The Immigrant Next Door: Long-Term Contact, Generosity, and Prejudice[*]

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Abstract

We study how decades-long exposure to 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 long-term exposure to a given foreign ancestry 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 long-term exposure: (*i*) decreases both explicit and implicit prejudice against Arab-Muslims, (*ii*) reduces support for policies and political candidates hostile toward Arab-Muslims, (*iii*) leads to more personal contact with Arab-Muslim individuals, and (*iv*) increases knowledge of Arab-Muslims and Islam in general.

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1 Introduction

Many countries face growing challenges surrounding immigrant integration and native backlash. As hypothesized by Allport (1954), and as empirically demonstrated in more recent work (e.g. Lowe 2020), the effects of short-run intergroup contact on attitudes and behavior depend heavily on the nature of interaction. This raises the question of the *long-run* effects of contact: as immigrants and natives interact repeatedly over the course of decades, summing across different contexts and different economic and social circumstances, what is the aggregate effect on natives' beliefs and behavior?

In this paper, we show that long-term exposure to descendants of foreign migrants induces more positive behavior and attitudes towards that ancestral group. We combine several sources of data to construct measures of long-term exposure to, generosity towards, and prejudice against foreign-origin groups in the United States. In particular, we measure long-term *exposure* 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.[1] Turning to mechanisms, we measure *attitudes* toward a specific foreign-origin group of particular relevance to the policy debate, Arab-Muslims,[2] 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 foreign migrants increases natives' generosity towards that ancestral 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 contact with descendants of foreign migrants on natives' generosity towards foreigners over the span of decades. Our estimates for the aggregate effects of long-term contact 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

[1] We are able to restrict our sample to donors with European-ancestry names, better capturing the behavior of white Americans. We follow a strict protocol to protect donor anonymity; importantly, we never directly observe donors' names or other personally identifying information. See Appendix C.2 for details.

[2] Despite their relatively small size — approximately one to two percent of the US population is of Arab heritage, and approximately one percent identifies as Muslim — Arab-Muslims have been particularly targeted in the recent surge of nationalist authoritarianism in the United States. More generally, discrimination against Arab-Muslims and Islamophobic violence and hate speech have risen substantially in recent years (Müller and Schwarz, 2018; Abdelkader, 2016), making it especially important to understand factors that may exacerbate or reduce these prejudices.

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 (a measure of revealed preferences), on explicit attitudes (stated preferences), and on implicit bias (preferences), shedding light on the mechanisms through which long-term contact 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%. Horseracing the effect of exposure to first-generation immigrants against the effects of exposure to second- and higher-generation immigrants, we find evidence that US-born residents of foreign ancestry have greater effects on locals' generosity towards their ancestral country than first-generation immigrants from that country. 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; whereas additional exposure to first-generation immigrants of a given foreign ancestry 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" (white Americans) 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. 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 uniquely 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. We then show that this exposure leads to more positive attitudes, measured by both Implicit Association Test results (IAT) and direct measures: white, non-Muslim respondents who reside in counties with (exogenously) larger populations of Arab ancestry are less implicitly and explicitly prejudiced against Arab-Muslims. These effects on attitudes carry over into measures of political choices: non-Muslim white residents in counties with (exogenously) larger Arab-Muslim ancestry are less supportive of Donald Trump's "Muslim Ban" and, in 2016, were less likely to vote for Donald Trump. Importantly, we control for Republican support in 2012, suggesting that exposure lowers support for anti-Muslim policies and candidates in particular, but not conservative policies in general.

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. Long-run exposure to minority immigrant groups, summing up over all types of day-to-day interaction, 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.[3] Other papers exploit natural experiments, such as the random assignment of roommates or classmates (Boisjoly et al., 2006; Rao, 2019; Carrell et al., 2019; Corno et al., 2019; Scacco and Warren, 2018), the random composition of military bootcamp cohorts (Dahl et al., 2020; Finseraas and Kotsadam, 2017) or the random assignment of province or country for military or missionary deployment (Bagues and Roth, 2020; Crawfurd, 2020).[4]

One important theme in this literature is persistence. Some studies (Schindler and Westcott, 2020; 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 relatively quickly. Recent work has also explored heterogeneity: contact may lead to more positive social preferences in some contexts while having no effects or even negative effects in others. 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. Given these disparate findings, a crucial question concerns the *aggregate effect* of long-run contact: 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 the causal effect of long-term intergroup contact 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 flows lead to greater support for right-wing parties,[5] while other work has found evidence in the opposite direction[6]: for instance, Tabellini (2020) finds that increased immigration to US counties caused higher support for

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[3]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 treatment.

[4]Other work examining the effects of contact with out-groups in schools includes (Billings et al., 2021; Cascio and Lewis, 2012).

[5]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) find that vote shares for both right- and left-wing extremist parties increase in German municipalities containing mosques when election dates are closer to the Ramadan period (a shock to the salience of the Muslim community). Alesina et al. (2018) find that priming subjects to think of immigration lowers support for redistribution. Derenoncourt (2022) finds that migration of African-Americans increased police spending, crime, and incarceration in destination counties.

[6]See, for example, Dill, 2013; Steinmayr, 2016.

anti-immigration legislation, the election of more conservative legislators, and lower redistribution, despite the economic benefits generated for non-immigrants.[7] Though right-wing voting is often associated with negative views toward out-groups, comparing right-wing platforms across countries reveals substantial heterogeneity: voting, while important, may not be a sufficient statistic for the effects of exposure to immigrants on beliefs and prejudice. This may help explain the diverging results documented above. We contribute to this literature by isolating the direct effect of exposure to out-groups on attitudes and altruistic behavior towards these groups, thus shedding light on the underlying mechanisms, and extending the results to dozens of different nationalities. More generally, exposure to out-groups over the period of *decades* may have very different effects than exposure over the period of months or years: we find robust evidence across several different domains that long-term exposure leads more positive attitudes and political choices and greater altruism toward the out-group.[8]

Recent contributions have used Implicit Association Test (IAT) scores as a *predictor* of biased behaviors.[9] For example, Glover et al. (2017) show that cashiers assigned to biased grocery-store managers (as measured by the IAT) are absent more often and perform less well, while Carlana (2019) shows that teachers' gender stereotypes about scientific ability predict the gender gap in maths. Our work instead uses the implicit attitudes as an *outcome* and provides novel evidence that implicit bias can be shaped by long-term exposure to out-groups, complementing recent work in other contexts.[10]

Finally, our work also contributes to an extensive literature on cultural persistence and change by showing that local exposure changes long-term attitudes toward out-groups.[11] More generally, we relate to an extensive literature on prejudice reduction (reviewed in e.g. Paluck et al. 2021).

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

[7] Sequeira et al. (2020), Burchardi et al. (2021), Kerr and Kerr (2016), and Arkolakis et al. (2020) similarly find that immigration leads to economic growth.

[8] Fouka et al. (2020c) 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 et al. (2020a) find that Mexican immigration improves white residents' attitudes and behavior towards Black Americans.

[9] Developed by social psychologists (Greenwald et al., 1998), the IAT is a measure of *implicit* bias that is difficult to manipulate (Greenwald et al., 2009). An important motivation for studying implicit bias is that respondents may not even be aware of their own prejudices, introducing potentially non-classical measurement error into standard survey measures of prejudice even if respondents answer honestly. However, the IAT has also come under increasing scrutiny: we summarize this debate in more depth in Section 2.3.

[10] See, for example, Lowes et al. 2015, 2017; Schindler and Westcott 2020.

[11] See, for example, Alesina et al., 2013; Grosjean and Khattar, 2019; Giuliano and Nunn, 2017. Our results relate to the finding in Voigtländer and Voth (2012) that anti-Semitism in Germany is less persistent in cities with high levels of trade and immigration. More generally, we relate to a literature on the role of experiences in shaping preferences. For example, Malmendier and Nagel (2011) find that individuals who have experienced low stock market returns throughout their lives display more risk-averse investment behavior, while Giuliano and Spilimbergo (2014) find that individuals who experienced a recession when young are more supportive of redistribution and are more likely to vote for left-wing parties.

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 exposure, generosity, and prejudice, with summary statistics provided in Appendix Table A1.[12] 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 pertaining to country f, measured 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 domestic county d.

2.1 Exposure: Historical Migrations and Ancestry

To quantify long-term exposure to members of a given ethnicity, we collect data on the historical ancestral composition of US counties. We conjecture that a person living in a domestic US county d with a larger community with ancestry from a given foreign country f has a stronger exposure to that community (a conjecture we corroborate empirically in Section 4). 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, 1970, 1980, and 1990 waves of the US Census and from the 2006-2010 five-year sample of the American Community Survey (ACS). Appendix B.1 provides additional details.

Our key measure of historical immigration is $I^t_{f,d}$: the number of immigrants who were born in 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 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. Our empirical strategy isolates quasi-random variation in this variable.

