictive, sample choice — that is, limiting the sample to donors with European-origin names — fails to exclude some donors with ancestry from the country to which they are donating. For example, our procedure might fail to detect women from a non-European country who took the name of a spouse of European ancestry. While we cannot directly address this concern, it is reassuring that our estimates remain similar and significant if we limit our sample to men (see Section 4.3).

Sample restrictions In Appendix Table A3, we verify that all of our main results hold if we examine data from both charities individually (considering the full set of countries in both charities' datasets rather than restricting to those countries in both). Appendix Table A9 instead explores the robustness of our main finding to removing specific groups of foreign countries (Panel A) or domestic Census regions (Panel B), confirming that no specific group of countries or US Census region drives the overall effect.

Inference In Appendix Table A10, we present the standard errors associated with five alternate clustering choices — robust standard errors, clustering at the domestic county level, clustering at the domestic state level, clustering at the foreign country level, and two-way clustering by foreign country and domestic state — and show that our baseline two-way clustering at the country-county levels is conservative. As an alternative and more demanding approach to inference, we conduct a series of permutation tests, randomly matching each country in our dataset to another "placebo" country and swapping the endogenous variables (IHS-transformed ancestry) and the excluded instruments to those associated with the placebo country. We then estimate Equation (1) to recover, for example, an average of the effect of Peruvian ancestry on donations to Ethiopia, of Ethiopian ancestry on donations to Nepal, etc. Under the null hypothesis that cross-country spillovers are *on average* zero, the resulting regression coefficients will have mean zero. Consistent with this null, Appendix Figure A5 shows an approximately normal distribution of one thousand placebo coefficients centered on zero. The implied p-value for the effect of ancestry on donations in our main specification is 0.03.

Functional form Finally, Appendix Table A11 replicates our main specifications using the share of the population with ancestry from foreign country f as the endogenous variable, rather than IHS-transformed ancestry from f. Again, we find similar quantitative and qualitative results using this alternative approach.

4 Mechanisms

Having established that the long-run presence of particular immigrant groups increases natives' propensity to donate disproportionately toward those groups' ancestral countries, we next probe the mechanisms underlying this reduced-form effect. We first use our donations data to explore one aspect of heterogeneity of particular interest: the presence of first vs. higher-generation immigrants. We then investigate mechanisms in greater depth, focusing on a single group of particular policy relevance (Arab-Muslims) for which large-scale cross-county data on attitudes and political choice are available. We conclude by exploring the heterogeneity of the effect of exposure by political affiliation and gender.

4.1 Cultural Bridge: First vs. Higher Generation Immigrants

A small literature, primarily in sociology, has argued that natives' attitudes toward second-generation immigrants (those born in the United States, but whose parents, grandparents, etc. were of foreign birth) are more positive than attitudes toward first-generation immigrants (Barrera et al., 2021; Kuziemko and Ferrie, 2014; Kunst and Sam, 2014; Hernandez et al., 2008). Is the presence of second (and higher) generation immigrants also more effective in increasing natives' generosity toward these immigrants' ancestral country? To test this hypothesis, we estimate the marginal effect of first-generation vs. higher-generation immigrants by adding the IHS of the number of immigrants born in f who reside in d in 2010 as a second endogenous variable to Equation (1).

Naturally, the number of US born residents in d with ancestry from f is correlated with the number of immigrants from f in d. Thus, we verify that our instruments have sufficient statistical power to separately isolate variation in the number of descendants versus first-generation immigrants, reporting the Sanderson and Windmeijer (2016) conditional first-stage F-statistics of both variables.[19] Our instruments pass this test for both endogenous variables, indicating that they isolate independent exogenous variation in both variables and that we can interpret our coefficients as marginal effects. In other words, we can separately estimate the effect of exogenously changing the size of the ancestral population (holding fixed the number of first-generation immigrants) and the effect of exogenously changing the number of immigrants (holding fixed the size of the ancestral population).

Table 4 presents the results of this horserace. An exogenously larger foreign-born population from

[19] The Sanderson-Windmeijer F-statistic builds upon the conditional first-stage F-statistic proposed by Angrist and Pischke (2009) and allows the econometrician to bound the bias induced by weak instruments in linear IV models with multiple endogenous variables. The procedure is as follows. We first residualize the size of the ancestral population (the first endogenous variable) by the predicted number of immigrants (fitted values of the second endogenous variable predicted by our instruments) and examine the resulting first-stage F-statistic. We repeat both steps switching the order of the endogenous variables (that is, residualizing the number of immigrants by the predicted size of the ancestral population, then checking whether our instruments induce sufficient variation in the residualized values of the number of immigrants).

foreign country f increases the number of charitable donations to f (Column 1). But this effect entirely disappears when we control for the size of the population with foreign ancestry from f (Columns 2-4), instrumenting both endogenous variables with our standard set of excluded instruments. The effect of exposure to foreign ancestry is stable as we measure the stock of foreign ancestry at different points, 1990 (Column 2), 2000 (Column 3), or 2010 (Column 4); while the marginal effect of exposure to foreign-born migrants remains insignificant in all specifications. This difference suggests that descendants of migrants from recipient countries have a larger impact on donations made by white natives than foreign-born migrants themselves. This larger impact could reflect the fact US counties with large populations of foreign ancestry, but not foreign born, from f have been exposed to immigrants from f for a longer period of time, with the effect of exposure building up over time. Alternatively, it may be that second and higher-generation immigrants are better able to act as a *cultural bridge* between white natives and foreign countries, inducing greater generosity toward their ancestral countries.

4.2 Attitudes, Political Choices, Contact, and Knowledge

We now turn to more direct measures of altruism and prejudice by focusing our analysis on Arab-Muslims, a group which not only has experienced widespread discrimination in recent years, but for which several large-scale cross-county datasets are available. We pool the migration data across all countries in the Arab League and construct a single set of instruments for the distribution of residents with Arab-Muslim ancestry across US counties. We begin by replicating our estimates on donations for the pooled group of Arab-Muslims, then turn to a number of outcomes measuring attitudes, political choices, contact, and knowledge of Islam.

4.2.1 Charitable Donations toward Arab-Muslim Countries

To quantify the effect of exposure to Arab-Muslims on donations by local residents, we estimate a simplified version of Equation (1):

$$IHS\left(\#\text{Donations}_{d,Arab}^{t}\right) = \beta IHS\left(\text{Ancestry}_{d,Arab}^{t}\right) + \delta_t + \text{Controls}_d^t + \epsilon_{d,Arab}^{t}, \tag{6}$$

where, again, we instrument the (IHS-transformed) number of residents of Arab ancestry in domestic county d, $IHS\left(\text{Ancestry}_{d,Arab}\right)$, using Equation (1). As before, we restrict to donors who have European-ethnicity names to ensure that we are not capturing a natural tendency of people of Arab-Muslim descent to donate to their home countries.[20]

[20] Figure A7 also shows, reassuringly, that our IHS transformation, which is bounded at zero with $IHS(0) = 0$, does not alter the approximately log-linear relation between Arab-Muslim and county populations.

However, limiting our analysis to a single foreign group poses an additional challenge for identification because it precludes including county fixed effects. If some omitted county-level characteristics were correlated with both our instruments for Arab-Muslim ancestry and with local generosity towards Arab-Muslims, our estimates could be biased. Our earlier results from Table 1 — which demonstrate that our estimated IV coefficient changes little when we include county fixed effects — already suggest that any such bias may be limited in magnitude. Nevertheless, to address concerns about omitted variables more systematically, Figures A6 and A7 in the Appendix show the predicted distribution of Arab-Muslim ancestry across counties graphically. Both figures show wide variation, with no apparent tendency of Arab-Muslims clustering in specific parts of the country, and significant Arab-Muslim populations in both small and large population centers.

Next, we perform a balance test by projecting a wide range of demographic characteristics as of 2000 (percent rural, percent over 65, percent over 18, median HHI, unemployment rate, percent below the FPL, percent with a high school degree, percent with a college degree) on the predicted values of Arab-Muslim ancestry. Appendix Figure A8 plots the coefficients from this balance test. The figure shows four cross-sectional variables significantly correlated with predicted Arab-Muslim ancestry: counties with a larger predicted Arab ancestry are more likely to be rural, have a slightly higher share of residents over the age of 65 and below the federal poverty line, and have a slightly lower share of the local population with a high school degree. Reassuringly, in every specification below, adding controls for these demographic characteristics has no detectable effect on our estimates. Finally, we present in Section 4.2.2 a series of *placebo* outcomes measuring the effects of exposure to Arab-Muslims on attitudes toward other groups, and show these effects are uniformly small and generally statistically insignificant.

Table 5 shows estimates of Equation (6). Mirroring our previous findings, an exogenously larger Arab population in county d substantially increases the flow of donations from d to all Arab countries. The estimated effects are substantial: in our preferred specification (Column 3), a one-unit increase in the IHS-transformed Arab population causes a 0.400 increase in the IHS-transformed number of donations. The fact that this estimated elasticity of the number of donations with respect to ancestry is larger for Arabs as a group than for individual countries (0.107 in Table 1) suggests there may exist positive spillovers between communities originating from nearby countries, such that (for example) a larger community from Jordan may increase generosity towards Syria.[21]

[21] Consistent with such positive spillovers among Arab countries, Panel A of Appendix Table A12 shows spillovers between different ancestral groups: we investigate how donations to a given foreign country are affected by a larger local populations of residents with ancestry from the continent containing that country (excluding residents with ancestry from that country). Panel B additionally controls for the IHS-transformed population of residents with ancestry from the country in question. While coefficient estimates are less precise, the evidence suggests weak positive spillovers between geographically proximate countries.

These results are robust to controlling for a battery of county-level demographic controls (those identified in Appendix Figure A8 as potentially unbalanced between high and low Arab-Muslim ancestry counties) and state fixed effects. The OLS coefficient fluctuates substantially with the inclusion of controls, while the IV coefficients remain stable across variations; in particular, when we add controls for all of the unbalanced county characteristics, the coefficient of interest changes from 0.388 (s.e.=0.048) to 0.373 (s.e.=0.057). Adding the interaction of state and time fixed effects raises it slightly to 0.400 (s.e.=0.059). Thus, any other county-level omitted variables would have to have dramatically larger effects than these observables to materially impact our results. Our instruments thus appear to be effective at isolating exogenous variation in ancestry uncorrelated with other drivers of differential generosity.

4.2.2 Attitudes toward Arab-Muslims

We now turn to measures of attitudes toward Arab-Muslims. Because our data on attitudes comes from individual-level surveys, we are now also able to include individual-level controls. We limit the sample to white, non-Muslim respondents who were required to take the IAT for work or school. Our baseline specification is

$$\text{Attitude}_{i,d,Arab} = \beta IHS\left(\text{Ancestry}_{d,Arab}\right) + \text{Controls}_{i,d} + \epsilon_{i,d}, \tag{7}$$

where we again instrument the number of residents of Arab ancestry using first-stage Equation (2). This specification uses a single cross-section, so we omit time subscripts. A higher score of $\text{Attitude}_{i,d,Arab}$ signifies lower prejudice against Arab-Muslims. All specifications again control for logged county population in 2010, and standard errors are clustered at the county level.

Panel A of Table 6 displays the estimated effect of the presence of a population with Arab-Muslim ancestry on white, non-Muslim respondents' IAT score from Project Implicit (implicit bias); Panel B displays analogous estimates on the explicit measure of prejudice from Project Implicit (warmth). The key coefficient of interest represents the effect (in standard deviations) of a one-unit increase in IHS(Arab ancestry), approximately half a standard deviation, on the prejudice measure.

We find that our estimated coefficients are statistically significant and economically meaningful: in our preferred specification with individual controls (age, male, age squared, age $\times$ male) and state fixed effects (Column 3), a one-unit increase in the IHS-transformed population of Arab ancestry in a county (approximately half a standard deviation) causes a 0.073 (s.e.=0.026) standard deviation increase in average Arab-Muslim IAT scores and a 0.136 (s.e.=0.033) standard deviation increase in explicitly stated warmth (Panel B). We show these results graphically in Figure 2.[22] Our estimates remain stable with and without state fixed effects and as we introduce a series of "bad controls" (Angrist and Pischke, 2009): Column 5 shows that our estimate remains stable as we introduce a control for the non-European population, evidence that our effects are not simply capturing exposure to non-white residents in general. Column 6 instead controls for the average Race IAT score within county d, which measures the implicit attitudes of white respondents toward African-Americans, while Column 7 controls for the 2012 Republican vote share. The coefficient of interest remains stable across these variations, suggesting that our measures do not simply proxy for general prejudice against minorities or for political or social conservatism.

It is possible that "supply-side" mechanisms — such as companies matching donations to certain causes or individuals of a particular ancestral group raising donations for their ancestral country (DellaVigna et al., 2012) — partially explain the effects of ancestry on donations documented above.[23] The effects of ancestry on implicit and explicit attitudes, however, indicate that "demand-side" mechanisms are present as well: greater contact with a given ancestral group changes natives' private views and, plausibly through this channel, induces them to donate.[24]

Evidence on selection In the Appendix, we replicate our results using the full sample of Project Implicit respondents rather than restricting to respondents who were forced to take the Implicit Association Test for work or school. All of our results remain statistically significant and coefficient estimates change little, suggesting a limited role of endogenous selection of more tolerant residents taking the IAT to confirm their lack of prejudice (see Appendix Table A14). To further ensure that our results are not driven by selection into Project Implicit tests, we replicate our analysis using outcomes from Nationscape (Appendix Table A15), again with virtually identical results.

Attitudes toward other groups As further evidence that our regressions are capturing effects on natives' attitudes specifically toward Arab-Muslims, rather than toward immigrants or minorities

[22] To put this effect into perspective, a one-IHS increase in the size of the Arab-ancestry population roughly corresponds to going from the Arab-ancestry population of Kings County, NY to that of Wayne County, MI, or going from the Arab-ancestry population of St. Louis County, MO to San Mateo County, CA (see Appendix Figure A7).

[23] For example, one alternative interpretation of our results could be that charities might strategically target fundraising campaigns for causes in disaster-struck countries toward areas with larger communities with ancestry from that country. To evaluate this concern, we asked our contacts at Charities 1 and 2 for information about their fundraising strategies. Reassuringly, neither charity strategically targets counties based on ancestry, region, or demographics.

[24] Appendix Table A13 shows coefficient estimates on the four other measures of explicit attitudes toward Arab-Muslims from Project Implicit. We find strong and robust positive treatment effects on measures of *personal* beliefs (Columns 3 and 4), in line with our earlier estimates on warmth and implicit bias. However, we find weaker and less robust treatment effects on measures of social norms against Islamophobia (Columns 1 and 2). We view these results as suggestive evidence that exposure causally improves *private* attitudes toward Arab-Muslims, and that these changes in private attitudes are more important in explaining changes in behavior than changes in social norms.

more broadly, Appendix Table A16 investigates the effect of the presence of an Arab-Muslim ancestral population on white respondents' attitudes toward other groups. In Panel A, we find no statistically detectable effect of Arab ancestry on implicit attitudes towards Asians and Blacks, nor on respondents' explicit attitudes towards Asians. Interestingly, we do find a small positive effect of Arab ancestry on explicitly stated attitudes towards Blacks, which is about a quarter of the size of the direct effect on explicit attitudes towards Arab-Muslims. Such a spillover is consistent with the findings of Fouka and Tabellini (2022), who show that greater inflows of Hispanic immigrants improved natives' attitudes toward Blacks.[25] Because the sample of test-takers differs on observables between groups, we conduct a number of exercises to facilitate more direct comparison of the point estimates. Panel B reweights the sample to match the sample of Arab-Muslim test-takers on observables; Panel C limits the sample to counties represented in the Arab-Muslim IAT data; and Panel D both limits the sample to these counties and reweights. In all cases, the estimated effects on attitudes toward Arab-Muslim remain significantly larger than the effects on Asians or Blacks.

4.2.3 Political Choices

To what extent do these effects on attitudes translate into political choices? We consider two outcomes: support for the Muslim Ban and voting for presidential candidate Donald Trump in 2016. Table 7 shows coefficient estimates using our individual-level specification, Equation (7), again limiting to white, non-Muslim respondents. The results suggest that an exogenous increase in the presence of residents of Arab ancestry significantly reduces both support for the Muslim Ban (Panel A) and voting for Donald Trump in 2016 (Panel B): in our preferred specification (Column 3), a one-unit increase in the IHS of Arab ancestry decreases the probability that a respondent supports the Muslim Ban by 7.6 percentage points (s.e.=0.024) and the probability that a respondent voted for Trump in 2016, controlling for the respondent's county-level vote share for Romney in 2012, by 7.6 percentage points (s.e.=0.020). To put these magnitudes in perspective, half a standard deviation increase in the population of Arab ancestry reduces support for candidate Trump by as much as a 14 percentage point decrease in the 2012 Republican vote share. Appendix Table A17 replicates Table 7, controlling for respondents' *own* 2012 vote rather than the vote share of their county. Our sample size drops substantially because this question was only asked in the 2016 wave; nonetheless, we continue to find statistically significant effects of Arab presence on Trump voting, suggesting that the most saliently anti-Muslim presidential candidate in recent memory activated political preferences in a way that

[25] Although the point estimates of the effect of Arab-Muslim ancestry on implicit and explicit attitudes toward Asians and Black Americans are positive, they are substantially smaller than the analogous effects on attitudes toward Arab-Muslims. A t-test allows us to reject the null hypotheses of coefficient equality for both explicit placebos and for the Black implicit placebo at the 10% level.

Romney did not.

4.2.4 Contact and Personal Knowledge

To gain further insight into the mechanisms by which greater exposure to Arab-Muslims might affect implicit and explicit attitudes, political choices, and charitable donations, we turn to our custom survey. We evaluate two possible mechanisms: personal contact and knowledge. First, if a greater population of Arab-Muslims leads to more personal interaction with Arab-Muslims, it may improve attitudes and increase altruism, in line with the contact hypothesis (Allport, 1954). Second, even in the absence of direct personal contact, a larger Arab-Muslim community may increase knowledge of Arab-Muslims and Islam in general — due to, for example, greater and more accurate coverage on local media and social media or contact with social acquaintances who themselves have greater personal contact with Arab-Muslims. Such increased knowledge may translate into greater altruism if it leads residents to update negative priors (Grigorieff et al., 2020).

We first examine whether living in a county with an exogenously greater population of Arab-Muslims translates into greater personal contact with Arab-Muslims. In Panel A of Table 8, we estimate the effects of the IHS-transformed population with Arab ancestry in a respondent's county on several binary outcomes: whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor (Columns 1–3), and has eaten in a Middle Eastern restaurant (Column 4). Column 5 reports effects on a binary variable taking value one if any of the variables in Columns 1–3 take value one.[26] We find statistically significant effects on all outcomes except for the "friends" indicator (though the point estimate is positive). The effects are large — a one-unit increase in the IHS of the Arab population (approximately half a standard deviation) translates into an approximately 13% increase in the probability that the respondent has an Arab-Muslim friend, neighbor, or workplace acquaintance — and are robust to weak IV-robust inference.[27]

In Panel B of Table 8, we examine whether greater exposure to Arab-Muslims also translates into greater *knowledge* of Arab-Muslims and Islam in general. We examine effects on knowledge of the pillars of Islam (Column 2), knowledge of the definition of Ramadan (Column 3), knowledge of the share of Muslims in the United States (Column 4), and an index of these three outcomes (Column 5) constructed by scaling each of the three knowledge questions to mean zero and standard deviation one and summing the scaled values. In Column 1, we examine a specific outcome (derived from the

[26] We show these results graphically in Appendix Figure A9.

[27] The interpretation of these estimates is complicated by the usual concerns associated with self-reported outcomes: respondents may erroneously believe some acquaintances to be Arab-Muslim when they are not, or fail to recognize that some acquaintances are in fact Arab-Muslim. To the extent that systematic under- or over-reporting is correlated with the size of the Arab-Muslim population in a respondent's area, this could bias our estimates. However, these concerns are not relevant for verifiable outcomes, which we turn to next.

question on the pillars of Islam) specifically measuring beliefs about *negative* traits of Islam: whether "holy war against non-believers" and/or the "subservience of women and children to men" are among the Five Pillars.[28] A one-unit increase in the IHS-transformed Arab ancestry translates into a 0.38 standard deviation increase in the knowledge index.

4.2.5 Additional Robustness

We conduct three additional exercises to examine the robustness of our results. First, because it is not straightforward to map the outcomes studied in this section to a specific group of countries (particularly because many Muslim-majority countries are not Arab), we consider two alternative definitions in Appendix Table A18, constructing new instruments specifically for each: all countries targeted by the Muslim Ban (Panel B), and all Muslim-majority countries (Panel C). The results remain stable and significant. Second, in Appendix Table A19, we again replicate all of our specifications using the share of the population of Arab-Muslim ancestry, rather than the IHS-transformed population, as our endogenous variable. All coefficient estimates are strong and statistically significant. Finally, Appendix Table A20 shows that, as with our main results, there is no evidence of selective white flight that could result in white residents who dislike Arabs moving towards counties with relatively few Arabs. If anything, white residents leaving counties with large Arab presence tend to relocate to areas with even larger Arab populations, conditional on moving at all.

4.3 (Lack of) heterogeneity

We conclude this section by examining whether the effect of the presence of descendants of foreign migrants is heterogeneous across different types of counties, or different types of natives' characteristics. For instance, one may expect the positive effect of ancestry on attitudes towards foreigners to be weaker in more conservative counties, or even reverse sign, a form of backlash effect. Figure 3 (left column) shows there is no such heterogeneity between conservative and liberal counties across any of the eight outcomes we study: all donations, Arab donations, Arab-Muslim IAT scores, warmth toward Arab-Muslims, support for the Muslim ban, Trump votes in 2016, our index of contact with Arab-Muslims, or our index of knowledge of Islam. Even though residents in more conservative counties tend to be less favorable (e.g. they are more likely to vote for Republican candidate Trump in 2016), they respond to the presence of foreign ancestry in a similar way as residents in more liberal counties.

With one exception (donations to Arab countries), the other columns of Figure 3 show no evidence of heterogeneity along several other important dimensions. The effect of the presence of foreign

[28] This outcome takes a value of two if the respondent indicated that both traits are among the Five Pillars, a value of one if the respondent indicated that one of the two is among the Five Pillars, and a value of zero if the respondent indicated that neither is among the Five Pillars.

ancestry on attitudes is similar in small and large population counties, despite the fact that residents in larger counties may have more freedom to strategically move away from specific foreign ancestry groups within their county. The effect is also similar in sparsely and densely populated counties, despite the fact that residents in denser counties may have more frequent interactions with all residents in their county. Finally, the effect is also similar for male and female respondents, the only characteristic we can observe (or infer) across our different data at the individual level. This absence of heterogeneity by gender alleviates the potential concern that our effects are driven by women from non-European ancestry who took European last names.

5 Conclusion

We examine the effect of the decades-long presence of foreign-origin groups on natives' generosity, attitudes, and political choices toward them, exploiting exogenous variation in the ancestral composition of US counties generated by historical immigration "push" and "pull" factors. We find that exposure to a larger population with ancestry from a given country induces greater generosity toward that group. Focusing on the case of Arab-Muslims to examine mechanisms, we find that exposure to Arab-Muslims leads to more positive stated attitudes and lower implicit prejudice, lower support for the "Muslim Ban" and for the then-candidate Trump, and greater charitable donations to Arab countries. We provide suggestive evidence that greater personal contact with and greater knowledge of Arab-Muslims may underly these effects.

We add two primary caveats to our analysis. First, our focus is on the types of long-run effects relevant for aggregate outcomes. While we are able to characterize these average effects in some detail, we do not claim that every interaction between an American of European descent with a neighbor of Arab descent reduces prejudice, nor that the presence of Arab-Americans always induces positive attitudes toward Arabs. Instead, our work characterizes the sum of the effects of the long-run presence of foreign ethnic groups. Second, groups we examine — both in our generalized analysis and in our case study of Arab Muslims — constitute relatively small fractions of the population in most counties; long-run exposure to much larger groups may fail to induce positive effects or even lead to backlash.

Our results suggest several directions for further research. In particular, several aspects of heterogeneity deserve closer attention. For example, are the positive effects of exposure muted — or even reversed — when local economic conditions are poor and out-groups may be seen as competitors for scarce jobs, or when immigrants cluster into ethnic enclaves? Second, our results on implicit and explicit prejudice, political choices, contact, and knowledge focus on Arab-Muslims. This is a sizeable group which has faced increasing discrimination and political hostility in recent years, but not all results may generalize to other minorities, such as Latinos, East Asians, or South Asians — particularly given the different stereotypes associated with these groups. Finally, what are the dynamics of attitudes toward immigrants — how do short-term effects differ from long-term effects? — and, relatedly, how does the vertical transmission of beliefs about immigrant groups from parents to children mediate the effects of exposure?

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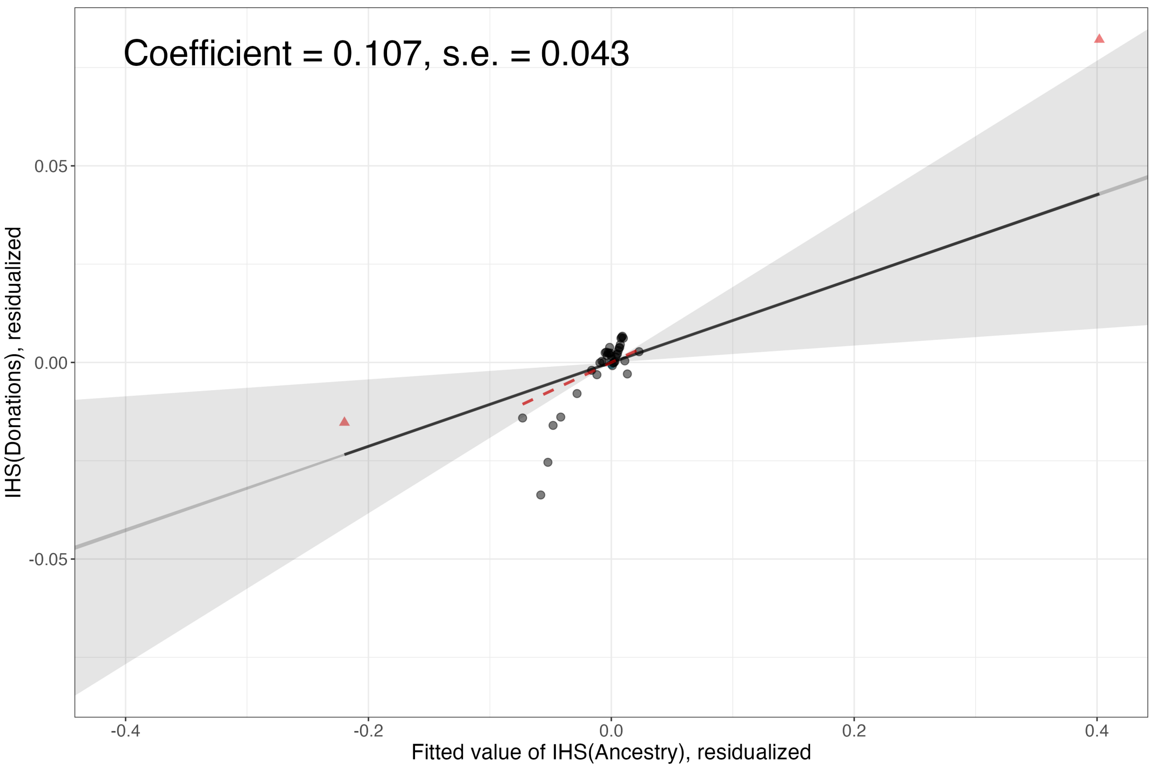
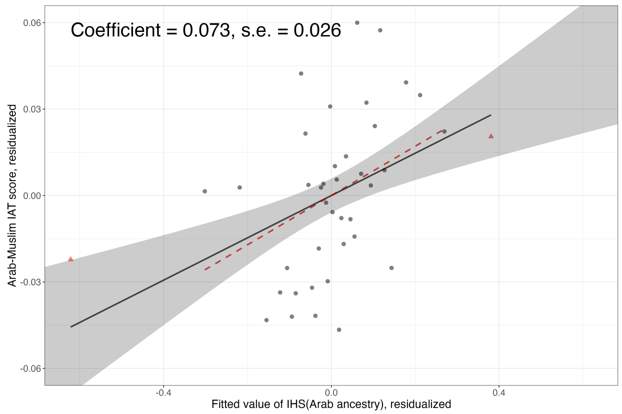
Tables and Figures

Figure 1: Binned scatter plot of donations

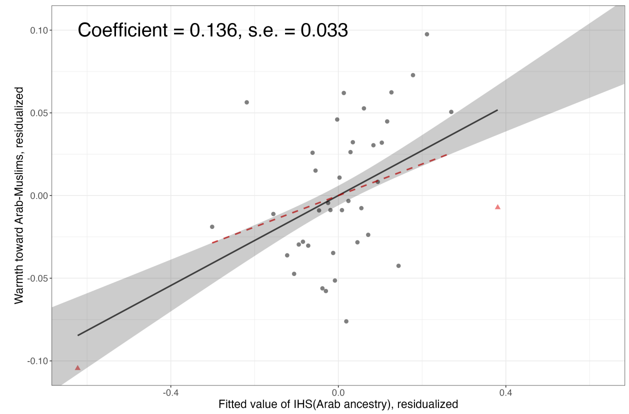


Notes: Figure 1 presents a binned scatter plot visualizing the relationship between the IHS-transformed number of donations from county to country in a given quarter and the IHS-transformed size of the ancestral population from that country. We include $I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. We residualize by the fixed effects and controls included in Column 4 of Table 1. The bin in blue contains all country-county-quarter observations with zero ancestry. Red triangles are used to indicate the top and bottom 2.5% of the data by fitted values; the red dotted line indicates the regression fit after dropping these observations. Standard errors are clustered at the county and country levels. 95% confidence intervals are reported.