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 (C1) and Charity 2 (C2).[13] While both organizations occasionally donate to US-based causes, they primarily

[12] We also use demographic data on US counties from a wide range of sources; see Appendix Section B.2.

[13] 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."

channel donations from US donors towards foreign non-governmental organizations, particularly in response to natural or man-made disasters. We focus solely on donations to specific foreign causes, which allow us to identify the country receiving the donation. After removing donors whom we are unable to match to a unique county of residence, we are left with 80,584 individual donations spanning 2006 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. Figure 1 maps the US distribution of donors and the worldwide distribution of the receiving countries. The figure shows significant variation across counties within the US and across foreign countries.

We pool donations across Charity 1 and Charity 2, restricting our sample to the 44 countries in both datasets.[14] 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.[15] In our main specification, we restrict the sample to donors matched to European countries to approximate a population of white "natives".[16] Given that no recipient country in our dataset is in Europe, this restriction also ensures that our results are not driven by the natural tendency of individuals to donate to their ancestral country. We then aggregate donations at the domestic county d × foreign country f × quarter t level.

2.3 Implicit and Explicit Prejudice

We draw data on implicit and explicit prejudice against Arab-Muslims from two sources. The first source is Project Implicit, a platform run by Harvard University researchers through which respondents can complete Implicit Association Tests (IATs) quantifying implicit prejudice against different groups.[17] IAT scores are generally regarded as difficult to manipulate (Egloff and Schmukle, 2002), and

[14] As we show in Appendix Table A3, we find very similar results if we analyze each dataset individually.

[15] Similar approaches have been used in Fryer and Levitt (2004) and Abramitzky et al. (2014). To ensure donors' privacy, individual donor names are never revealed to us researchers, and details about donations are never revealed to NamSor. See Appendix Section C.2 for details.

[16] 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 names associated with Native American tribes are matched to the United States, while most Americans are matched to European countries. Furthermore, while we cannot rule out the concern that our sample includes spouses of non-European ancestry who took their European-ancestry spouses' names, the fact that our estimates remain similar when we restrict our sample to donors with ancestry from another continent or to donors classified as men suggests that any bias is likely to be small.

[17] IATs require subjects to associate two sets of words and images with either the left or the right side of their screen. Typically, one set includes words and images associated with two demographic groups (for instance, European names and Arab-Muslim names), while the other set includes both positive and negative affective words (such as "peaceful," "frightening," etc.). In each round of the IAT, subjects are told to place one subset of affective words on the same side as one demographic group's names and to place the other subset of affective words with other group's names. For example, if the left side of the screen contains both the "Arab-Muslim" *and* the "good" categories and the right side

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 July 1, 2020 (we use the Asian and Race IATs as placebo outcomes). Subjects taking the IAT answer a set of additional questions, including a "Thermology" question in which they rate their feelings towards the group in question on a scale of 0 ("very cold") to 10 ("very warm"). We use this question as one measure of explicit attitudes. They 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 108,535 white, non-Muslim forced respondents to the Arab-Muslim IAT.[18]

To ensure that our estimates generalize to a representative sample, we turn to Nationscape, a large-scale survey administered by the Democracy Fund Voter Study Group. Nationscape was fielded online to over 300,000 respondents between July 2019 and July 2020 and is broadly representative of the US population.[19] In this survey, respondents explicitly state their favorability toward Muslims. Unfortunately, the Democracy Fund Voter Study Group does not make available individuals' county-level identifiers; the most granular available geographical identifier is Congressional district c (of which there are 435) and we are thus forced to conduct our analysis at this coarser level.[20] To the extent that this assignment introduces measurement error in our measure of ancestry, it should bias our coefficient toward zero. 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. Higher values represent more positive attitudes.

contains the "Other People" *and* the "bad" categories, the subject must assign a positive affective word to the left as quickly as possible, ignoring the "Arab-Muslim" category). Different combinations of these potential categorizations are randomized, and the measure of bias is computed from the difference in speed between categorizing the stereotypical out-group with negative versus positive words. This difference is typically attributed to the respondents' implicit associations or stereotypes.

[18] A wide range of institutions, from law firms to tech companies to police forces to schools and universities administer the IAT as part of diversity trainings and other initiatives (see, for instance, Lawyers Are Uniquely Challenging Audience for Anti-Bias Training, *Bloomberg Law* May 13, 2019; What Facebook's Anti-Bias Training Program Gets Right, *Harvard Business Review*, Aug 24, 2015; Lenora Billings-Harris Leads Unconscious Bias Training for Leadership, *Office for Diversity and Inclusion, The University of Alabama at Birmingham*, Sep 1, 2020). Our results, displayed in the Appendix, are qualitatively unchanged and quantitatively similar if we also include the additional 117,656 "unforced respondents".

[19] Nationscape matches the national population on gender, the four major Census regions, race, Hispanic ethnicity, household income, education, age, language spoken at home, nativity (U.S.- or foreign-born), 2016 presidential vote, and the urban-rural mix of the respondent's ZIP code. Because the survey is administered online and respondents are anonymous, responses are arguably less sensitive to experimenter demand or social desirability bias than those obtained from face-to-face or phone surveys.

[20] Because our instrument is at the county level d, we duplicate observations and assign one duplicate to each county d within district c. We then weight each observation by the population share of district c that lives in county d and cluster standard errors at the district rather than the county level.

2.4 Political Choice

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.[21] In the 2017 and 2018 waves of the CCES, respondents are asked whether they support or oppose the policy.

As our second measure of political choice, we study changes in voting behavior between the 2012 and 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.[22] We thus in part attribute increases in Republican support between 2012 and 2016 to hostility toward Arab-Muslims. As before, we limit to white, non-Muslim respondents. Nationscape, described in Section 2.3, also includes questions on the Muslim Ban and 2016 voting behavior, allowing us to replicate our results using this alternative data source.

2.5 Contact and Mechanisms

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. Bursztyn et al. 2020; 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 ($n = 6,536$) is broadly representative of the targeted population in terms of age, gender, income, Hispanic ethnicity, and education (Appendix Table A2). 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

[21] 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. 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. Indeed, many legal challenges to the ban alleged that the order violated the Establishment Clause of the First Amendment, which bars the government from instituting policies that disfavor a particular religion.

[22] 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; Donald Trump's Plan for a Muslim Database Draws Comparison to Nazi Germany *NBC News*, Nov 19, 2015

ask respondents to select the correct definition of Ramadan among one correct and three incorrect definitions. Second, we ask a multiple-choice, multiple-response question on the Pillars of Islam among a number of possible choices; they 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).

3 Effect of Exposure to Foreign Ancestries on Donations

We begin by examining the effects of exposure to groups of foreign descent 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 flowing 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, $IHS\left(\text{Ancestry}_{d,f}\right)$.[23] 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

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

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 (1), β, approximates the elasticity of donations with respect to ancestry.

The fixed effects included in (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

[23] 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.

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 counties that are and always have been more tolerant towards Arabs 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).[24] Our instrument purposefully excludes 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 her arrival in the US, a migrant from f is more likely to settle in d if she 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 her 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

[24] Variants of this approach have since been employed by Burchardi et al. (2021) 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; Katz and Murphy, 1992), adapted to two dimensions, with a push-pull model (e.g. Card, 2001; Boustan, 2010).

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 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.[25]

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

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

$\text{Controls}_{d,f}^{t}$ include d, f, and $d \times f$ observables. $I_{f,-r(d)}^{s}$ 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 economic pull factors.[26] In practice, including these principal components or not has little impact on our estimates.

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: 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),

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

[26] 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.

the first wave corresponds to the push-pull term $\delta_{1920} I^{1920}_{Mexico, not\, Midwest} \frac{I^{1920}_{not\, Latin\, America, Detroit}}{I^{1920}_{not\, Latin\, America}}$; the next two waves are summarized in the principal components.

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

3.3 Main Results

Table 1 presents estimates from variations 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 time (quarter) and the interaction of quarter and 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 the interaction of time and state fixed effects, and Column 4 replaces the county-level demographic controls with the interaction of time and county fixed effects.

Our preferred estimate in Column 4 (0.106, 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 0.10 (approximately two-thirds of a standard deviation).[27] 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 probability that any residents with European names in the county donate to country f by 4.6% and increases the dollar amount of donations by 0.328% (Charity 2 only).

To put those 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.

Importantly, as all specifications include county fixed effects, the impact of exposure is specific to each immigrant group, and arises even after any cross-county differences in overall generosity are controlled for: exposure to 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.