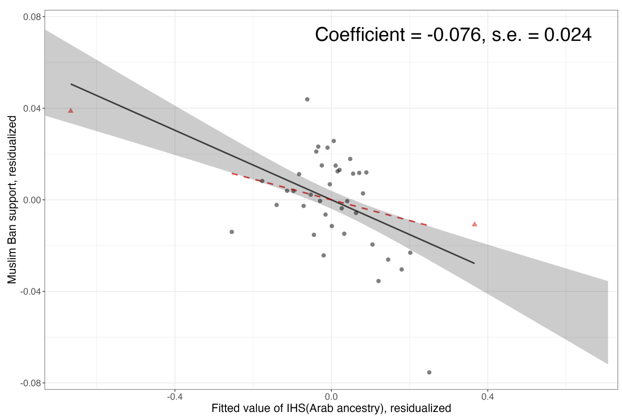
Figure 2: Binned scatter plots of attitudes and political preferences



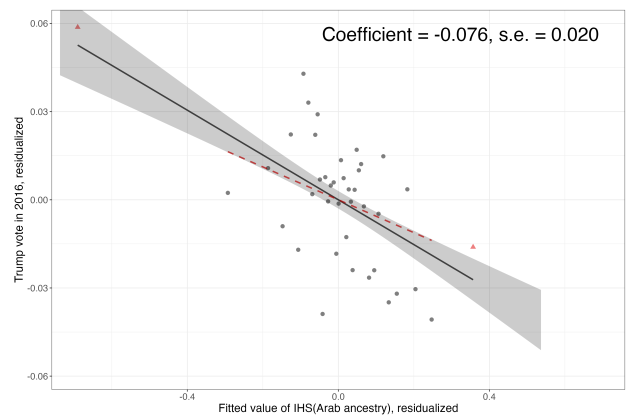
(a) Score on Arab-Muslim IAT (Project Implicit)



(b) Warmth toward Arab-Muslims (Project Implicit)



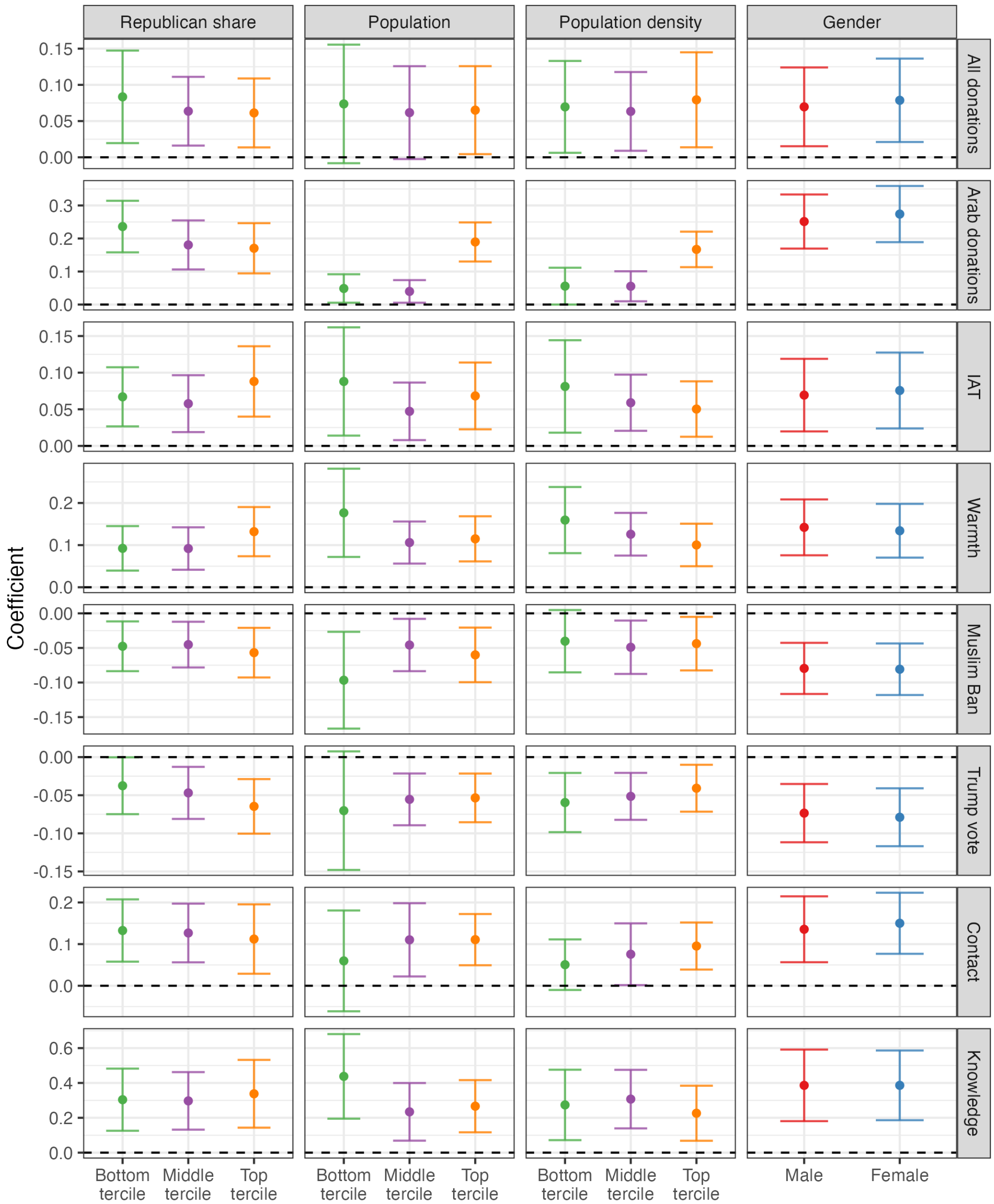
(c) Muslim Ban support (CCES)



(d) Trump vote in 2016 (CCES)

Notes: Figure 2 presents binned scatter plots displaying the relationship between the fitted values of IHS(Arab ancestry) and four outcomes: scores on the Arab-Muslim IAT, reported warmth toward Arab-Muslims, support for the Muslim Ban, and Trump voting in 2016. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. We residualize by the controls used in Column 3 of Table 6. Red triangles are used to indicate the top and bottom 2.5% of the data by fitted values; the red dotted line indicates the regression fit after dropping these observations. Standard errors are clustered at the county level. 95% confidence intervals are reported.

Figure 3: Heterogeneity by gender, population, population density, and Republican vote share



Notes: Figure 3 presents the estimated coefficients on the interactions between indicator variables for individual (gender) or county-level (population, population density, and Republican vote share) characteristics and our measure of ancestry. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,...,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments, as well as the interactions of all of these variables with the characteristic indicators. Controls in the first row are those used in Column 4 of Table 1; controls in the second row are those used in Column 3 of Table 5; controls in third through sixth row are those used in Column 4 of Table 6; controls in the final two rows are those used in Column 5 of Table 8. Error bars represent 95% confidence intervals.

Table 1: Effect of ancestral presence on donations

	(1)	(2)	(3)	(4)	(5)	(6)
	IHS(# donations)				Donations (dummy)	IHS($ donations)
Panel A: IV						
IHS(Ancestry)	0.139	0.132	0.132	0.107	0.047	0.329
	(0.028)	(0.032)	(0.033)	(0.043)	(0.021)	(0.136)
First-stage F-statistic	417.1	404.2	393.6	330.6	330.6	337.8
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Panel B: OLS						
IHS(Ancestry)	0.015	0.010	0.009	0.004	0.002	0.016
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.013)
Dep. var. mean	0.019	0.019	0.019	0.019	0.015	0.078
Dep. var. sd	0.182	0.182	0.182	0.182	0.121	0.652
Observations	4,703,862	4,700,864	4,700,864	4,703,862	4,703,862	3,972,708
Foreign country × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	No	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	—	—	—
US state × quarter FE	No	No	Yes	—	—	—
US county × quarter FE	No	No	No	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable in Columns 1–4 is the IHS-transformed number of donations from county to country in a quarter. The dependent variable in Column 5 is a dummy for the presence of at least one donation from county to country in a quarter. The dependent variable in Column 6 is the IHS-transformed total value of donations from county to country in a quarter (available only for Charity 2). The main variable of interest is the IHS-transformed population with ancestry from country f. In Panel A, in all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. Columns 1–3 control for log 2010 population. Columns 2–6 include logged county-country distance and latitude difference. Columns 2 and 3 include the following county-level demographic controls (as of 2000): the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Table 2: Effect of ancestral presence on white flight

	(1)	(2)
Panel A:	*Selective white flight index*	
IHS(Ancestry)	0.035	−0.009
	(0.010)	(0.007)
First-stage F-statistic	69.78	44.93
Weak IV-robust p-value	< 0.01	< 0.01
Dep. var. mean	0.036	0.036
Dep. var. s.d.	0.061	0.061
Observations	363,802	363,802
Panel B:	*Selective white flight index, by subgroup*	
IHS(Ancestry) × Married	−0.002	−0.002
	(0.002)	(0.002)
IHS(Ancestry) × Female	−0.0001	−0.0001
	(0.0004)	(0.0004)
IHS(Ancestry) × College	0.002	0.002
	(0.002)	(0.002)
IHS(Ancestry) × Age	0.001	0.001
	(0.001)	(0.001)
IHS(Ancestry) × Income	0.001	0.001
	(0.001)	(0.001)
Year FE	Yes	Yes
Foreign country FE	Yes	Yes
US county FE	No	Yes

Notes: The table presents coefficient estimates from regressions at the country-county-decade level. The dependent variable is the selective white flight index, defined in Section 3.4; in Panel A, the index is computed from the full sample, whereas in Panel B, two separate indices are computed for each dimension of heterogeneity (one for each subgroup). The endogenous variable in Panel A is the IHS-transformed population with ancestry from country f. Each row of Panel B presents a separate regression of the selective white flight index for a given subgroup on an indicator for the subgroup, the IHS-transformed population with ancestry from country f, and the interaction of the indicator and IHS-transformed ancestral population. The excluded instruments in Panel A are $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,1980}$ and the first five principal components of the higher-order interactions; in Panel B, we additionally include as instruments the interaction of each instrument with the subgroup indicator. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Table 3: Effect of ancestral presence on donations: Investigating local vs. aggregate effects

	(1)	(2)	(3)	(4)	(5)	(6)
	IHS(# donations) to...					
	Country					Continent
IHS(Ancestry) from country in county	0.107	0.066	0.132			
	(0.043)	(0.015)	(0.033)			
IHS(Ancestry) from country in CZ				0.219		
				(0.104)		
IHS(Ancestry) from country in state					0.534	
					(0.225)	
IHS(Ancestry) from continent in county						0.341
						(0.113)
First-stage F-statistic	330.6	492.1	393.6	177.5	90.36	84.16
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.019	0.019	0.019	0.061	0.551	0.122
Dep. var. sd	0.182	0.182	0.182	0.348	1.045	0.512
Observations	4,703,862	4,700,864	4,700,864	1,062,791	76,449	489,528
Foreign country × quarter FE	Yes	No	Yes	Yes	Yes	No
Foreign continent × quarter FE	No	No	No	No	No	Yes
Distance controls	Yes	Yes	Yes	Yes	Yes	No
Demographic controls	—	Yes	Yes	—	—	—
US state × quarter FE	—	No	Yes	No	Yes	—
US commuting zone × quarter FE	—	No	No	Yes	No	—
US county × quarter FE	Yes	No	No	No	No	Yes

Notes: Table 3 presents variants of our primary county-level specification. In Columns 1–3, observations are at the county-country-quarter level; in Columns 4-6 observations are at the commuting zone-country-quarter, state-country-quarter level, and county-continent-quarter levels, respectively. Columns 1 and 3 replicate Columns 4 and 1 of Table 1, respectively. Column 2 drops US state × quarter and foreign country × quarter fixed effects. Column 4 presents the coefficient from the analogous instrumental variables regression at the commuting zone, rather than county, level: that is, the dependent variable (IHS-transformed number of donations), the endogenous variable of interest (IHS-transformed ancestry), and the instruments are calculated at the commuting zone level. Column 5 aggregates analogously to the state level. Column 6 instead aggregates *foreign countries* to the continent level: that is, it presents a regression of the IHS-transformed number of donations to all countries in a given continent on the county-level IHS-transformed ancestry from all countries in that continent. Columns 1–5 include logged county-country distance and latitude difference. Columns 2–3 control for log 2010 population and include the following county-level demographic controls (as of 2000): the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses and are clustered at the foreign country and domestic county levels in Columns 1–3, at the foreign country and commuting zone level in Column 4, at the foreign country and state level in Column 5, and at the foreign continent and domestic county level in Column 6.

Table 4: Ancestral presence vs. presence of first-generation immigrants

	(1)	(2)	(3)	(4)
	IHS(# donations)			
IHS(Foreign-born 2010)	0.192	0.014	−0.001	−0.027
	(0.079)	(0.085)	(0.091)	(0.112)
IHS(Ancestry 1990)		0.089		
		(0.032)		
IHS(Ancestry 2000)			0.100	
			(0.043)	
IHS(Ancestry 2010)				0.120
				(0.063)
F-stat IHS(Foreign-born 2010)	39.73	20.06	27.77	23.32
F-stat IHS(Ancestry)	—	20.32	22.61	19.72
Dep. var. mean	0.019	0.019	0.019	0.019
Dep. var. sd	0.182	0.182	0.182	0.182
Observations	4,703,862	4,703,862	4,703,862	4,703,862
Foreign country × quarter FE	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable is the IHS-transformed number of donations from county to country in a quarter. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for logged county-country distance and latitude difference. The table reports Sanderson-Windmeijer conditional first-stage F-statistics. Standard errors are clustered at the foreign country and domestic county levels.

Table 5: Effect of presence of Arab ancestry on donations toward Arab countries

	(1)	(2)	(3)
Panel A: IV	IHS(# donations)		
IHS(Arab ancestry)	0.388	0.373	0.400
	(0.048)	(0.057)	(0.059)
First-stage F-statistic	465.9	358.8	317.0
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01
Panel B: OLS	IHS(# donations)		
IHS(Arab ancestry)	0.027	0.013	0.011
	(0.002)	(0.002)	(0.002)
Dep. var. mean	0.048	0.048	0.048
Dep. var. sd	0.296	0.296	0.296
Observations	150,096	150,096	150,096
Quarter FE	Yes	Yes	—
Distance controls	No	Yes	Yes
Demographic controls	No	Yes	Yes
US state × quarter FE	No	No	Yes

Notes: The table presents coefficient estimates from regressions at the county-quarter level. Only donations to Arab League countries are included. The dependent variable is the IHS-transformed number of donations from the county to Arab League countries in a quarter. The main variable of interest is the IHS-transformed population with ancestry from Arab countries. In Panel A, in all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Columns 2 and 3 include average logged county-country distance, average latitude difference, and the following county-level demographic controls (as of 2000): the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Table 6: Effect of presence of Arab ancestry on attitudes toward Arab-Muslims

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Panel A:	*Score on Arab-Muslim IAT (std., higher score = less prejudiced)*						
IHS(Arab ancestry)	0.013	0.070	0.073	0.065	0.067	0.052	0.052
	(0.006)	(0.018)	(0.026)	(0.025)	(0.026)	(0.023)	(0.024)
IHS(non-Euro ancestry)					−0.009		
					(0.018)		
Avg. race IAT score						0.348	
						(0.065)	
2012 Rep. vote share							−0.129
							(0.053)
AP F-statistic	—	12.41	9.808	6.572	6.402	6.604	6.287
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	107,083	107,083	105,968	105,968	105,968	105,968	105,968
Panel B:	*Warmth toward Arab-Muslims (std., higher score = more favorable)*						
IHS(Arab ancestry)	0.043	0.155	0.136	0.108	0.116	0.086	0.088
	(0.008)	(0.029)	(0.033)	(0.031)	(0.029)	(0.027)	(0.031)
IHS(non-Euro ancestry)					−0.046		
					(0.020)		
Avg. race IAT score						0.590	
						(0.085)	
2012 Rep. vote share							−0.272
							(0.073)
AP F-statistic	—	12.51	9.852	6.518	6.360	6.560	6.230
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	106,956	106,956	105,856	105,856	105,856	105,855	105,856
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Individual-level demographics	No	No	Yes	Yes	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variable in Panel A is the score on the Arab-Muslim IAT (from Project Implicit); the dependent variable in Panel B is the stated warmth toward Arab-Muslims (also from Project Implicit). Both measures are scaled to take mean zero and standard deviation one. Only respondents who self-reported their reason for taking the Project Implicit test as "Assigned for work," "Assigned for school," or "Assigned for discussion group" are included. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age × male. County-level demographic controls are as of 2000 and include the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Table 7: Effect of presence of Arab ancestry on political preferences

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV
Panel A:			*Support for the Muslim Ban*		
IHS(Arab ancestry)	−0.033	−0.098	−0.076	−0.038	−0.044
	(0.005)	(0.036)	(0.024)	(0.022)	(0.021)
IHS(non-Euro ancestry)					−0.001
					(0.012)
AP F-statistic	—	16.80	9.516	5.150	4.995
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.530	0.530	0.530	0.530	0.530
Dep. var. sd	0.499	0.499	0.499	0.499	0.499
Observations	56,837	56,837	56,837	56,837	56,837
Panel B:			*Voted for Trump in 2016*		
IHS(Arab ancestry)	−0.015	−0.056	−0.076	−0.045	−0.052
	(0.004)	(0.019)	(0.020)	(0.021)	(0.021)
IHS(non-Euro ancestry)					0.007
					(0.012)
2012 Rep. vote share	0.635	0.578	0.526	0.512	0.513
	(0.033)	(0.043)	(0.032)	(0.035)	(0.033)
AP F-statistic	—	19.11	10.67	5.292	5.179
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.464	0.464	0.464	0.464	0.464
Dep. var. sd	0.499	0.499	0.499	0.499	0.499
Observations	97,403	97,403	97,403	97,403	97,403
State FE	No	No	Yes	Yes	Yes
Individual-level demographics	No	No	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variable in Panel A is stated support for the Muslim Ban; the dependent variable in Panel B is self-reported Trump votership. The data is from the CCES. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age × male. County-level demographic controls are as of 2000 and include the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Table 8: Effect of presence of Arab ancestry on contact and knowledge