3.4 Robustness of Instrumental Variables Strategy

We next probe the robustness of our instrumental variable strategy. To this end, 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 in 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 large changes in the OLS coefficient suggest that some of the positive association between donations and ancestry could be explained by the fact that counties with more residents of foreign ancestry are simply 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 0.106 (s.e.=0.043) in column 4, as we add more and more stringent controls – in particular 150,768 interacted quarter $\times$ county fixed effects when going from column 3 to 4. This

[27] Consistent with Burchardi et al. (2019), the F-statistics on the excluded instruments are well above critical levels throughout (331.3 in column 4), showing that the first stage has sufficient power across all of these variations.

stability suggests that our instruments successfully isolate exogenous variations in ancestry that is orthogonal to such confounding factors across counties and countries. Instead, the IV estimates are identified by within-county variation in ancestry composition that we trace directly to the coincidence of historical push and pull factors – the fact that counties will have received relatively more migrants from those ethnic origins that happened to send many migrants to the United States when the county was particularly attractive to migrants from all origins.

The OLS estimates tend to be about an order of magnitude smaller than the IV estimates. 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 would 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).

Table 2 shows variations of our standard specification using different instruments for our first stage Equation (2), each designed to address different potential concerns with our instrumentation strategy. Column 1 drops the principal components summarizing higher-order interactions between push and pull factors, which arise recursively due to the social factor in migrations. Our results are unchanged even if we rely only on the simple interactions between push and pull factors in equation (2).

The remaining columns show that our results remain virtually identical if we alter the construction of our instruments to allow for a range of other types of confounding variation. In our standard specification, we measure the "pull" factor (the county's attractiveness to the average migrant arriving at the time) 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 2, we go one step further by measuring 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.

Reassuringly, doing so has essentially no effect on our estimate. In Column 3, 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,[28] again with no effect on our coefficient of interest. Finally, in Column 4, 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 variations in our instrument construction yield almost identical results bolsters our confidence that they isolate quasi-random variation in the ancestral composition of US counties.

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 Panel B of Table 2, 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 (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 (0.115, 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 (0.157, s.e.=0.076), reflecting the natural tendency of people to donate to their ancestral country.

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 Figure 5 and the discussion of heterogeneity by gender in Section 4.3).

3.5 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

[28]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.

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 disproportionately positive attitudes and generosity toward Mexicans.

While such selective white flight would have to operate at an implausibly granular scale to bias our estimates — for example, some types of white Americans would have to move away from Somalis but not Nigerians, while others would have to move away from Nigerians but not Somalis — we systematically test 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 high value if white residents leaving d move to counties with a disproportionately large ethnic enclave from f. For instance, for $d = Detroit$ and $f = Mexico$, this index takes a high value if a large share of white movers from Detroit choose domestic locations where Mexican ancestry is large relative to its national average. Under the selective white flight hypothesis, white natives who dislike neighbors from f selectively move towards places with *few* residents from f: the larger the community from f in d, the higher the value of the index.

In Columns 1–3 of Table 3, we estimate various specifications of

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

where we again instrument for ancestry using (2). Panel A shows results for moves by white Americans between 1970 and 1980, as reported in the 1980 census, the earliest census that contains enough information to construct (4). Panel B examines the entire period between 1970 and 2000, using also the data from subsequent censuses. Columns 1-3 show specifications corresponding to those in Table 1. While selective white flight would manifest as a significant *negative* coefficient on ancestry; we instead find either positive or precise zero effects. In particular, the estimated coefficient in our preferred specification in column 3 of Panel B is -0.009 (s.e.=0.007). We thus find no evidence of the kind of selective white flight that could bias our results. Columns 4 and 5 show the same is true when we estimate this specifications for Arab countries alone, as we will discuss below.

3.6 Additional Robustness Checks

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

Sample restrictions In Appendix Table A3, we verify that all of our main results hold if we examine both charities individually (considering the full set of countries in both charities' datasets rather than restricting to those countries in both).

Strategic targeting One alternative interpretation of our results is 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.[29]

Standard errors Throughout our analyses, we employ two-way clustering at the foreign country and domestic county levels. In Appendix Table A4, we present the standard errors associated with five other possible 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. Our estimates remain statistically significant under every choice. Our baseline two-way clustering at the country-county levels is the most conservative.

Permutation test As an alternative and more demanding approach to inference, we conduct a series of permutation tests. In particular, we randomly match each country in our dataset to another "placebo" country. For each observation, we then switch the endogenous variable (IHS-transformed ancestry) and the excluded instruments to those associated with the placebo country, keeping the donation outcome the same (i.e. donations to the original rather than placebo country). We then

[29] The senior manager at Charity 1 who authorized our access to the data wrote: "To answer your question, [Charity 1] does not strategically target counties based on ancestry. We also do not regularly target based on region or demographics." The Director of Data Science and Analytics at Charity 2 (Global Giving) wrote: "We don't strategically target any of our disaster-specific marketing to regions with large diaspora communities who may have families affected by an event. It's entirely plausible that a disaster might affect a large segment of people to whom we'd likely be reaching out anyway (our coronavirus response is one example of this, Hurricane Sandy, or California Wildfires might be others), but that's entirely happenstantial and not the result of any kind of strategic targeting choice based on geography."

estimate Equation (1) (including county and quarter fixed effects) in this permuted sample. This regression recovers, 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 should be approximately normally distributed at zero. If exposure to people of foreign descent instead leads to greater donations to foreign countries in general, the distribution of these placebo coefficients should have a positive mean.

Appendix Figure A1 plots the distribution of one thousand placebo coefficients, with the true coefficient indicated by a dashed line. As expected, placebo coefficients are centered on zero, and very few are larger in magnitude than the true coefficient: the corresponding p-value (the rate at which we falsely reject the null hypothesis) is 0.03, similar to that which we compute in our main estimates.

Subsets of countries and counties Appendix Table A5 instead explores the robustness of our main finding to removing specific subsets of countries (Panel A) or counties (Panel B). Column 1 of Panel A replicates our baseline estimates; Column 2 drops Arab countries; Column 3 drops Latin American countries; and Column 4 drops non-Arab African countries. In Panel B, we instead explore whether any of the four US Census regions drive our estimates: we drop the Northeast in Column 1, the Midwest in Column 2, the South in Column 3, and the West in Column 4. While the estimates do change across the different samples in Panel A, they remain economically large and statistically significant at least at the 10% level. In Panel B, estimates are highly stable and statistically significant. We conclude that while the effect of exposure on generosity may vary between different foreign ancestries, no specific group of countries or specific US Census region drives the overall effect.

Percent functional form As discussed in Section 2, our primary specification places weight on the *absolute* size of the foreign community. However, one might instead think that the *relative* size — that is, the size of the foreign community as a fraction of the population — is the measure of interest, since it may better proxy for personal interaction with people with ancestry from f or for discussion of issues pertinent to f in the local media. To facilitate this alternative interpretation, in Appendix Table A6, we replicate our main specifications using ancestral shares, rather than IHS-transformed ancestral population, as the endogenous variable. Our coefficient estimates are stable across specifications and statistically significant in robustness checks. Our baseline specification suggests that a one percentage point increase in ancestry from country f increases donations by residents with European names to that foreign country by 1.9% percent.

4 Examining Mechanisms

Having established that long-term exposure to 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 effects of exposure to 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 First vs. Higher Generation Immigrants

A small literature in economics and sociology (e.g. Barrera et al. 2021; Kunst and Sam 2014) studies natives' attitudes toward first-generation immigrants (those born in a different country) vs. their attitudes toward second or higher-generation immigrants (those born in the United States, but whose parents, grandparents, etc. were of foreign birth).[30] In particular, some authors have argued that natives' attitudes toward second-generation immigrants are more positive (or less negative) than attitudes toward first-generation immigrants (Hernandez et al., 2008).

Is exposure to 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).

Table 4 presents the results of this horserace. 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 induce sufficient statistical power to allow us to separately examine variation in the number of descendants versus first-generation immigrants, reporting the Sanderson-Windmeijer conditional first-stage F-statistics of both variables (Sanderson and Windmeijer, 2016).[31]

An exogenously larger foreign-born population from 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

[30] Also related is work examining the mechanisms through which higher-generation immigrants either facilitate or hinder their parents' assimilation (e.g. Kuziemko and Ferrie 2014).

[31] 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.

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 non-European migrants have a larger impact on donations made by Americans with European names than foreign-born migrants. This larger impact could reflect the fact US counties with large populations of foreign ancestry 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 *bridge* the cultural gap 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 Arab-Muslim countries (Algeria, Bahrain, Comoros, Djibouti, Palestine, Iraq, Jordan, Kuwait, Lebanon, Libya, Mauritania, Morocco, Oman, Qatar, Saudi Arabia, Somalia, Sudan, Syria, United Arab Emirates, Tunisia, Egypt, Yemen) 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 estimate the effect of exposure to Arab-Muslims on donations by local residents, we estimate a simplified version of (2):

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

where again δ_t is a time fixed effect and we instrument the (IHS-transformed) number of residents of Arab ancestry in domestic county d, $IHS\left(\text{Ancestry}_{d,Arab}\right)$, using (2). All specifications control for a time fixed effect (δ_t) and for logged county population in 2010. As before, we continue to 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.