	(1)	(2)	(3)	(4)	(5)
Panel A:	*Contact with Arab-Muslims*				
	Friends	Workplace	Neighbors	Restaurant	Any (1–3)
IHS(Arab ancestry)	0.037	0.102	0.090	0.115	0.129
	(0.025)	(0.037)	(0.025)	(0.043)	(0.038)
AP F-statistic	9.185	9.185	9.185	8.464	8.464
Weak IV-robust p-value	< 0.01	< 0.01	0.67	< 0.01	< 0.01
Dep. var. mean	0.098	0.285	0.198	0.439	0.396
Dep. var. std. dev	0.297	0.452	0.399	0.496	0.489
Observations	5,189	5,189	5,189	5,189	5,189
Panel B:	*Knowledge of Arab-Muslims*				
	Subservice/war	Pillars	Ramadan	Pop. accuracy	Index (2–4)
IHS(Arab ancestry)	−0.130	0.434	0.108	2.952	0.377
	(0.053)	(0.149)	(0.040)	(1.054)	(0.103)
AP F-statistic	8.464	8.464	8.464	8.053	8.053
Weak IV-robust p-value	0.23	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.590	4.492	0.764	-15.070	0.000
Dep. var. std. dev	0.758	1.558	0.425	13.628	1.000
Observations	5,020	5,020	5,020	4,729	4,729
Demographics	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. In Panel A, the dependent variables in Columns 1–3 are indicators for whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor, respectively; the dependent variable in Column 4 is an indicator for whether the respondent reports having ever eaten at a Middle Eastern restaurant; and the dependent variable in Column 5 is an indicator taking value one if any of the indicators in Columns 1–3 take value one. In Panel B, the dependent variable in Column 1 takes value 0 if the respondent answered that neither "holy war against non-believers" and "subservience of women and children to men" are among the Five Pillars of Islam, value 1 if the respondent answered that one of these two are among the Five Pillars; and value 2 if the respondent answered that both are among the Five Pillars. The dependent variable in Column 2 is the respondent's total score on the "pillars" question (ranging from 0 to 7). The dependent variable in Column 3 is an indicator for whether the respondent correctly answered the Ramadan question. The dependent variable in Column 4 is the negative absolute value of the difference between the respondent's guess as to the size of the Muslim population in the US and the actual size of the Muslim population in the US. Respondents with invalid guesses ($< 0\%$ or $> 100\%$) were dropped. The dependent variable in Column 5 is constructed by scaling the dependent variables in Columns 2–4 to mean zero and standard deviation one, summing these three scaled values, and renormalizing. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age $\times$ male. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Online Appendix

"The Immigrant Next Door"

Leonardo Bursztyn

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A Additional Tables and Figures

Appendix Table A1: Summary statistics

	Obs.	Mean	Std. dev.	Median	Min	Max
Panel A: County-country-quarter level						
A.1: Ancestry, Charity 1 and 2						
2010 population from country f (thousands)	4,703,862	0.236	9.453	0.000	0.000	2,629.376
2010 IHS-transformed population from country f	4,703,862	1.188	2.073	0.007	0.000	15.475
2010 share of population from country f ($\times 100$)	4,703,862	0.124	1.356	0.000	0.000	72.765
A.2: Donations, Charity 1 and 2						
IHS-transformed number of donations to country f	4,703,862	0.019	0.182	0.000	0.000	7.71
A.3: Donations, Charity 2 only						
IHS-transformed dollar value of donations to country f	3,972,708	0.08	0.65	0.00	0.00	11.84
Panel B: County-quarter level						
B.1: Donations to Arab countries						
IHS-transformed number of donations	150,096	0.048	0.296	0.000	0.000	6.397
Panel C: Individual level						
C.1: Project Implicit						
Arab-Muslim IAT score	107,083	0.017	0.989	0.002	-4.208	4.39
Warmth toward Arab-Muslims	106,956	0.034	0.996	-0.315	-2.567	1.938
C.2: CCES						
Support for the Muslim Ban	56,837	0.530	0.499	1.000	0.000	1
Voted for Trump in 2016	97,576	0.464	0.499	0.000	0.000	1
C.3: Nationscape						
Favorability toward Arab-Muslims	188,411	-0.073	1.002	0.313	-1.668	1.304
Support for the Muslim Ban	58,466	0.309	0.462	0.000	0.000	1
Voted for Trump in 2016	171,150	0.534	0.499	1.000	0.000	1

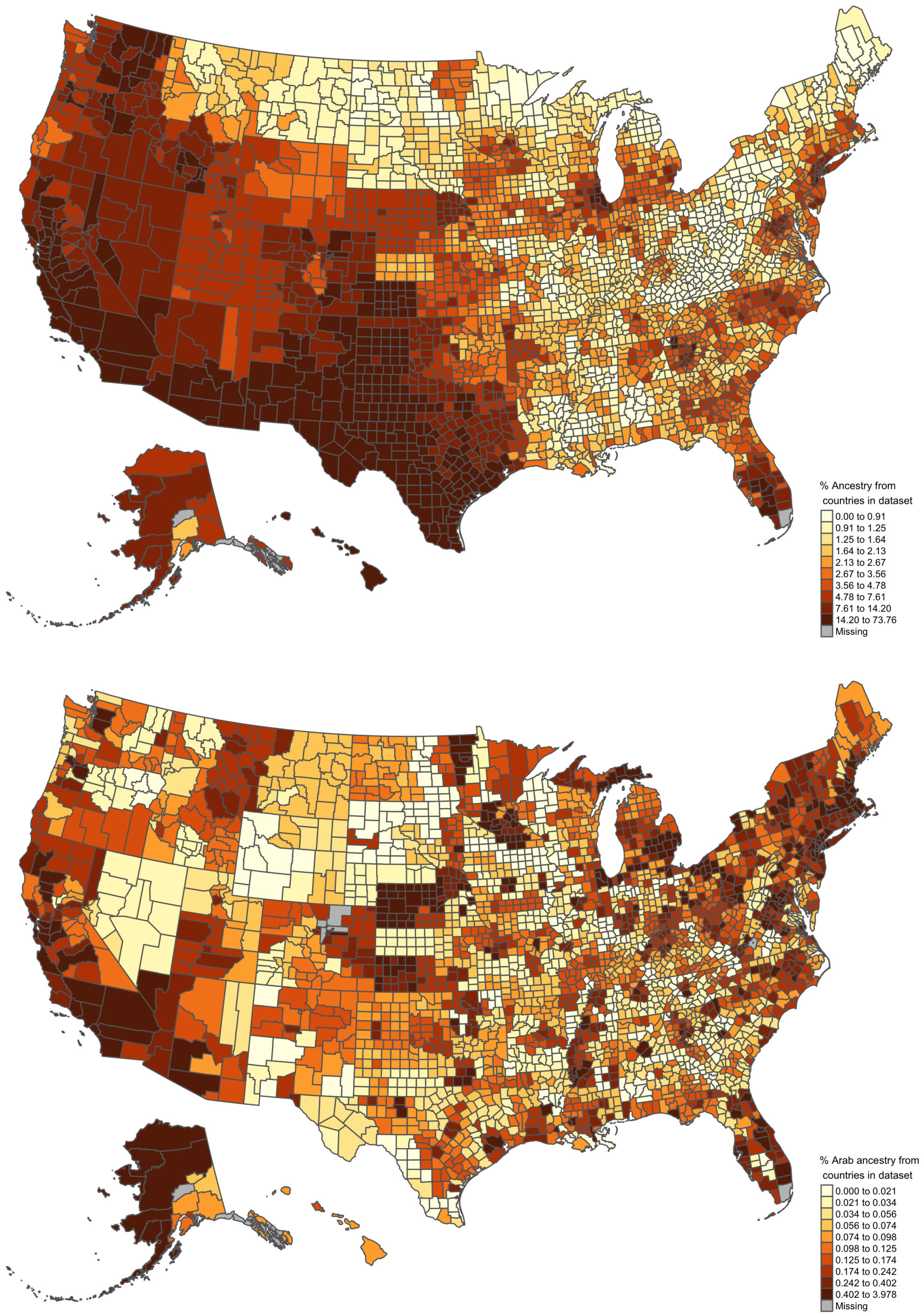
Notes: The table presents summary statistics for all datasets used in the main analyses except the custom survey (summary statistics for which are presented in Appendix Table A4). Donations statistics are calculated from the pooled donations across Charity 1 and Charity 2.

Appendix Table A2: Top ten foreign countries by size of ancestral population

	Ancestry (thousands)	# counties	Peak arrival time
Mexico	22,903.85	3,136	1990-2000
Philippines	2,729.48	3,136	1990-2000
India	2,433.13	3,108	2000-2010
Japan	1,144.04	3,105	1990-2000
Haiti	868.67	2,596	1990-2000
Peru	662.80	3,125	2000-2010
Ecuador	606.75	3,121	2000-2010
Iran	419.04	2,882	1980-1990
Lebanon	371.66	3,047	1980-1990
Pakistan	371.52	2,844	1990-2000

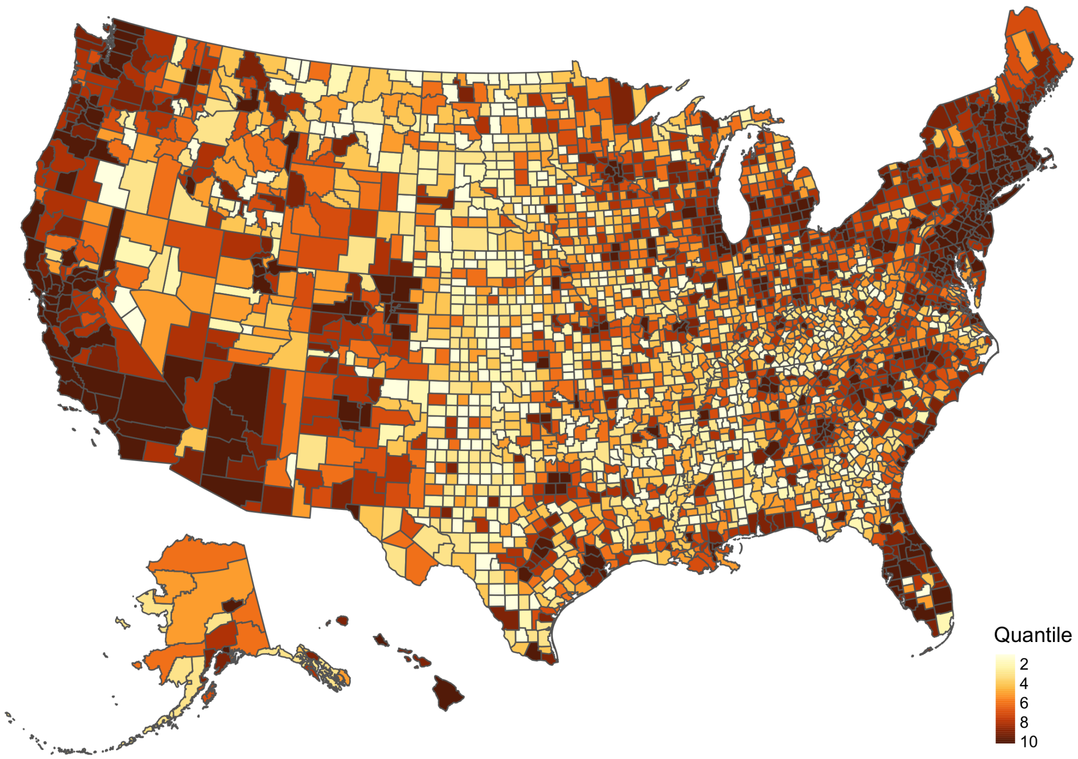
Notes: Table A2 lists the top ten countries in our sample by size of ancestral population. For each country, Column 1 displays the size of the ancestral population (in thousands); Column 2 displays the number of counties with nonzero ancestral population; and Column 3 displays the decade in which the maximum number of immigrants from that country arrived in the U.S.

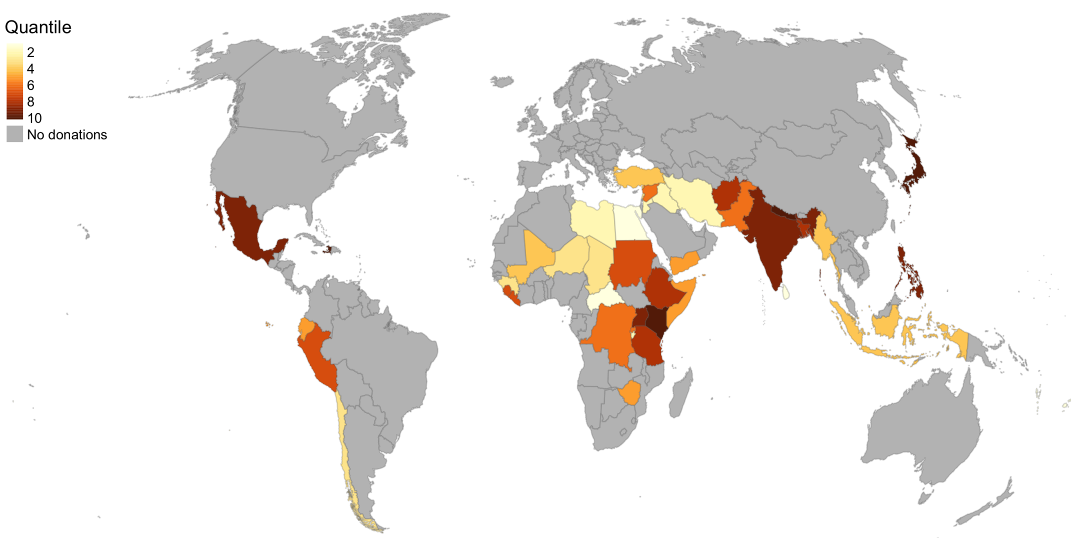
Appendix Figure A1: Foreign ancestry share (top) and Arab ancestry share (bottom)



Notes: The top map plots the share of each county's population with ancestry from a country in our donations dataset. The bottom map plots the share of each county's population with ancestry from Arab countries in our donations dataset.

Appendix Figure A2: Donations by origin (top) and destination (bottom)





Notes: The top map plots the quantile of the number of donations in our dataset emanating from each domestic county. The bottom map plots the quantile of the number of donations in our dataset to each foreign country.

Appendix Table A3: Effect of ancestral presence on donations, separated by charity

	IHS(# donations)	Donations (dummy)	IHS($ donations)
Panel A: Charity 1			
IHS(Ancestry)	0.042	0.019	—
	(0.014)	(0.006)	—
First-stage F-statistic	53.79	53.79	—
Weak IV-robust p-value	< 0.01	< 0.01	
Dep. var. mean	0.009	0.007	—
Dep. var. sd	0.128	0.082	—
Observations	2,193,462	2,193,462	—
Panel B: Charity 2			
IHS(Ancestry)	0.068	0.033	0.203
	(0.030)	(0.015)	(0.090)
First-stage F-statistic	309.9	309.9	309.9
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.013	0.010	0.051
Dep. var. sd	0.146	0.101	0.529
Observations	9,410,862	9,410,862	9,410,862
Foreign country × quarter FE	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes

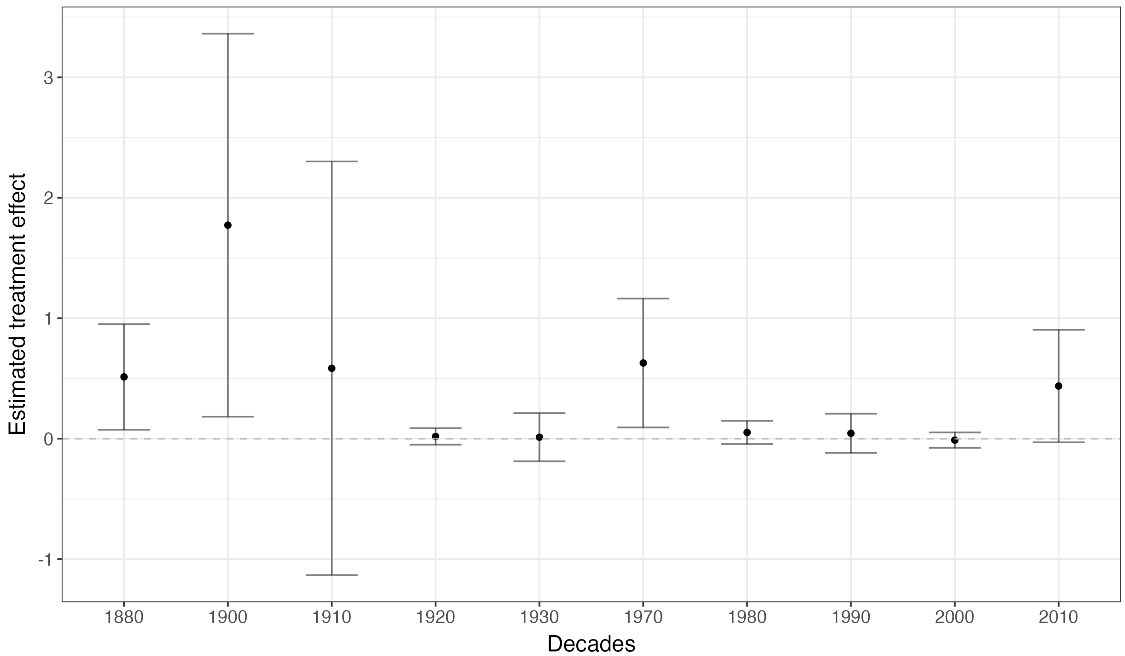
Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable in Column 1 is the IHS-transformed number of donations from county to country in a quarter. The dependent variable in Column 2 is a dummy for the presence of at least one donation from county to country in a quarter. The dependent variable in Column 3 is the IHS-transformed total value of donations from county to country in a quarter. The main variable of interest is the IHS-transformed population with ancestry from country f in county d. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\dots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for logged county-country distance and latitude difference. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Table A4: Survey Representativeness

	Survey mean	CCES mean
Age	52.392	50.344
Male	0.458	0.460
Hispanic	0.049	0.027
High school degree or higher	0.984	0.967
Family income		
under \$20,000	0.071	0.121
\$20,000 - 39,999	0.197	0.220
\$40,000 - 59,999	0.197	0.197
\$60,000 - 79,999	0.165	0.159
\$80,000 - 99,999	0.108	0.100
\$100,000 - 120,000	0.117	0.071
over \$120,000	0.145	0.131
Census region		
Midwest	0.245	0.253
Northeast	0.169	0.199
South	0.385	0.349
West	0.201	0.200
Observations	5,032	115,930

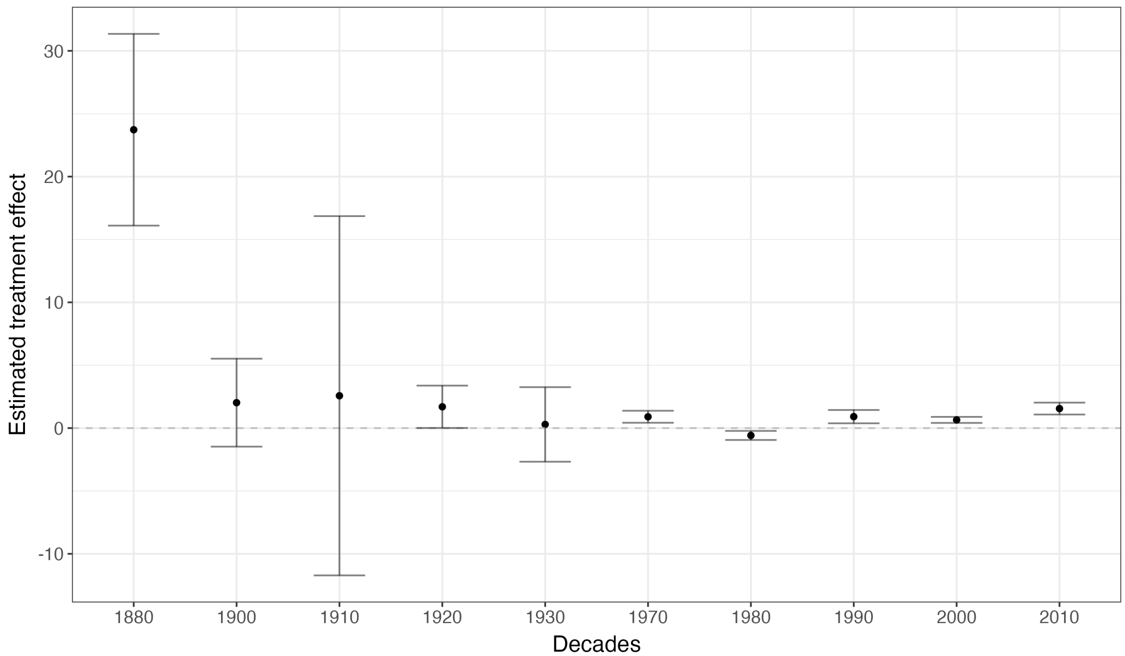
Notes: Column 1 presents means of respondent characteristics from our survey. Column 2 presents means of respondent characteristics from the 2016-2019 waves of the CCES.

Appendix Figure A3: First-stage coefficients: all countries



Notes: Figure A3 presents coefficient estimates from regressions of IHS-transformed ancestry on the instruments in Equation (2). Following Burchardi et al. (2019), to facilitate the interpretation of coefficients as the marginal effect of migrations in that period, we sequentially orthogonalize each instrument with respect to the previous instruments. Error bars indicate 90% confidence intervals. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Figure A4: First-stage coefficients: Arab-Muslim countries



Notes: Figure A4 presents coefficient estimates from regressions of IHS-transformed Arab-Muslim ancestry on the instruments in Equation (2). Following Burchardi et al. (2019), to facilitate the interpretation of coefficients as the marginal effect of migrations in that period, we sequentially orthogonalize each instrument with respect to the previous instruments. Error bars indicate 90% confidence intervals. Standard errors are clustered at the domestic county level.

Appendix Table A5: Stability of estimated effect of ancestral presence on donations, varying instruments

	(1)	(2)	(3)
	Eur. only pull	Excl. corr. origins	Excl. corr. dest.
IHS(Ancestry)	0.099	0.095	0.106
	(0.040)	(0.041)	(0.046)
First-stage F-statistic	133.3	160.0	202.0
Weak IV-robust p-value	< 0.01	< 0.01	0.35
Dep. var. mean	0.019	0.019	0.019
Dep. var. s.d.	0.182	0.182	0.182
Observations	4,703,862	4,703,862	4,703,862
Foreign country × quarter FE	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable is the IHS-transformed number of donations from county to country in a quarter. The main variable of interest is the IHS-transformed population with ancestry from country f. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. Column 1 uses an alternative construction of the instrument that calculates the pull factor based only on European emigrants; Column 2 uses an alternative construction of the instrument that excludes countries with correlated migrant flows; Column 3 uses an alternative construction of the instrument that excludes counties with correlated migrant flows. All specifications control for logged county-country distance and latitude difference. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Table A6: Effect of ancestral presence on donations: sensitivity to including principal components of interactions as instruments

	(1)	(2)	(3)	(4)	(5)	(6)
	IHS(# donations)				Donations (dummy)	IHS($ donations)
Panel A: IV, including principal components						
IHS(Ancestry)	0.139	0.132	0.132	0.107	0.047	0.329
	(0.028)	(0.032)	(0.033)	(0.043)	(0.021)	(0.136)
First-stage F-statistic	417.1	404.2	393.6	330.6	330.6	337.8
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Panel B: IV, excluding principal components						
IHS(Ancestry)	0.137	0.130	0.130	0.114	0.052	0.354
	(0.025)	(0.028)	(0.029)	(0.044)	(0.023)	(0.138)
First-stage F-statistic	466.8	364.3	375.8	327.3	327.3	325.7
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	4,703,862	4,700,864	4,700,864	4,703,862	4,703,862	3,972,708
Foreign country × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	No	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	—	—	—
US state × quarter FE	No	No	Yes	—	—	—
US county × quarter FE	No	No	No	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable in Columns 1–4 is the IHS-transformed number of donations from county to country in a quarter. The dependent variable in Column 5 is a dummy for the presence of at least one donation from county to country in a quarter. The dependent variable in Column 6 is the IHS-transformed total value of donations from county to country in a quarter (available only for Charity 2). The main variable of interest is the IHS-transformed population with ancestry from country f. In both panels, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ as excluded instruments. In Panel A, we include the first five principal components of the higher-order interactions of push and pull factors as additional excluded instruments. Columns 1–3 control for log 2010 population. Columns 2–6 include logged county-country distance and latitude difference. Columns 2 and 3 include the following county-level demographic controls (as of 2000): the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Table A7: Effect of ancestral presence on donations dropping recent periods from the instrument

	IHS(# donations)					
Includes decades until:	2010	2000	1990	1980	1970	1930
IHS(Ancestry)	0.114	0.108	0.107	0.103	0.101	0.066
	(0.044)	(0.050)	(0.051)	(0.051)	(0.051)	(0.088)
First-stage F-statistic	327.3	247.1	282.0	320.4	374.6	253.8
Weak IV-robust p-value	< 0.01	< 0.01	0.14	< 0.01	< 0.01	< 0.01
Observations	4,703,862	4,703,862	4,703,862	4,703,862	4,703,862	4,703,862
Foreign country $\times$ quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes	Yes	Yes
US county $\times$ quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable is the IHS-transformed number of donations from county to country in a quarter. The main variable of interest is the IHS-transformed population with ancestry from country f. In the first column, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ as excluded instruments. Columns 2–6 incrementally drop the last decade of the instrument; i.e., Column 2 includes $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2000}$, Column 3 includes $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,1990}$, and so on. All columns control for logged county-country distance and latitude difference as well as foreign country $\times$ quarter and domestic county $\times$ quarter fixed effects. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Table A8: Stability of estimated effect of ancestral presence on donations, varying population

	(1)	(2)	(3)	(4)
	European donors	Other continents	Other countries	No country restriction
IHS(Ancestry)	0.107	0.110	0.116	0.157
	(0.043)	(0.045)	(0.048)	(0.077)
First-stage F-statistic	330.6	330.6	330.6	330.6
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.019	0.021	0.023	0.024
Dep. var. s.d.	0.182	0.192	0.200	0.209
Observations	4,703,862	4,703,862	4,703,862	4,703,862
Foreign country × quarter FE	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Column 1 limits the sample to European donors; Column 2 additionally limits the sample to donors whose name is matched to a country on a different continent than the receiving country; Column 3 additionally limits the sample to donors whose name is matched to a country different than the receiving country; Column 4 presents the results for all donors with no limitation of the sample. The dependent variable is the IHS-transformed number of donations from county to country in a quarter. The main variable of interest is the IHS-transformed population with ancestry from country f. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for logged county-country distance and latitude difference. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Table A9: Effect of ancestral presence on donations, excluding different countries and Census regions

	IHS(# donations)			
Panel A: Excluding different countries				
Countries excluded:	Muslim-majority	Arab	Latin American	non-Arab African
IHS(Ancestry)	0.077	0.082	0.069	0.263
	(0.040)	(0.036)	(0.036)	(0.149)
First-stage F-statistic	397.7	349.7	722.1	157.6
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.026	0.022	0.018	0.019
Dep. var. s.d.	0.218	0.198	0.176	0.183
Observations	2,479,020	3,605,562	4,195,506	3,025,032
Panel B: Excluding different census regions				
Census region excluded	Northeast	South	Midwest	West
IHS(Ancestry)	0.113	0.111	0.110	0.090
	(0.042)	(0.046)	(0.047)	(0.039)
First-stage F-statistic	397.7	349.7	722.1	157.6
Weak IV-robust p-value	0.59	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.015	0.025	0.024	0.016
Dep. var. s.d.	0.162	0.209	0.204	0.165
Observations	4,378,579	2,570,785	3,122,417	4,039,805
Foreign country × quarter FE	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes	Yes

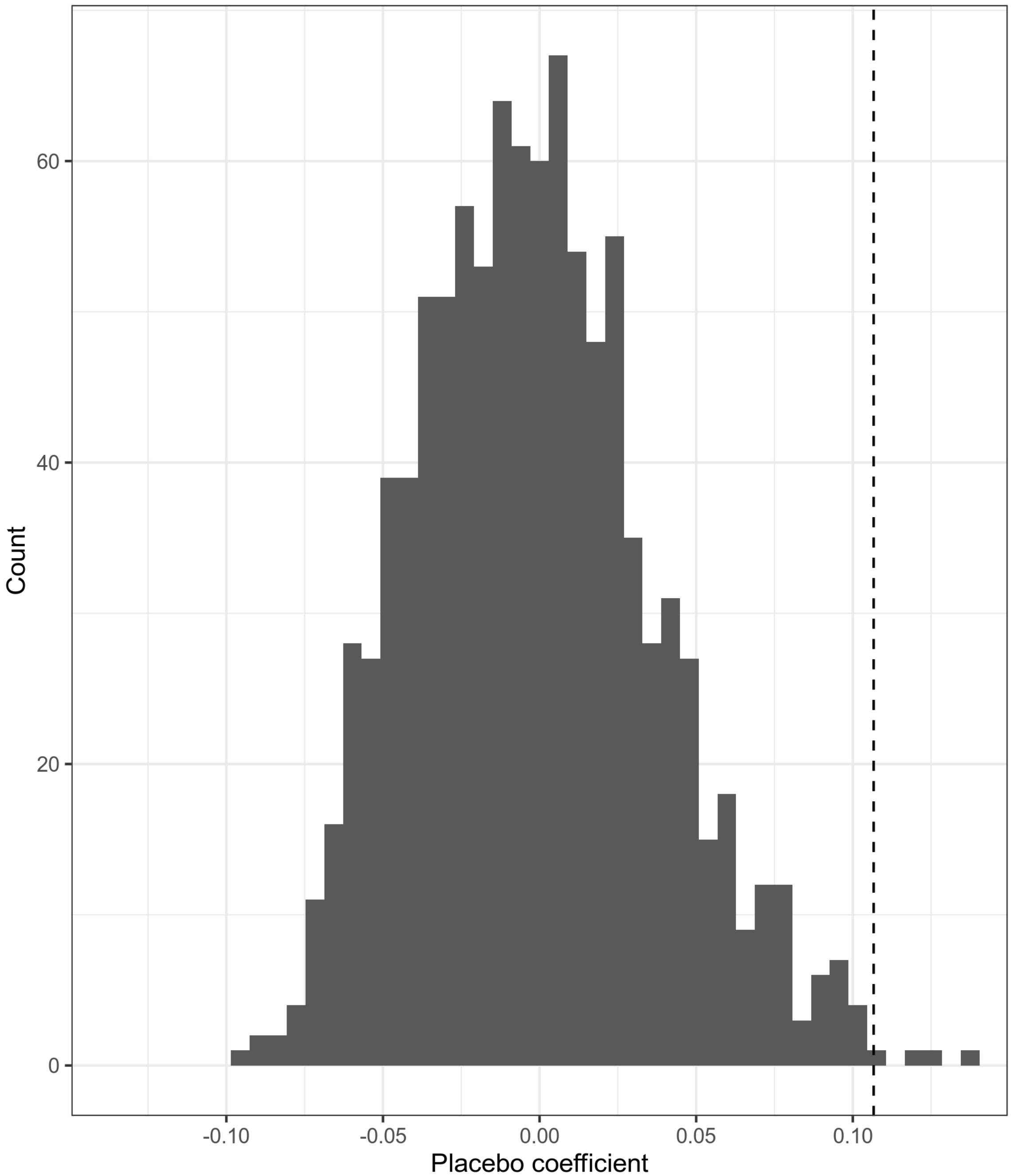
Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The main variable of interest is the IHS-transformed population with ancestry from country f in county d. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. In Panel A, Column 1 excludes Muslim-majority countries; Column 2 excludes Arab League countries; Column 3 excludes Latin American countries; and Column 4 excludes African countries which are not members of the Arab League. In Panel B, Column 1 excludes domestic counties in the Northeast; Column 2 excludes domestic counties in the South; Column 3 excludes domestic counties in the Midwest; and Column 4 excludes domestic counties in the West. All specifications control for logged county-country distance and latitude difference. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Table A10: Effect of ancestral presence on donations, different choices of clustering

	(1) All countries	(2) Arab countries (pooled)
	IHS(# donations)	
IHS(Ancestry)	0.107	0.400
Robust SE	(0.004)	(0.017)
Clustering: Foreign country	(0.044)	—
Clustering: Domestic county	(0.009)	(0.059)
Clustering: Domestic state	(0.012)	(0.083)
2-way clustering: Country/county	(0.043)	—
2-way clustering: Country/state	(0.042)	—
Dep. var. mean	0.019	0.048
Dep. var. sd	0.182	0.296
Observations	4,703,862	150,096
Distance controls	Yes	Yes
Foreign country × quarter FE	Yes	No
US county × quarter FE	Yes	No
Demographic controls	—	Yes
US state × quarter FE	—	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. We present standard errors associated with different choices of clustering. In Column 2, only donations to Arab League countries are included. In Column 1, the dependent variable is the IHS-transformed number of donations from county to country in a quarter. In Column 2, the dependent variable is the IHS-transformed number of donations from the county to Arab League countries in a quarter. The main variable of interest in Column 1 is the IHS-transformed population with ancestry from country f, while it is the IHS-transformed population with ancestry from Arab countries in Column 2. In both columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\dots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for logged county-country distance and latitude difference. Column 2 additionally includes the following county-level demographic controls (as of 2000): the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, log income, and log 2010 population.