Appendix Figure A2 shows the predicted distribution of Arab-Muslim ancestry across counties graphically, where we residualize values by state fixed effects and log population. Appendix Figure A3 plots the distribution of (actual) Arab-Muslim ancestry against county population, showing that Arab-Muslim communities are distributed across small and large counties.[32]

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 directly address concerns about omitted variables, we estimate a series of IV regressions on 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 A4 plots the coefficients of this balance test.[33] We identify four cross-sectional variables that are significantly correlated with our instrument: counties with a larger predicted Arab ancestry are more likely to be rural, with a slightly higher share of residents over the age of 65 and below the federal poverty line, and 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 4.2.5 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 results. Paralleling our previous findings, an exogenously larger Arab population in county d substantially increases the flow of donations from d to all Arab countries. The first-stage F-statistics tend to be below 10, but in all cases, we report p-values from weak IV-robust inference (based on Conditional Likelihood Ratio tests, following Andrews 2016; Sun 2018).[34] The estimated effects are substantial: in our preferred specification (Column 3), a one-unit increase in the IHS-transformed Arab population (approximately half a standard deviation increase) causes a 0.401 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 (0.106 in Table 1) suggests there exist positive spillovers between communities originating from nearby countries, such that (for example) a larger community from Jordan may increase generosity towards Syria.

[32] Figure A3 also shows that our IHS transformation, which is bounded at zero with $IHS(0) = 0$, does not alter the approximate linear distribution of Arab-Muslims.

[33] As these coefficients are estimated by IV, neither measurement error in Arab ancestry nor in the outcome variables would bias the coefficient toward zero.

[34] In the presence of weak instruments, the IV estimate is biased toward the OLS estimate. Because our OLS estimates are smaller in magnitude than our IV estimates, to the extent that weak instruments bias our point estimates, they will do so toward zero.

Our results are robust to controlling for a battery of county-level demographic controls (those identified in Appendix Figure A4 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 0.374 (s.e.=0.058). Adding the interaction of state and time fixed effects raises it slightly to 0.401 (s.e.=0.060). Thus, any other county-level omitted variables would have to have dramatically larger effects than these observables to materially impact our results. Our instruments 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 base 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 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 exposure to people of Arab-Muslim ancestry on the 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 × 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 0.075 (s.e.=0.027) 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). 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 A3). We show these results graphically in Panels A and B of Figure 2.

Notably, our Project Implicit estimates remain stable with and without state fixed effects (Column 2 vs. Column 3) and as we introduce a series of "bad controls" (Angrist and Pischke, 2009). Column 5 shows a placebo experiment where we control for the overall population with non-European ancestry, a proxy for the total size of the local minority (non-white) population. Doing so has essentially no effect on the coefficient of interest, and the coefficient on the population with non-European ancestry is statistically indistinguishable from zero. Thus, our effects are not driven by exposure to non-white residents in general, but specific to Arab-Muslims.

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 statistically significant and similar to our preferred specification across all of these variations, suggesting that our measures of implicit and explicit prejudice toward Arab-Muslims do not simply proxy for general prejudice against minorities or for political or social conservatism.

Although also positive and statistically significant, the OLS estimates in Column 1 are smaller than the IV estimates (Columns 2–8). As in the generalized analysis, this likely reflects measurement error in the endogenous variable.

All respondents In Appendix Table A7, 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.

Representativeness To further ensure that our results are not driven by selection into Project Implicit tests, we replicate our analysis using outcomes from Nationscape. The results are displayed in Column 1 of Appendix Table A8. Our coefficient estimates remain strong and statistically significant in all specifications, although the magnitude is smaller; we attribute this difference to the imputation procedure discussed in Section 2.3.

Auxiliary explicit outcomes Appendix Table A9 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). Interpreting these results is not straightforward: for example, does disagreement with the statement in Panel B ("I attempt to appear nonprejudiced toward Arab Muslims in order to avoid disapproval from others") indicate that the respondent is unconcerned with being prejudiced against Arab-Muslims, or that she avoids prejudice against Arab-Muslims for reasons other than avoiding disapproval from others? Nevertheless, 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.

Placebo outcomes As further evidence that our regressions are capturing effects on natives' attitudes specifically toward Arab-Muslims, rather than toward immigrants or minorities more broadly, Appendix Table A10 shows a series of placebo regressions. It shows no statistically detectable effect of Arab ancestry on implicit attitudes towards Asians and African Americans, nor on the explicit attitudes of white respondents towards Asians. Interestingly, we do find a small positive effect of Arab ancestry on explicitly stated attitudes towards African Americans, which is about a quarter of the size of the direct effect on explicit attitudes towards Arab-Muslims.[35] Although the estimated effects of Arab-Muslim ancestry on implicit and explicit attitudes toward Asians and Black Americans are positive, the estimates are substantially smaller than the analogous estimates 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.

"Selective white flight" As with our main results, columns 4–5 of Table 3 show there is no evidence of selective white flight that could result in white residents who dislike Arabs leaving their home counties towards other counties with relative fewer Arabs. If anything, exposure to Arab communities appears to make white residents more likely to relocate to areas with relatively *large* Arab populations, conditional on moving at all.

[35]Such a spillover to attitudes towards Black Americans is consistent with the findings of Fouka et al. (2020b) who show that greater inflows of Hispanic immigrants change natives' attitudes toward Black Americans. However, if these spillover effects are indeed positive, as Fouka et al. (2020b) find, then our estimated null effects would suggest that our instrument is *negatively* correlated with county-level tolerance toward out-groups, which would work *against* finding an effect in our main specification. Negative spillovers, on the other hand, might result in null effects in our placebo regressions even if our instrument is positively correlated with county-level tolerance. However, for this argument to explain our main estimates, we would require that exposure to Arab-Muslims has no effect (or only a small effect) on attitudes toward Arab-Muslims, but a large negative effect on attitudes toward other groups, a possibility we view as unlikely.

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 2016 voting for presidential candidate Donald Trump (controlling for voting for 2012 Republican candidate Mitt Romney).

Table 7 shows coefficient estimates using our individual-level specification, Equation (7), again limiting to white, non-Muslim respondents. The results suggest that exogenous exposure to people 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).[36] To put these magnitudes in perspective, half a standard deviation increase in the population of Arab ancestry reduces support for candidate Trump as much as a 14 percentage point decrease in the 2012 Republican vote share.[37] We show these results graphically in Panels C and D of Figure 2.

As an even sharper test, we can exploit the fact that in the 2016 wave of the CCES, respondents report the candidates for whom they voted *both* in 2016 and in 2012. Appendix Table A11 replicates Table 7 but controls for respondents' *own* 2012 vote rather than the vote share of their county. Our sample size drops substantially due to data limitations, but we still estimate statistically significant effects of exposure on Trump voting across specifications, suggesting that Trump, the most saliently anti-Muslim presidential candidate in recent memory, activated latent political preferences in a way that Romney did not. We also replicate these results in the Nationscape data (Columns 2 and 3 of of Appendix Table A8), although the estimated magnitudes are smaller. This may again reflect the measurement error associated with our assignment of Nationscape respondents to counties.

To put our estimates in perspective, comparing two individuals, one who voted for Mitt Romney in 2012 and one who did not, the chance of voting for Donald Trump is 72 percentage points higher for the first. In contrast, comparing two individuals, one who lives in a county where the local population of Arab-Muslim ancestry is half a standard deviation larger than where the other lives (approximately one extra IHS point), the chance of voting for Donald Trump is 6 percentage points lower for the first

[36] Not all Arab countries were targeted by the Muslim Ban (e.g. Egypt, Algeria, Morocco), and one country targeted by the ban, Iran, does not have a majority Arab population. In Appendix Table A12, we consider two alternative specifications. In Panel B, we instrument total ancestral population across all countries targeted by the Muslim Ban with a set of instruments constructed specifically with these countries. In Panel C, motivated by the possibility that exposure to Muslim immigrants from non-Arab countries may also influence attitudes toward and political choices regarding both Arab-Muslims and Muslims in general, we repeat this exercise considering the effects of exposure to immigrants from *all* Muslim-majority countries. Our estimates are qualitatively and quantitatively similar across all outcomes.

[37] This quantification should be interpreted with caution: first, our estimates are local average treatment effects; second, while we exploit exogenous variations in Arab ancestry, variations in the 2012 Republican vote share are endogenous.

(Appendix Table A11 Column 3). In other words, the effect of a half standard deviation increase in the local population of Arab-Muslim ancestry undoes about 10% of the effect of party affiliation.

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, which are by no means mutually exclusive: personal contact and knowledge. First, to the extent that a greater population of Arab-Muslims in a respondent's county 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 — for example, due to greater and more accurate coverage on local media and social media, due to changes in information-seeking behavior, or due to greater "indirect contact" (e.g. with social acquaintances who themselves have greater personal contact with Arab-Muslims). Such increased knowledge may translate into greater knowledge and greater altruism, especially if it leads residents to update negative priors (Grigorieff et al., 2020; Audette et al., 2020).