Appendix Figure A5: Effect of ancestral presence on donations, permutation test



Notes: Figure A5 presents the results of a permutation test in which we permute ancestry and the excluded instruments, such that our regression estimates an average of the effect of the presence of one ancestral group on donations toward another country. The dotted line is placed at the true coefficient estimate. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. We control for logged county-country distance and latitude difference as well as foreign country $\times$ quarter and domestic county $\times$ quarter fixed effects.

Appendix Table A11: Effect of ancestral presence on donations, percent functional form

	(1)	(2)	(3)	(4)
		# of donations, per capita		
Percent country ancestry	0.016	0.017	0.018	0.018
	(0.009)	(0.007)	(0.006)	(0.007)
First-stage F-statistic	240.4	246.7	274.7	281.4
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.020	0.020	0.020	0.020
Dep. var. sd	0.472	0.472	0.472	0.472
Observations	4,703,862	4,700,864	4,700,864	4,703,862
Foreign country × quarter FE	Yes	Yes	Yes	Yes
Distance controls	No	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	—
US state × quarter FE	No	No	Yes	—
US county × quarter FE	No	No	No	Yes

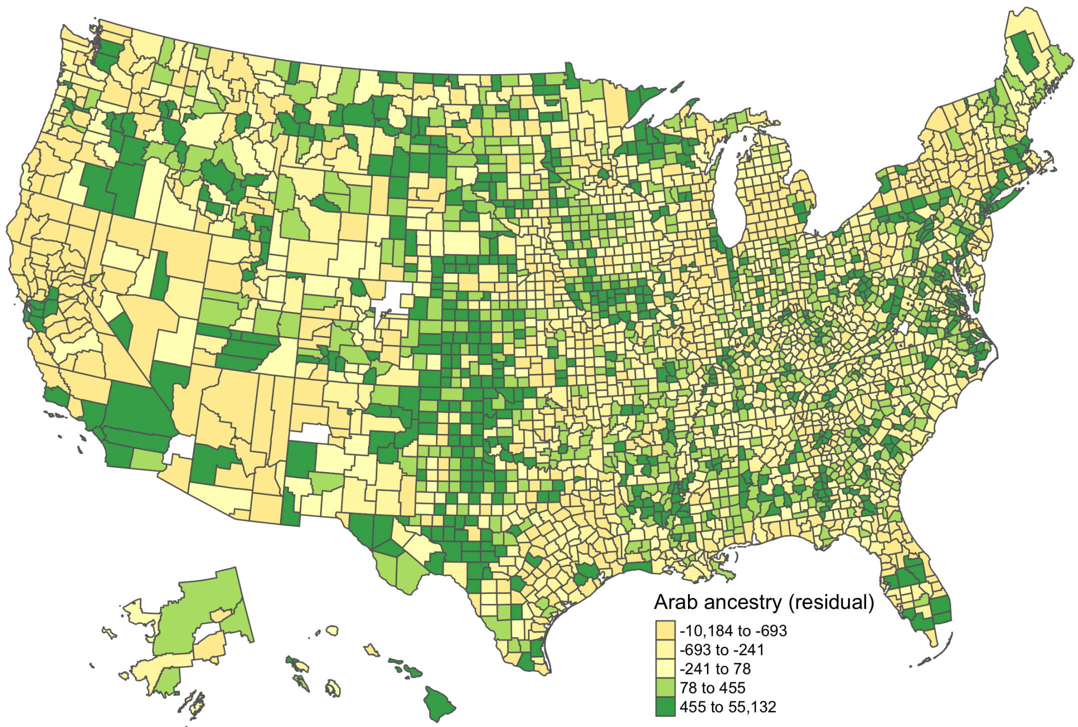
Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable is the number of donations per capita from county to country in a given quarter. The main variable of interest is the percentage of the population with ancestry from country f. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. Columns 1–3 control for log 2010 population. Columns 2–4 include logged county-country distance and latitude difference. Columns 2 and 3 include the following county-level demographic controls (as of 2000): the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Table A12: Effect of ancestral presence on donations by ancestry: aggregating to continents

	(1)	(2)	(3)	(4)
		IHS(# donations)		
Panel A: Effect of continent ancestry				
IHS(Ancestry), Continent exc. country	0.053	0.116	0.102	0.105
	(0.015)	(0.026)	(0.027)	(0.026)
First-stage F-statistic	4992.1	3685.9	3051.2	2727.1
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01
Panel B: Effect of continent and country ancestry				
IHS(Ancestry)	0.056	0.067	0.061	0.060
	(0.021)	(0.013)	(0.013)	(0.013)
IHS(Ancestry), Continent exc. country	-0.007	0.015	0.009	0.011
	(0.024)	(0.025)	(0.023)	(0.023)
F-stat IHS(Ancestry)	48,254.71	36,514.34	1,375.46	180.37
F-stat IHS(Ancestry), Continent exc. country	64,810.73	427,364.19	36,843.70	8,445.44
Observations	4,703,862	4,703,862	4,700,864	4,700,864
Continent × quarter FE	No	Yes	Yes	Yes
Distance controls	No	No	Yes	Yes
Demographic controls	No	No	Yes	Yes
US state × quarter FE	No	No	No	Yes

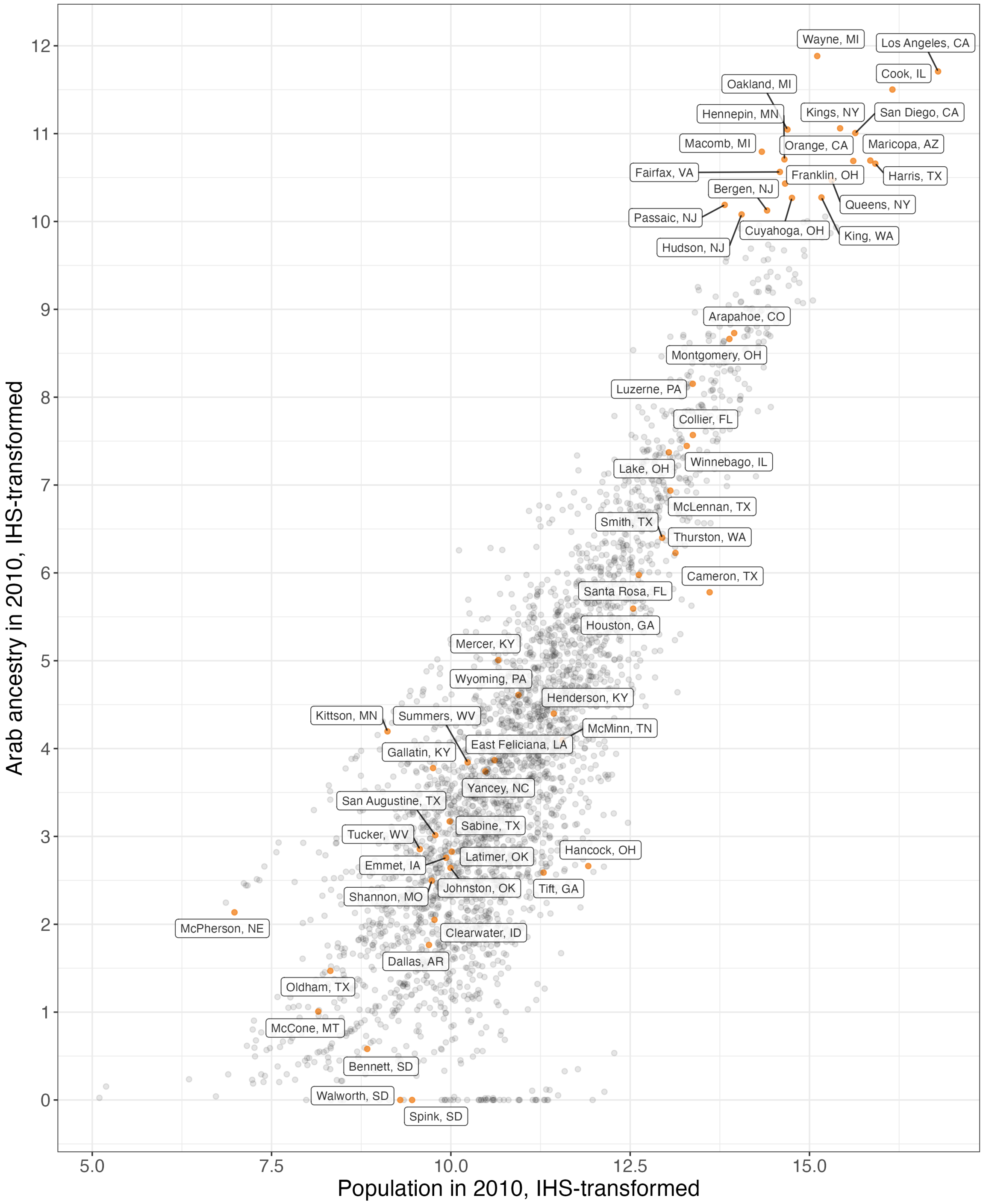
Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable is the IHS-transformed number of donations from county to country in a quarter. The main variables of interest are the IHS-transformed population with ancestry from country f and the IHS-transformed population with ancestry from continent c, excluding country f. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. To instrument the 2010 IHS-transformed population with ancestry aggregated to the continent level c excluding foreign country f, we modify the push factor, $I^s_{f,-r(d)}$, in Equation (2) as the total number of migrants arriving from continent c in period s, excluding those from country f and those who settle in d's region, i.e. $I^s_{c(-f),-r(d)}$. Columns 1–4 control for log 2010 population. Columns 3 and 4 include logged county-country distance and latitude difference and the following county-level demographic controls (as of 2000): the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. The table reports Sanderson-Windmeijer conditional first-stage F-statistics. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

Appendix Figure A6: Residualized predicted values of Arab-Muslim ancestry



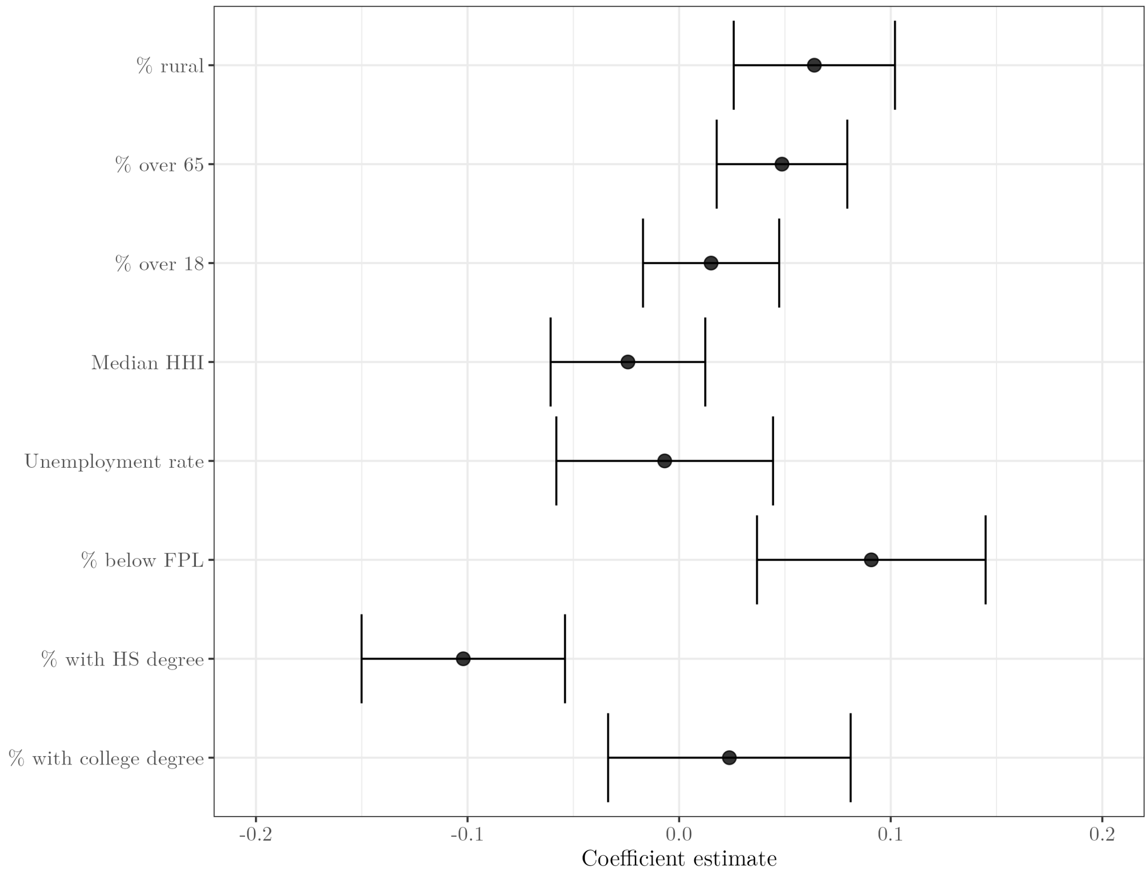
Notes: Figure A6 maps the residualized values of predicted Arab-Muslim ancestry, where we use $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as instruments and residualize by state fixed effects, log population, and the following county-level demographic controls: the shares of the population with a high school education, with a college education, and population density as of 2000.

Appendix Figure A7: Arab-ancestry population across counties



Notes: Figure A7 plots the IHS-transformed 2010 population of each US county against the IHS-transformed 2010 Arab-ancestry population of that county.

Appendix Figure A8: Balance test of Arab-Muslim instruments



Notes: Figure A8 presents coefficient estimates from regressions of a number of demographic characteristics (scaled to take mean zero and standard deviation one) on the predicted values of IHS-transformed Arab-Muslim ancestry (scaled similarly). We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All regressions control for log 2010 population and include state fixed effects. Error bars indicate 95% confidence intervals. Standard errors are clustered at the state level.

Appendix Table A13: Effect of presence of Arab ancestry on auxiliary measures of prejudice and social norms

	(1) Standards	(2) Disapproval	(3) Beliefs (1)	(4) Beliefs (2)
Panel A: IV				
IHS(Arab ancestry)	0.045	0.009	0.084	0.090
	(0.026)	(0.030)	(0.037)	(0.042)
AP F-statistic	9.861	9.835	9.851	9.865
Weak IV-robust p-value	< 0.01	0.95	< 0.01	< 0.01
Panel B: OLS				
IHS(Arab ancestry)	0.024	0.016	0.035	0.033
	(0.005)	(0.005)	(0.007)	(0.007)
Observations	106,281	106,205	106,557	106,665
State FE	Yes	Yes	Yes	Yes
Individual-level demographics	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variables represent agreement with different statements about prejudice and social norms; all outcomes are scaled to mean zero and standard deviation one such that higher values indicate less prejudice. "Standards" refers to the statement "Because of today's standards I try to appear nonprejudiced toward Arab Muslims" (Column 1); "Disapproval" refers to the statement "I attempt to appear nonprejudiced toward Arab Muslims in order to avoid disapproval from others" (Column 2); "Beliefs (1)" refers to the statement "I am personally motivated by my beliefs to be nonprejudiced toward Arab Muslims" (Column 3); and "Beliefs (2)" refers to the statement "Because of my personal values, I believe that using stereotypes about Arab Muslims is wrong" (Column 4). Only respondents who self-reported their reason for taking the Project Implicit test as "Assigned for work," "Assigned for school," or "Assigned for discussion group" are included. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. In Panel A, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age $\times$ male. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Appendix Table A14: Effect of presence of Arab ancestry on attitudes toward Arab-Muslims, forced and unforced respondents

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Panel A:	*Score on Arab-Muslim IAT (std., higher score = less prejudiced)*						
IHS(Arab ancestry)	0.012	0.061	0.056	0.062	0.061	0.044	0.054
	(0.005)	(0.017)	(0.019)	(0.022)	(0.025)	(0.017)	(0.021)
IHS(non-Euro ancestry)					−0.023		
					(0.017)		
Avg. race IAT score						0.378	
						(0.049)	
2012 Rep. vote share							−0.126
							(0.044)
AP F-statistic	—	14.15	11.01	6.605	6.780	6.779	6.127
Weak IV-robust p-value	—	0.44	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	226,191	226,191	223,567	223,567	223,567	223,567	223,567
Panel B:	*Warmth toward Arab-Muslims (std., higher score = more favorable)*						
IHS(Arab ancestry)	0.037	0.132	0.128	0.108	0.110	0.078	0.090
	(0.007)	(0.023)	(0.030)	(0.031)	(0.032)	(0.026)	(0.030)
IHS(non-Euro ancestry)					−0.045		
					(0.021)		
Avg. race IAT score						0.608	
						(0.061)	
2012 Rep. vote share							−0.260
							(0.059)
AP F-statistic	—	14.16	11.06	6.548	6.740	6.735	6.067
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	226,684	226,684	224,102	224,102	224,102	224,101	224,102
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Individual-level demographics	No	No	Yes	Yes	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variable in Panel A is the score on the Arab-Muslim IAT (from Project Implicit); the dependent variable in Panel B is the stated warmth toward Arab-Muslims (also from Project Implicit). Both measures are scaled to take mean zero and standard deviation one. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age × male. County-level demographic controls are as of 2000 and include the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are clustered at the county level.

Appendix Table A15: Effect of presence of Arab ancestry on attitudes toward Arab-Muslims and political preferences, representative sample

	(1) Favorability	(2) Trump	(3) Muslim Ban
Panel A: IV			
IHS(Arab ancestry)	0.112	−0.059	−0.077
	(0.029)	(0.018)	(0.018)
AP F-statistic	10.14	10.42	10.45
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01
Panel B: OLS			
IHS(Arab ancestry)	0.034	−0.015	−0.014
	(0.005)	(0.003)	(0.003)
Observations	188,411	171,150	58,466
State FE	Yes	Yes	Yes
Individual-level demographics	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variable in Column 1 is the stated favorability toward Muslims; the dependent variable in Column 2 is self-reported Trump votership; and the dependent variable in Column 3 is stated support for the Muslim Ban. The data is from Nationscape. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. In Panel A, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age $\times$ male. Standard errors are given in parentheses. Standard errors are clustered at county level.

Appendix Table A16: Effect of presence of Arab ancestry on attitudes toward different groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Arab-Muslim		Asian		Black	
	IAT	Warmth	IAT	Warmth	IAT	Warmth
Panel A:	*Unweighted*					
IHS(Arab ancestry)	0.073	0.136	0.034	0.037	0.018	0.030
	(0.026)	(0.033)	(0.028)	(0.028)	(0.017)	(0.015)
AP F-statistic	9.808	9.852	10.57	11.58	10.05	10.09
Weak IV-robust p-value	< 0.01	< 0.01	0.09	< 0.01	< 0.01	< 0.01
Observations	105,968	105,856	74,152	34,605	1,118,084	1,117,484
Panel B:	*Reweighted to match Arab-Muslim test-takers on observables*					
IHS(Arab ancestry)	0.073	0.136	0.032	0.033	0.019	0.033
	(0.026)	(0.033)	(0.029)	(0.030)	(0.017)	(0.014)
AP F-statistic	9.808	9.852	10.62	11.66	9.979	10.03
Weak IV-robust p-value	< 0.01	< 0.01	0.02	0.05	< 0.01	< 0.01
Observations	105,968	105,856	69,218	32,324	1,032,246	1,032,213
Panel C:	*Limiting to counties in Arab-Muslim data, unweighted*					
IHS(Arab ancestry)	0.073	0.136	0.033	0.037	0.016	0.029
	(0.026)	(0.033)	(0.028)	(0.027)	(0.016)	(0.014)
AP F-statistic	9.808	9.852	10.78	12.10	10.33	10.37
Weak IV-robust p-value	< 0.01	< 0.01	0.09	< 0.01	< 0.01	< 0.01
Observations	105,968	105,856	73,948	34,503	1,113,452	1,112,868
Panel D:	*Limiting to counties in Arab-Muslim data, reweighted*					
IHS(Arab ancestry)	0.073	0.136	0.031	0.034	0.017	0.033
	(0.026)	(0.033)	(0.028)	(0.030)	(0.016)	(0.014)
AP F-statistic	9.808	9.852	10.83	12.22	10.25	10.30
Weak IV-robust p-value	< 0.01	< 0.01	0.02	0.05	< 0.01	< 0.01
Observations	105,968	105,856	69,022	32,225	1,028,068	1,028,048
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual demographics	Yes	Yes	Yes	Yes	Yes	Yes

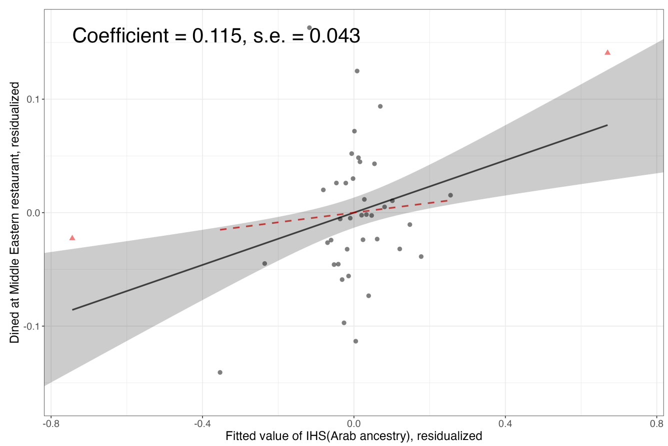
Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variables in Columns 1, 3, and 5 are IAT scores toward Arab-Muslims, Asians, and Black Americans, respectively. The dependent variables in Columns 2, 4, and 6 are stated warmth toward Arab-Muslims, Asians, and Black Americans, respectively. Panel A weights all observations equally. We conduct a t-test to test the null that effects on attitudes toward Asians and Blacks are equal to the effects on Arab-Muslims. The resulting p-value is 0.31 for the comparison of Columns 1 and 3; 0.076 for Columns 1 and 5; 0.02 for Columns 2 and 4; and 0.003 for Columns 2 and 6. Columns 3–6 of Panel B reweight observations to match the sample of Columns 1–2 on age, gender, education, and Hispanic status; Columns 3–6 of Panel C limit the sample to counties with at least one Arab-Muslim IAT; and Columns 3–6 of Panel D first limit the sample to counties with at least one Arab-Muslim IAT, then reweight observations to match the sample of Columns 1–2 on age, gender, education, and Hispanic status. All measures are scaled to take mean zero and standard deviation one. Only respondents who self-reported their reason for taking the Project Implicit test as "Assigned for work," "Assigned for school," or "Assigned for discussion group" are included. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Appendix Table A17: Effect of presence of Arab ancestry on political preferences, individual Romney control

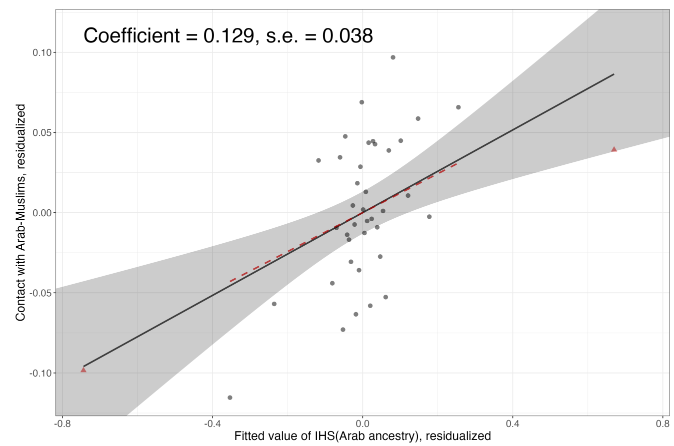
	(1) OLS	(2) IV	(3) IV	(4) IV
	Voted for Trump in 2016			
IHS(Arab ancestry)	−0.012	−0.052	−0.061	−0.035
	(0.003)	(0.014)	(0.021)	(0.019)
Voted for Romney in 2012	0.739	0.736	0.725	0.724
	(0.005)	(0.005)	(0.005)	(0.005)
AP F-statistic	—	17.40	9.368	5.302
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01
Observations	32,529	32,529	32,529	32,529
State FE	No	No	Yes	Yes
Individual-level demographics	No	No	Yes	Yes
County-level demographics	No	No	No	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variable is self-reported Trump votership. The data is from the CCES. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age × male. County-level demographic controls are as of 2000 and include the shares of the population above 18, above 65, with a high school education, with a college education, below the poverty line, and living in a rural area; population density, the unemployment rate, and log income. Standard errors are given in parentheses. Standard errors are clustered at the county level.

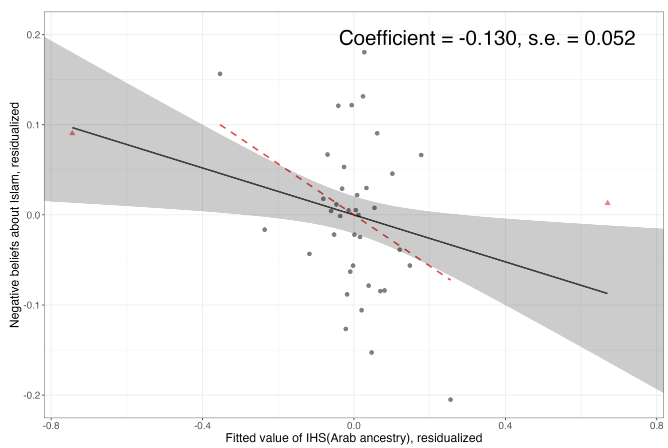
Appendix Figure A9: Binned scatter plots of contact and knowledge



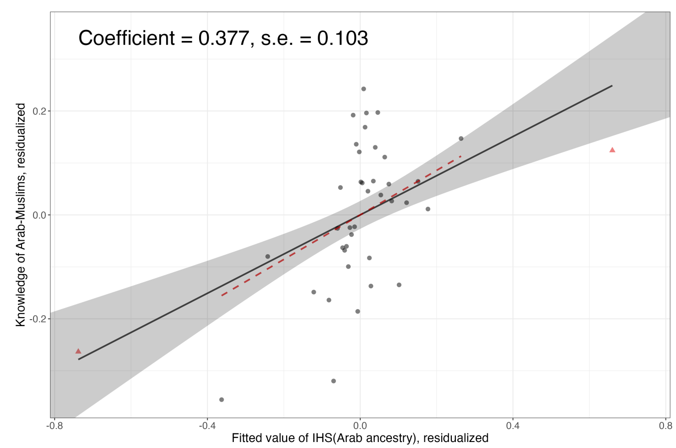
(a) Visited Middle Eastern restaurant



(b) Contact with Arab-Muslims



(c) Negative beliefs about Islam



(d) Knowledge about Islam

Notes: Figure A9 presents binned scatter plots displaying the relationship between the fitted values of IHS(Arab ancestry) and four outcomes: an indicator taking value one if the respondent reports ever visiting a Middle Eastern restaurant, an indicator taking value one if the respondent personally knows an Arab-Muslim friend, neighbor, or colleague; a measure of the respondent's negative beliefs about Islam; and an index measuring respondents' knowledge of Islam. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. We residualize outcomes and instruments by the controls used in Columns 1–5 of Table 8. Red triangles are used to indicate the top and bottom 2.5% of the data by fitted values; the red dotted line indicates the regression fit after dropping observations in the top and bottom 2.5% of fitted values. Standard errors are clustered at the county level. 95% confidence intervals are reported.

Appendix Table A18: Robustness across different definitions of Muslim ancestry

	(1) IAT	(2) Warmth	(3) Muslim Ban	(4) Trump vote	(5) Contact	(6) Knowledge
Panel A: Arab-Muslim ancestry						
IHS(Ancestry)	0.073	0.136	−0.076	−0.073	0.129	0.377
	(0.026)	(0.033)	(0.024)	(0.027)	(0.038)	(0.103)
AP F-statistic	9.808	9.852	9.516	9.643	8.464	8.053
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Panel B: Ancestry from Muslim Ban countries						
IHS(Ancestry)	0.060	0.124	−0.088	−0.069	0.096	0.132
	(0.027)	(0.034)	(0.030)	(0.037)	(0.035)	(0.080)
AP F-statistic	3.779	3.766	7.460	6.982	17.59	17.46
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	0.04	< 0.01
Panel C: Ancestry from Muslim-majority countries						
IHS(Ancestry)	0.075	0.131	−0.084	−0.072	0.131	0.326
	(0.028)	(0.038)	(0.029)	(0.034)	(0.029)	(0.064)
AP F-statistic	6.510	6.576	5.560	5.781	5.829	5.988
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	0.02	0.03	< 0.01
Dep. var. mean	0.017	0.034	0.530	0.464	0.396	0.000
Dep. var. sd	0.989	0.996	0.499	0.499	0.489	1.000
Observations	105,968	105,856	56,837	97,576	5,020	4,729
State FE	Yes	Yes	Yes	Yes	No	No
Individual demographics	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variables in Columns 1–6 are scores on the Arab-Muslim IAT, stated warmth toward Arab-Muslims, stated support for the Muslim Ban, self-reported Trump voting, an indicator for whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor, and a normalized index of knowledge about Arab-Muslims. The dependent variables in Columns 1–2 are drawn from Project Implicit; the dependent variables in Columns 3–4 are drawn from the CCES; and the dependent variables in Columns 5–6 are drawn from our survey. In Columns 1 and 2, only respondents who self-reported their reason for taking the Project Implicit test as "Assigned for work," "Assigned for school," or "Assigned for discussion group" are included. The main variable of interest in Panel A is the 2010 IHS-transformed population with ancestry from Arab League countries; the main variable of interest in Panel B is the 2010 IHS-transformed population with ancestry from countries affected by Executive Order 13769 ("Muslim ban"); and the main variable of interest in Panel C is the 2010 IHS-transformed population with ancestry from Muslim-majority countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age $\times$ male. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Appendix Table A19: Effect of presence of Arab ancestry, percent functional form

	(1) Donations	(2) IAT	(3) Warmth	(4) Muslim Ban	(5) Trump vote	(6) Contact	(7) Knowledge
Percent Arab ancestry	0.122	0.072	0.177	−0.129	−0.151	0.260	0.592
	(0.058)	(0.026)	(0.053)	(0.049)	(0.051)	(0.104)	(0.226)
AP F-statistic	492.21	10.75	10.77	11.01	10.35	13.16	12.39
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.044	0.017	0.034	0.530	0.464	0.396	0.000
Dep. var. sd	0.533	0.989	0.996	0.499	0.499	0.489	1.000
Observations	150,096	105,968	105,856	56,837	97,576	5,020	4,729
State FE	–	Yes	Yes	Yes	Yes	No	No
State × quarter FE	Yes	No	No	No	No	No	No
Distance controls	Yes	No	No	No	No	No	No
County-level demographics	Yes	No	No	No	No	No	No
Individual demographics	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-quarter (Column 1) and individual (Columns 2–7) levels. The dependent variable in Column 1 is the number of donations per capita from the county to Arab League countries in a quarter. The dependent variables in Columns 2–7 are scores on the Arab-Muslim IAT, stated warmth toward Arab-Muslims, stated support for the Muslim Ban, self-reported Trump voting, an indicator for whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor, and a normalized index of knowledge about Arab-Muslims. The dependent variables in Columns 2–3 are drawn from Project Implicit; the dependent variables in Columns 4–5 are drawn from the CCES; and the dependent variables in Columns 6–7 are drawn from our survey. Only respondents who self-reported their reason for taking the Project Implicit test as "Assigned for work," "Assigned for school," or "Assigned for discussion group" are included. The main variable of interest is the percentage of the population with ancestry from Arab countries. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age × male. Standard errors are given in parentheses. Standard errors are clustered at the county level.