Personal contact We begin by examining whether living in a county with an exogenously greater population of Arab-Muslims indeed translates into substantially greater personal contact with Arab-Muslims. In Panel A of Table 8, we estimate the effects of the IHS-transformed Arab population in a respondent's county on several binary outcomes: whether the respondent is friends with an Arab-Muslim (Column 1), whether the respondent is acquainted with an Arab-Muslim through work (Column 2), whether the respondent has an Arab-Muslim neighbor (Column 3), and whether the respondent has eaten in a Middle Eastern restaurant (Column 4). Column 5 reports effects on a binary variable taking value one if any of the binary variables in Columns 1–3 take value one.

We find statistically significant effects on all outcomes except for the "friends" indicator (though the estimate here, too, is positive). The effect sizes are large: a one-unit increase in the IHS of the Arab population (approximately half a standard deviation increase) translates into an approximately 13% increase in the probability that the respondent has an Arab-Muslim friend, neighbor, or workplace acquaintance. Once again, using weak instrument-robust inference, we are able to reject the null of a zero effect for every coefficient that is statistically significant under Wald standard errors. We show these results graphically in Panels A and B of Figure 3.

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.

Knowledge of Arab-Muslims 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 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. 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.

We once again find economically large effects. A one-unit increase in the IHS of the Arab population (approximately half a standard deviation) translates into a 0.13 lower score on the measure of negative beliefs, a 0.17 standard deviation change. It also translates into a 0.43 higher accuracy in guessing the Pillars of Islam (scored from 0 to 7, with a mean of 4.5 and a standard deviation of 1.6), and an 11% greater probability that the respondent will correctly define Ramadan. To put these magnitudes into perspective, the corresponding gaps between respondents with and without college degrees are -0.15, 0.678, and 14%, respectively. An exogenously greater Arab population also increases the accuracy of respondents' guess about the size of the Arab population in the United States: a one-unit increase translates into 3% greater accuracy, approximately 0.22 standard deviations. Turning to the index, a one-unit increase in the size of the Arab population increases scores by 0.38 standard deviations. We show these results graphically in Panels C and D of Figure 3.

4.2.5 Additional Robustness

Percent functional form In Appendix Table A13, 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.

Stability of coefficient estimates We examine the stability of our estimated coefficients to the inclusion of the unbalanced county-level characteristics displayed in Appendix Figure A4 in Appendix Tables A14 (attitudes) and A15 (politics). We find that the estimated effects on attitudes are quite stable to the inclusion of each unbalanced characteristic individually or all of them together. Similarly, the estimated effects on Trump voting change little across specifications, whereas the estimated effects on Muslim Ban support are somewhat less stable.

4.3 (Lack of) Heterogeneity

We conclude this section by examining whether the effect of exposure to descendants of foreign migrants varies systematically across counties with different political leanings, and whether this effect varies by gender. We find no evidence that more liberal counties are more or less responsive to exposure than conservative counties, nor that women are more or less responsive to exposure than men.

To explore the heterogeneous effect of exposure across the political spectrum, we estimate an augmented version of Equation (1) in which we interact our measure of ancestry, $IHS\left(\text{Ancestry}_{d,f}^{t}\right)$ with indicators for the county's tercile of 2012 Republican vote share. Figure 4 shows heterogeneity by counties' 2012 Republican vote share for all outcomes above: donations, donations specifically to Arab countries, implicit and explicit attitudes toward Arab-Muslims, support for the Muslim Ban, Trump voting, contact with Arab-Muslims, and knowledge of Islam. The results suggest that the estimated effects of exposure are positive and similar in all three terciles. While point estimates vary slightly between liberal and conservative counties, we cannot rule out the null hypothesis of homogeneous treatment effects.

We next turn to heterogeneity by gender. We interact indicators for whether the respondent is male or female (or, for the donations data, whether the donors is predicted to be male or female based on their name) with $IHS\left(\text{Ancestry}_{d,f}^{t}\right)$. Figure 5 reports these coefficients. Again, we find no systematic evidence of heterogeneity by gender across any of the eight outcomes we study.

5 Conclusion

In this paper, we examine the effect of decades-long exposure to individuals of foreign descent on natives' generosity, attitudes, and political choices toward them, exploiting exogenous variation in the ancestral composition of US counties generated by historical "push" and "pull" factors in immigration. We find that long-term exposure to a larger population from a given country induces greater generosity toward that group, as measured by charitable donations. This effect of exposure on generosity appears to transmit itself over long periods of time and is driven disproportionately by exposure to the descendants of immigrants rather than exposure to first generation immigrants.

Focusing on the case of Arab-Muslims to examine mechanisms, we find that long-run 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 be underlying these effects.

We add three primary caveats to our analysis. First, our results focus on ethnic differences induced by voluntary historical migrations, but not those induced by the legacy of slavery. Though it may be possible to extrapolate our findings regarding donations to causes in Africa to more positive attitudes towards African Americans in general, the legacy of slavery in the United States deserves careful, separate, attention. Second, our focus is on the types of long-run effects that are relevant for aggregate outcomes. While we are able to characterize these average effects in some detail, we are purposely agnostic about what types of contact or exposure may have larger or smaller effects in particular circumstances. That is, 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 produces positive attitudes toward Arabs in every circumstance. Instead, our work characterizes the sum of the effects of the presence of foreign ethnic groups over long periods of time. Finally, the groups we examine — both in our generalized analysis and in our case study of Arab Muslims — constitute relatively small fractions of the population. It is possible that long-run exposure to much larger groups (for example, the large and growing populations of Arab ancestry in some European cities) fails to induce positive effects or even leads to backlash.

Our results suggest several directions for further research. First, our goal in this paper is to assess the effects of long-term exposure, intentionally aggregating across different types of interactions and time periods (e.g. periods of economic growth vs. contraction, periods where international conflicts are more or less salient, etc.). However, 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? 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, how does the horizontal and vertical transmission of beliefs about immigrant groups — for example, transmission from neighbor to neighbor or from parents to children — mediate the effects of exposure?

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Tables and Figures

Figure 1: Donations by origin (top) and destination (bottom)

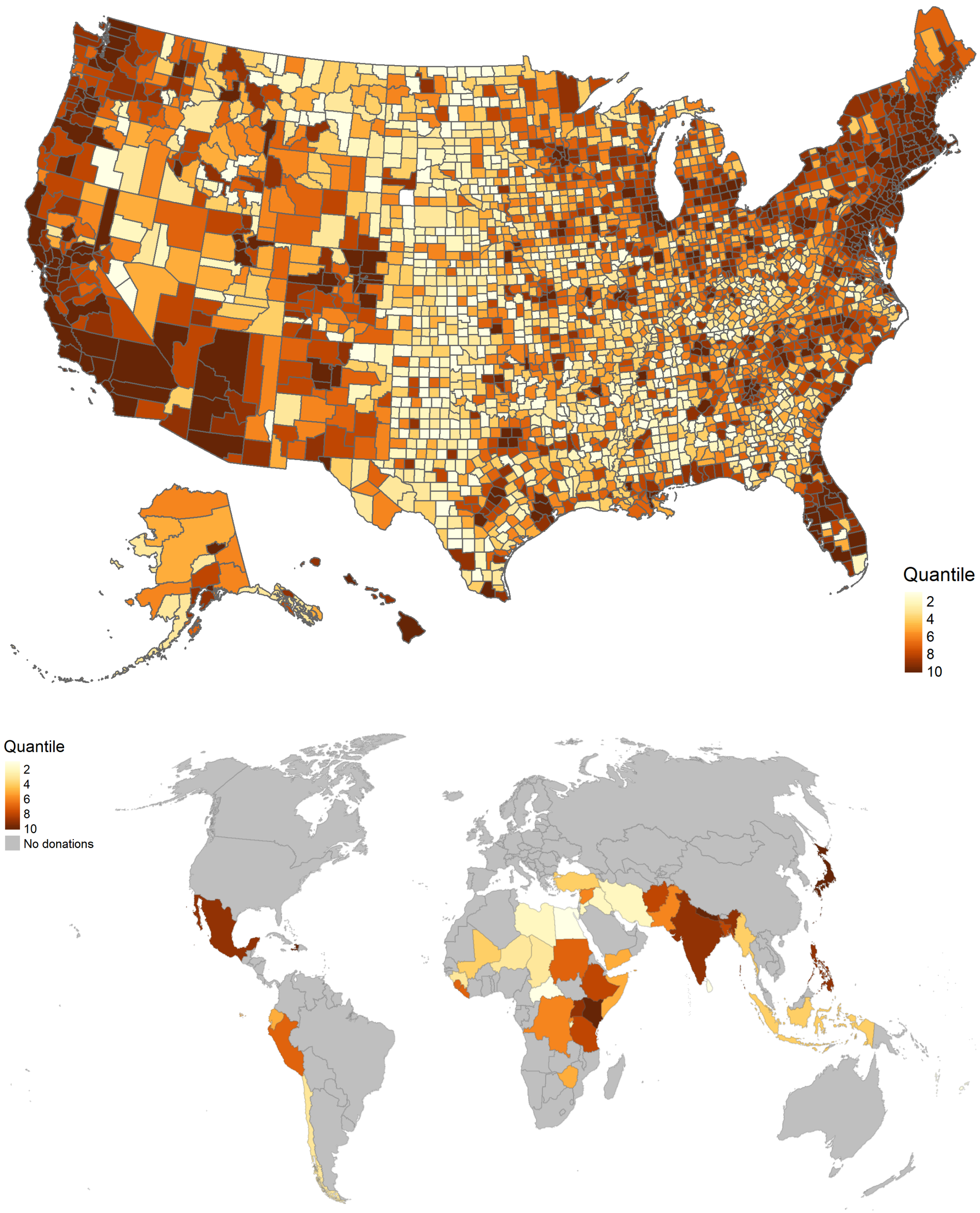
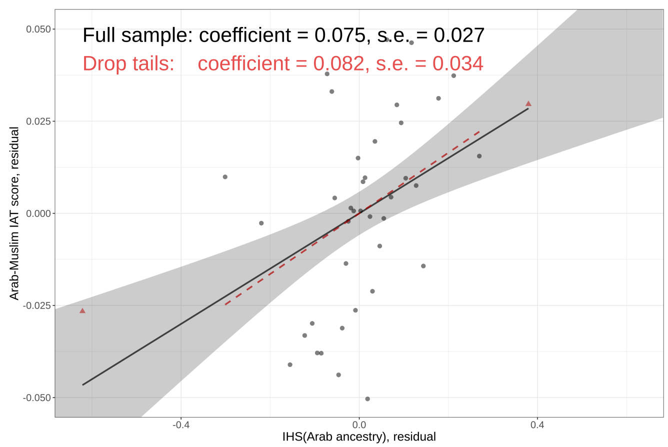
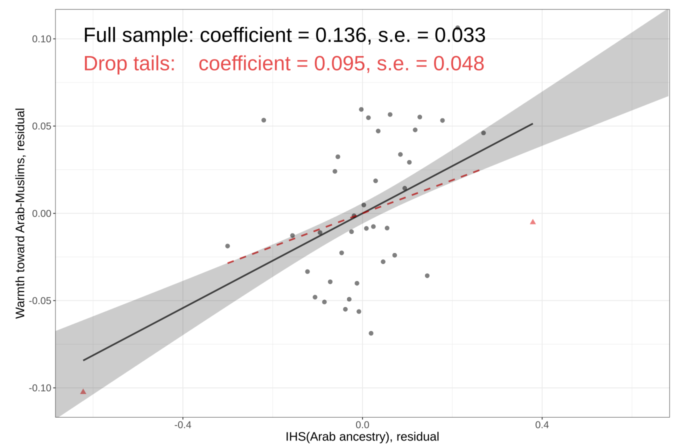


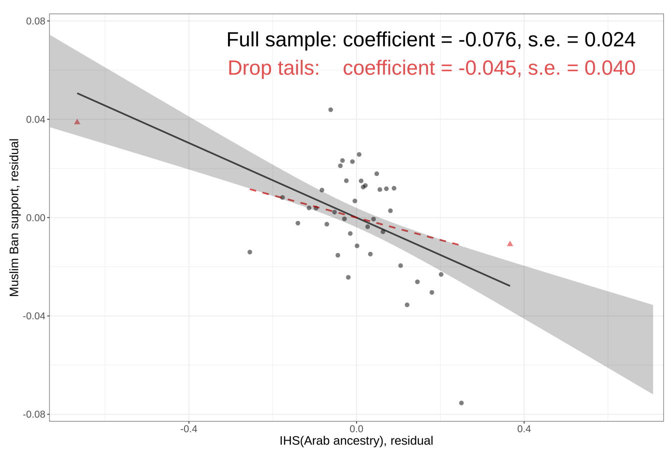
Figure 2: Binned Scatter Plots of Attitudes and Political Preferences



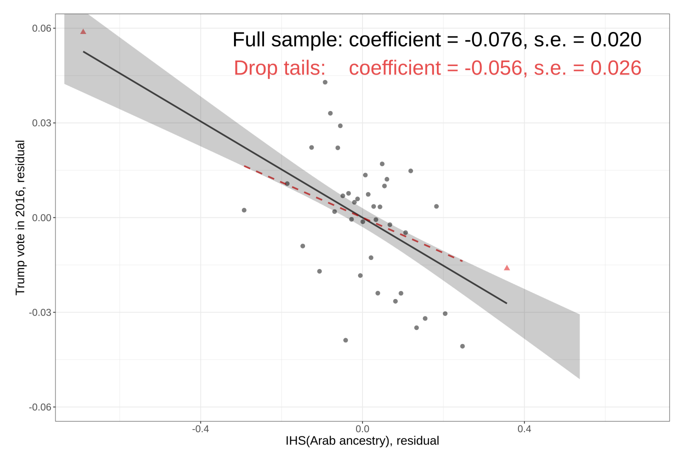
(a) Score on Arab-Muslim IAT (Project Implicit)



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



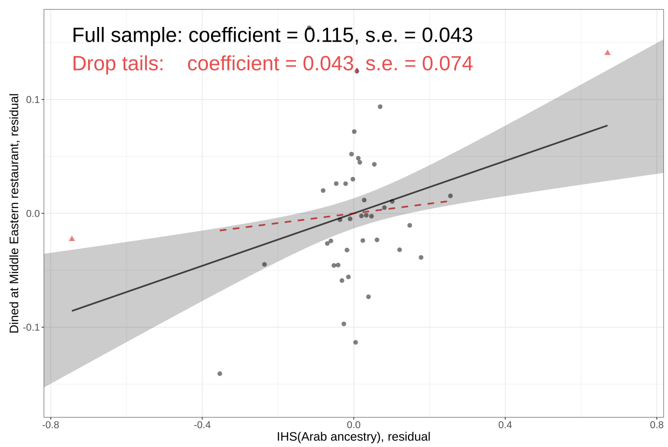
(c) Muslim Ban support (CCES)



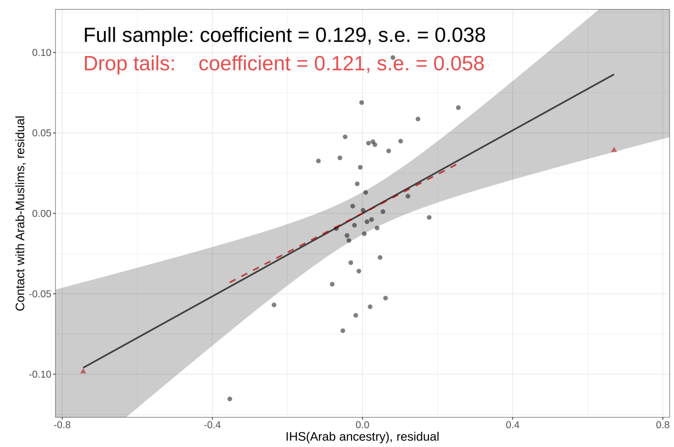
(d) Trump vote in 2016 (CCES)

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 outcomes and instruments 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 observations in the top and bottom 2.5% of fitted values. Standard errors are clustered at the county level. 95% confidence intervals are reported.