Appendix Table A20: Effect of presence of Arab ancestry on white flight

	(1)	(2)
Panel A:	*Selective white flight index*	
IHS(Ancestry)	0.026	0.017
	(0.003)	(0.002)
First-stage F-statistic	63.04	48.30
Weak IV-robust p-value	< 0.01	< 0.01
Dep. var. mean	0.062	0.062
Dep. var. s.d.	0.029	0.029
Observations	9,333	9,333
Panel B:	*Selective white flight index, by subgroup*	
IHS(Ancestry) × Married	−0.002	−0.002
	(0.0003)	(0.0003)
IHS(Ancestry) × Female	0.0004	0.0004
	(0.0001)	(0.0001)
IHS(Ancestry) × College	0.001	0.001
	(0.0005)	(0.0005)
IHS(Ancestry) × Age	0.001	0.001
	(0.0003)	(0.0003)
IHS(Ancestry) × Income	0.002	0.002
	(0.0004)	(0.0004)
Year FE	Yes	Yes
US state FE	No	Yes

Notes: The table presents coefficient estimates from regressions at the country-county-decade level. The dependent variable is the selective white flight index, defined in Section 3.4; in Panel A, the index is computed from the full sample, whereas in Panel B, two separate indices are computed for each dimension of heterogeneity (one for each subgroup). The endogenous variable in Panel A is the IHS-transformed population with ancestry from Arab League countries. Each row of Panel B presents a separate regression of the selective white flight index for a given subgroup on an indicator for the subgroup, IHS-transformed population with ancestry from Arab League countries, and the interaction of the indicator and IHS-transformed ancestral population. The excluded instruments in Panel A are $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,1980}$ and the first five principal components of the higher-order interactions; in Panel B, we additionally include as instruments the interaction of each instrument with the subgroup indicator. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels.

B Data Appendix

B.1 Details on the construction of migration and ethnicity data

County residence is defined at the level of historic counties, and at the level of historic county groups or PUMAs starting in 1970. Whenever necessary, we use contemporaneous population weights to transition data from the historic county group or PUMA to historic county, and then area weights to transition data from the historic county to 1990 counties. Stated ancestry often corresponds to foreign countries in their 1990 borders (e.g. "Syrian"), though not always. In cases with ambiguous correspondence (e.g. "Kurdish"), we construct transition matrices that map into 1990 national boundaries using approximate population weights when feasible and approximate area weights otherwise.

Calculation of post-1880 flow of immigrants

For each census wave after 1880, we count the number of individuals in each historic US domestic county d who were born in historic foreign country f (as identified by birthplace variable "bpld" in the raw data) that had immigrated to the United States since the last census wave that contains the immigration variable (not always 10 years earlier). Then we transform these data

- from the non-1990 foreign-country ("bpld") level to the 1990 foreign-country level using bpld-to-country transition matrices.
- from the US-county group/puma level to the US-county level using group/puma-to-county transition matrices.
- from the non-1990 US-county level to the 1990 US-county level using county-to-county transition matrices.
- from the post-1990 US-county level to the 1990 US county level. Based on the information from `https://www.census.gov/programs-surveys/geography/technical-documentation/county-changes.html`, a new county is either created from part of ONE 1990 county or assigned a new FIPS code after 1990, so we manually change that county's FIPS code to what it was in 1990. A few counties' boundaries have been changed after 1990 but that only involved a tiny change in population, so we ignore these differences.

Calculation of pre-1880 stock of immigrants

The initial 1880 Census did not report the immigration date. Thus, for the year 1880, we calculate for each historic US county d the number of individuals who were born in a historic foreign country f

(no matter when they immigrated). We add to those calculations the number of individuals in county d who were born in the United States, but whose parents were born in historic foreign country f. (If the parents were born in different countries, we count the person as half a person from the mother's place of birth, and half a person from the father's place of birth). Then we transform these data

- from the pre-1880 foreign-country ("bpld") level to the 1990 foreign-country level using the pre-1880 country-to-country transition matrix.
- from the pre-1880 US-county level to the 1990 US-county level using the pre-1880 county-to-county transition matrix.

Calculation of stock of ancestry (1980, 1990, 2000, and 2010)

For the years 1980, 1990, 2000, and 2010, we calculate for each US county group the number of individuals who state as primary ancestry ("ancestr1" variable) some nationality/area. We transform the data

- from the ancestry-answer ("ancestr1") level to the 1990 foreign-country level using ancestry-to-country transition matrices.
- from the US-county group/puma level to the US county-level using group/puma-to-county transition matrices.
- from the non-1990 US-county level to the 1990 US-county level using county-to-county transition matrices.
- from the post-1990 US-county to the 1990 US-county level. Based on the information from `https://www.census.gov/programs-surveys/geography/technical-documentation/county-changes.html`, a new county is either created from part of ONE 1990 county or assigned a new FIPS code after 1990, so we manually change that county's FIPS code to what it was in 1990. A few counties' boundaries have been changed after 1990 but that only involved a tiny change in population, so we ignore the difference.

B.2 Details on other demographic data

We source county-level population and population density from IPUMS. Our data on average age, racial composition, average household income, and educational attainment is drawn from the 2018 round of the American Community Survey. Our county-level measures of poverty is provided by the

Appendix Table B1: Description of each IPUMS wave

Wave	Description
1880	We use the 10% sample with oversamples; the sample is weighted, so we use the provided person weights to get to a representative sample; we use the region identifiers statefip and county.
1900	We use the 5% sample; the sample is weighted, so we use the provided person weights to get to a representative sample; we use the region identifiers statefip and county.
1910	We use the 1% sample; the sample is unweighted; we use the region identifiers statefip and county.
1920	We use the 1% sample; the sample is weighted, so we use the provided person weights to get to a representative sample; we use the region identifiers statefip and county.
1930	We use the 5% sample; the sample is weighted, so we use the provided person weights to get to a representative sample; we use the region identifiers statefip and county.
1970	We use the 1% Form 1 Metro sample; the sample is unweighted; we use the region identifiers statefip and cntygp97 (county group 1970); note that only four states can be completely identified because metropolitan areas that straddle state boundaries are not assigned to states; identifies every metropolitan area of 250,000 or more.
1980	We use the 5% State sample; the sample is unweighted; we use the region identifiers statefip and cntygp98 (county group 1980); the sample identifies all states, larger metropolitan areas, and most counties over 100,000 population.
1990	We use the 5% State sample; the sample is weighted, so we use the provided person weights to get to a representative sample; we use the region identifiers statefip and puma; the sample identifies all states, and within states, most counties or parts of counties with 100,000 or more population.
2000	We use the 5% Census sample; the sample is weighted, so we use the provided person weights to get to a representative sample; we use region identifiers statefip and puma; the sample identifies all states, and within states, most counties or parts of counties with 100,000 or more population.
2010	We use the American Community Service (ACS) 5-Year sample; the sample is weighted, so we use the provided person weights to get to a representative sample; we use region identifiers statefip and puma, which contain at least 100,000 persons; the 2006-2010 data contains all households and persons from the 1% ACS samples for 2006, 2007, 2008, 2009 and 2010, identifiable by year.

US Census Bureau under the 2018 Small Area Income and Poverty Estimates (SAIPE) programs. Our data on unemployment is from the US Bureau of Labor Statistics' 2019 Local Area Unemployment Statistics (LAUS).

We compute the distance between foreign country f and a US county d, $Distance_{f,d}$, as the great circle distance between the county and country centroids, measured in kilometers. The latitude difference between a foreign country f and a US county d, $LatitudeDifference_{f,d}$, is the absolute difference between the latitudes of the two, measured in degrees.[29] References to distance as a control include both distance and latitude difference.

[29] Geo-coordinates for counties and countries are sourced from www.geonames.org and www.cepii.fr respectively, with a county's latitude and longitude as the average of that of all postal codes within the county, and a country's latitude and longitude as that of the largest city within the country.

C NamSor Classification

C.1 Validation

We are not aware of any published attempts to validate NamSor's algorithm matching names to countries of origin, though research examining the accuracy of NamSor's gender-matching algorithm (Van Buskirk et al., 2022) and NamSor's Census designation (Asian, Black, non-Latino, Hispanic Latino, white non-Latino) algorithm (Krishnan et al., 2021) has found these algorithms to be highly accurate.

We conduct an additional validation using a random 250,000 person sample from the North Carolina Voter Registration Data[30], which contains registrants' first and last names alongside self-reported ethnicity (Asian, African American, American Indian or Alaskan Native, Two or More Races, Other, Native Hawaiian or Pacific Islander, Undesignated, and White). Given that we use this classification exercise to exclude donors with ancestry from non-European countries, we are primarily concerned with classification errors of the type: Reports Asian/Native Hawaiian or Pacific Islander/Other, Classified as European. We find that this error occurs for fewer than one percent (2,322 of 250,000) of cases, suggesting that any bias induced by erroneously including these donors is negligible.

C.2 Data Privacy

Privacy for individual microdata was maintained at all stages of the data process, with no organization receiving more information than necessary. A 3-way Non Disclosure Agreement was signed by relevant parties to ensure that the following data privacy procedure was adhered to:

1. The charitable organization sends the research team the donation data, stripped of identifying information including names and addresses, with each donation containing a unique anonymized identifier (ID)

2. The charitable organization sends the third party NamSor a list containing *only* the ID of the donations and the name associated with each donation

3. Based on these names, NamSor determines the most likely origin country of the name

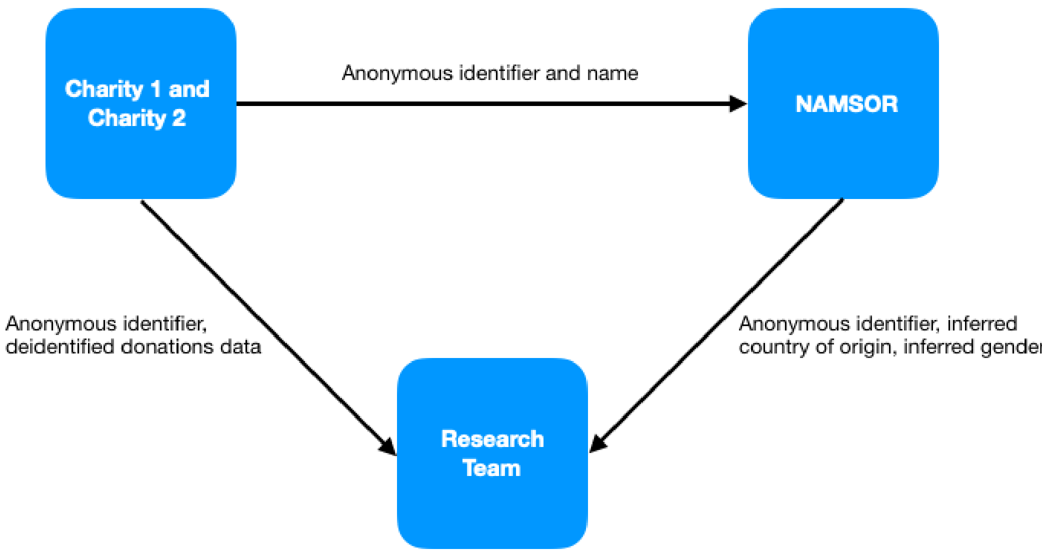
4. NamSor sends the research team a list containing *only* the ID of the donations and the origin country associated with each donation

5. The research team uses the donation ID to match up the donation data from the charitable organization and the origin country data from NamSor

[30] Sood, Gaurav, 2020, "NC Voter Registration Data", https://doi.org/10.7910/DVN/NEFUBN, Harvard Dataverse, V1

A summary of the process is displayed below in Appendix Figure C1.

In this way, the organizations only receive the information that they need, and no more. The charitable organization does not receive NamSor data regarding origin countries for donor names, NamSor does not receive any variables regarding donations except for the donor's name, and the research team does not receive any personally identifying information for any donation. Finally, data was shared using a number of secured Dropbox folders only shared with the intended recipients of the data.



Appendix Figure C1: Data Flow for Privacy

D Contact Survey Questionnaire

Demographics

Please indicate your gender.

- Male
- Female
- Other/prefer not to answer

In what year were you born?

Were you born in the US?

- Yes
- No

What was your family's gross household income in 2019 in US dollars?

Do you have any children?

- Yes
- No

How many people are in your household?

Which of the following best describes your race or ethnicity?

- African American/Black
- Asian/Asian American
- Caucasian/White
- Native American, Inuit or Aleut
- Native Hawaiian/Pacific Islander
- Other

Are you of Hispanic, Latino, or Spanish origin?

- Yes
- No

Are you of Arab or Middle Eastern origin?

- Yes
- No

Which category best describes the highest level of education you have completed?

- 12th grade or less, but no high school diploma
- Graduated high school or equivalent
- Some college, no degree
- Associate degree
- Bachelor's degree
- Post-graduate degree

Are you married or in a long-term domestic partnership?

- Yes
- No

In general, how would you describe your physical health?

- Excellent
- Very good
- Good
- Only fair
- Poor

What is your present religion, if any?

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:

1. Enter your address into https://www.whatcountyamiin.com/ to find your county name
2. Use your state name and the county name to look up the FIPS code on this page: https://www.nrcs.usda.gov/wps/portal/nrcs/detail/ma/home/?cid=nrcs143_01369

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

Politics

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

- Republican
- Democrat
- Independent

In politics, as of today, do you lean towards the Republican Party or lean towards the Democratic Party?

- The Republican Party
- The Democratic Party
- Do not lean toward either party

In politics, as of today, would you call yourself a strong Democrat or not a very strong Democrat?

- Strong
- Not very strong

In politics, as of today, would you call yourself a strong Republican or not a very strong Republican?

- Strong
- Not very strong

Who did you vote for in the 2012 Presidential election?

- Mitt Romney
- Barack Obama
- Other
- I did not vote

Who did you vote for in the 2016 Presidential election?

- Donald Trump
- Hillary Clinton
- Other
- I did not vote

Who did you vote for in the 2020 Presidential election?

- Donald Trump
- Joe Biden
- Other
- I did not vote

So far as you and your family are concerned, how worried are you about your current financial situations?

- Extremely worried
- Very worried

- Moderately worried
- A little worried
- Not at all worried

Which of the following networks do you watch at least once a week? If you watch multiple networks, please choose the one you watch most often.

- Fox News
- CNN
- MSNBC
- None of the above

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 among each group.

	Close friends and family members	Neighbors	Workplace acquaintances	Others with whom I regularly interact	Service or hospitality workers
African-Americans	☐	☐	☐	☐	☐
Arabs and/or Muslims	☐	☐	☐	☐	☐

Knowledge

We'd now like to ask you some questions about various religions.

What is Ramadan?

- Hindu festival of lights
- Jewish prayer for the dead
- An Islamic holy month
- Festival celebrating Buddha's birth

Which text is most closely associated with Hinduism?

- Tao Te Ching
- Vedas
- Quran
- Mahayana sutras

Which of the following are among the Five Pillars of Islam?

(You can select multiple options.)

- Fasting (sawm)
- Profession of faith (shahada)
- Charity to community members in need (zakat)
- Maintaining physical and mental health (sahi)
- Holy war against non-believers (jihad)
- Pilgrimage (hajj)
- Subservience of women and children to men (alnisa)

What percentage of the US population is Muslim? Please write your answer as a number, with 0 meaning that none of the US population is Muslim and 100 meaning that the entire US population is Muslim.

Restaurant

Have you ever eaten at a Middle Eastern restaurant? (For example, Iranian/Persian, Turkish, Egyptian, or Afghani restaurants)

- Yes
- No

End

Thank you for participating in our survey!

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