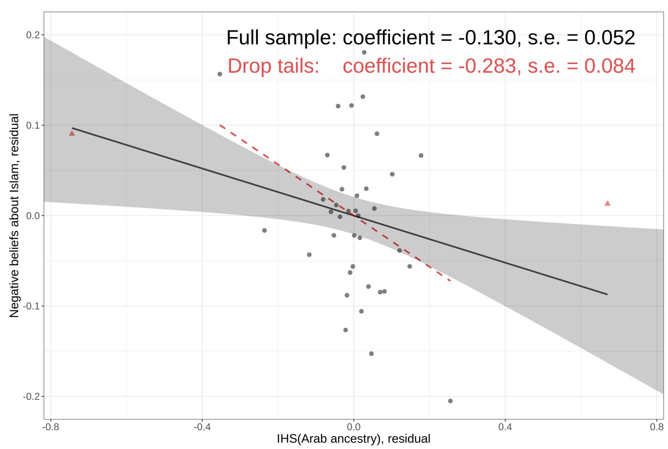
Figure 3: Binned Scatter Plots of Contact and Knowledge



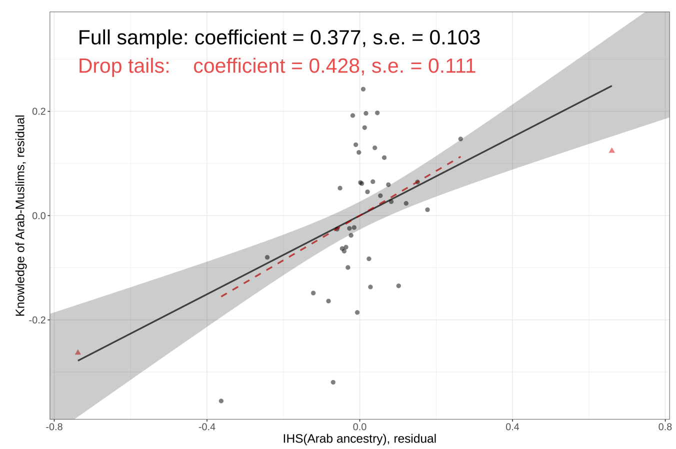
(a) Visited Middle Eastern restaurant



(b) Contact with Arab-Muslims



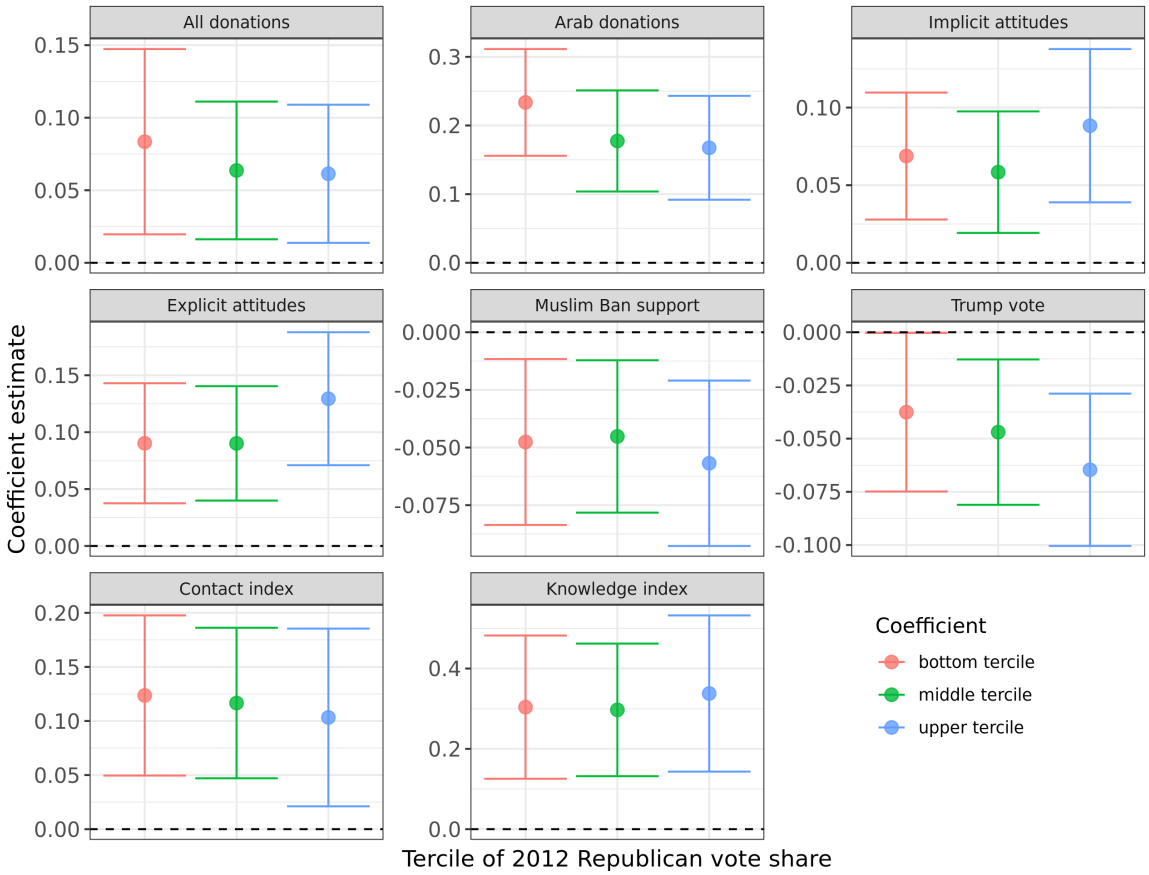
(c) Negative beliefs about Islam



(d) Knowledge about Islam

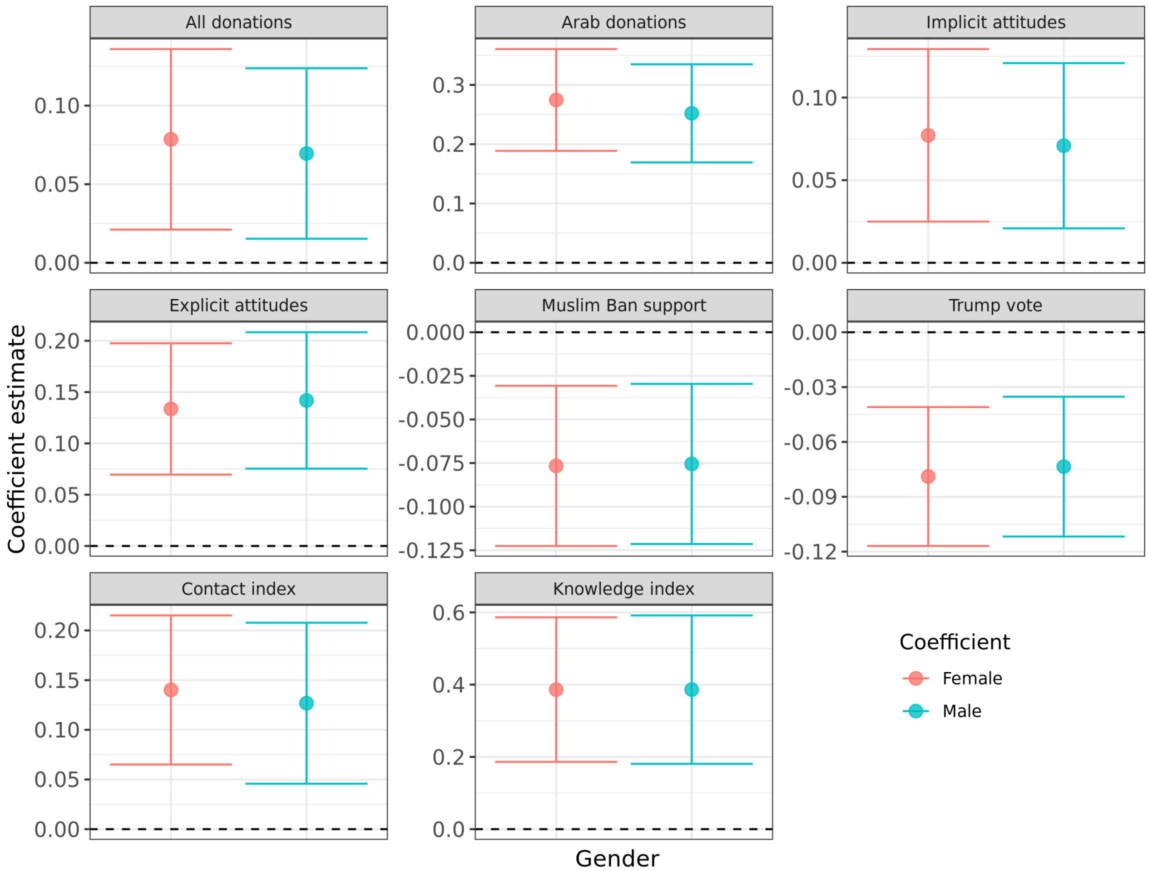
Notes: Figure 3 presents binned scatter plots displaying the relationship between the fitted values of IHS(Arab ancestry) and four outcomes: an indicator taking value one if the respondent reports ever visiting a Middle Eastern restaurant, an indicator taking value one if the respondent personally knows 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.

Figure 4: Heterogeneity by 2012 Republican vote share



Notes: Figure 4 presents the estimated coefficients on the interactions between a set of indicator variables for counties' tercile of the 2012 Republican vote share and our measure of ancestry. 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, as well as the interactions of all of these variables with the 2012 Republican vote share tercile indicators. We 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 clustered at the foreign country and domestic county level. Error bars represent 95% confidence intervals.

Figure 5: Heterogeneity by gender



Notes: Figure 5 presents the estimated coefficients on the interactions between indicator variables for individuals' gender (or their predicted gender, based on their name, for the two donations outcomes) and our measure of ancestry. 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, as well as the interactions of all of these variables with the gender indicators. We control for logged county-country distance and latitude difference as well as foreign country × quarter and domestic county × quarter fixed effects. Standard errors are clustered at the foreign country and domestic county level. Error bars represent 95% confidence intervals.

Table 1: Effect of ancestral exposure on donations

	(1)	(2)	(3)	(4)	(5)	(6)
	IHS(# donations)				Donations (dummy)	IHS($ donations)
Panel A: IV						
IHS(Ancestry)	0.139^{***}	0.133^{***}	0.132^{***}	0.106^{**}	0.046^{**}	0.328^{**}
	(0.028)	(0.032)	(0.033)	(0.043)	(0.021)	(0.136)
First-stage F-statistic	417.1	403.6	392.6	331.3	331.3	338.1
Panel B: OLS						
IHS(Ancestry)	0.015^{***}	0.010^{***}	0.009^{**}	0.004	0.002	0.015
	(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	0.181	0.181	0.181	0.182	0.121	0.652
Observations	4,703,862	4,700,864	4,700,864	4,708,359	4,708,359	3,976,506
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. Only donations from donors with European-ethnicity names are included. 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 d. 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 2: Stability of estimated effect of ancestral exposure on donations

	(1)	(2)	(3)	(4)
Panel A: Varying instruments				
	No PCs	Eur. only pull	Excl. corr. origins	Excl. corr. dest.
IHS(Ancestry)	0.114^{**}	0.099^{**}	0.095^{**}	0.106^{**}
	(0.043)	(0.040)	(0.040)	(0.046)
First-stage F-statistic	330.4	133.6	160.0	204.3
Panel B: Varying population				
	European donors	Other continents	Other countries	No country restriction
IHS(Ancestry)	0.106^{**}	0.110^{**}	0.115^{**}	0.157^{**}
	(0.043)	(0.045)	(0.048)	(0.076)
Dep. var. mean	0.019	0.021	0.022	0.024
Dep. var. s.d.	0.182	0.192	0.200	0.209
First-stage F-statistic	331.3	331.3	331.3	331.3
Observations	4,708,359	4,708,359	4,708,359	4,708,359
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. In Panel B, Column 1 additionally 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 d. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$. In all columns, except Column 1 in Panel A, we additionally include the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. In Panel A, Column 2 uses an alternative construction of the instrument that calculates the pull factor based only on European emigrants; Column 3 uses an alternative construction of the instrument that excludes countries with correlated migrant flows; Column 4 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Effect of ancestral exposure on White Flight

	(1)	(2)	(3)	(4)	(5)
	All countries			Pooled Arab	
Panel A: 1980 cross-section	*Selective white flight index*				
IHS(Ancestry)	0.047***	0.020	−0.003	0.025***	0.013***
	(0.017)	(0.012)	(0.010)	(0.002)	(0.001)
Dep. var. mean	0.041	0.041	0.041	0.070	0.070
Dep. var. s.d.	0.060	0.060	0.060	0.024	0.024
Observations	132,612	132,612	132,612	3,084	3,084
Panel B: 1980-2000 panel	*Selective white flight index*				
IHS(Ancestry)	0.035***	0.016**	−0.009	0.026***	0.017***
	(0.010)	(0.007)	(0.007)	(0.001)	(0.001)
Dep. var. mean	0.036	0.036	0.036	0.062	0.062
Dep. var. s.d.	0.061	0.061	0.061	0.029	0.029
Observations	363,802	363,802	363,802	9,333	9,333
Foreign country FE	Yes	Yes	Yes	No	No
US state FE	No	Yes	—	No	Yes
US county FE	No	No	Yes	No	No

Notes: The table presents coefficient estimates from regressions at the country-county level (Panel A) and the country-county-decade level (Panel B). The dependent variable is the selective White flight index, defined in Section 3.5. Panel A presents a cross-sectional regression for the year 1980, while Panel B presents a panel regression for the years 1980, 1990, and 2000. The endogenous variable in Columns 1–3 is the IHS-transformed population with ancestry from country d; the endogenous variable in Columns 4 and 5 is the IHS-transformed population with ancestry from Arab League countries. The excluded instruments include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,1980}$ and the first five principal components of the higher-order interactions. Columns 4–5 limit the sample to domestic county–foreign country pairs in which the foreign country is in the Arab League. Standard errors are given in parentheses. Standard errors are clustered at the foreign country level in Columns 1–3 and are robust in Columns 4–5. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Horseracing ancestral exposure vs. exposure to first-generation immigrants

	IHS(# donations)			
	(1)	(2)	(3)	(4)
IHS(Foreign-born 2010)	0.193***	0.016	0.0004	−0.025
	(0.022)	(0.019)	(0.014)	(0.016)
IHS(Ancestry 1990)		0.089***		
		(0.009)		
IHS(Ancestry 2000)			0.099***	
			(0.008)	
IHS(Ancestry 2010)				0.118***
				(0.011)
F-stat (Foreign-born 2010)	237.0	6.858	8.016	13.62
F-stat (Ancestry)	—	11.57	11.34	18.40
Dep. var. mean	0.019	0.019	0.019	0.019
Dep. var. sd	0.182	0.182	0.182	0.182
Observations	4,708,359	4,708,359	4,708,359	4,708,359
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. Only donations from donors with European-ethnicity names are included. 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. Standard errors are given in parentheses. Standard errors are clustered at the foreign country and domestic county levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Effect of exposure to Arab ancestry on donations toward Arab countries

	(1)	(2)	(3)
Panel A: IV	IHS(# donations)		
IHS(Arab ancestry)	0.388***	0.374***	0.401***
	(0.048)	(0.058)	(0.060)
AP F-statistic	9.703	7.590	6.580
Weak IV-robust p-value	< 0.01	< 0.01	< 0.01
Panel B: OLS	IHS(# donations)		
IHS(Arab ancestry)	0.027***	0.012***	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	150,048	150,048	150,048
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 from donors with European-ethnicity names 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Effect of exposure to 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**	0.072***	0.075***	0.067**	0.068**	0.055**	0.053**
	(0.006)	(0.017)	(0.027)	(0.026)	(0.028)	(0.024)	(0.025)
IHS(non-Euro ancestry)					−0.010		
					(0.019)		
Avg. race IAT score						0.028***	
						(0.005)	
2012 Rep. vote share							−0.130**
							(0.054)
AP F-statistic	—	12.39	9.814	6.528	6.373	6.567	6.241
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.05	< 0.05	< 0.05	< 0.05
Observations	108,535	108,535	107,399	107,399	107,399	107,399	107,399
Panel B:	*Warmth toward Arab-Muslims (std., higher score = more favorable)*						
IHS(Arab ancestry)	0.043***	0.154***	0.136***	0.109***	0.116***	0.089***	0.089***
	(0.008)	(0.029)	(0.033)	(0.031)	(0.029)	(0.028)	(0.031)
IHS(non-Euro ancestry)					−0.045**		
					(0.020)		
Avg. race IAT score						0.046***	
						(0.007)	
2012 Rep. vote share							−0.263***
							(0.073)
AP F-statistic	—	12.49	9.820	6.498	6.321	6.546	6.205
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	108,410	108,410	107,292	107,292	107,292	107,291	107,292
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. In Panels A and B, 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Effect of exposure to 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^{***}	-0.040^{*}	-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	10.46	5.480	5.245
Weak IV-robust p-value	—	< 0.01	< 0.10	< 0.10	< 0.05
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^{***}	-0.046^{**}	-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^{***}	0.508^{***}	0.511^{***}
	(0.033)	(0.043)	(0.032)	(0.035)	(0.033)
AP F-statistic	—	19.11	11.48	5.336	5.328
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.05	< 0.05
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 given in parentheses. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Effect of exposure to 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)
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
AP F-statistic	9.185	9.185	9.185	8.464	8.464
Weak IV-robust p-value	> 0.10	< 0.01	> 0.10	< 0.01	< 0.01
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)
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
AP F-statistic	8.464	8.464	8.464	8.053	8.053
Weak IV-robust p-value	> 0.10	< 0.01	< 0.01	< 0.01	< 0.01
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 $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\dots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Online Appendix

"The Immigrant Next Door"

Leonardo Bursztyn

Thomas Chaney

Tarek A. Hassan

Aakaash Rao

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 d (thousands)	4,708,359	0.236	9.448	0.000	0.000	2,629.375
2010 IHS-transformed population from country d	4,708,359	1.187	2.072	0.007	0.000	15.475
A.2: Donations, Charity 1 and 2						
IHS-transformed number of donations to country d	4,708,359	0.019	0.182	0.000	0.000	7.71
A.3: Donations, Charity 2 only						
IHS-transformed dollar value of donations to country d	3,976,506	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	150,048	0.048	0.296	0.000	0.000	6.397
Panel C: Individual level						
C.1: Project Implicit						
Arab-Muslim IAT score	108,535	0.016	0.990	0.002	-4.208	4.39
Warmth toward Arab-Muslims	108,410	0.033	0.995	-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	-0.087	1.003	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 A2). Donations statistics are calculated from the pooled donations across Charity 1 and Charity 2, and only donations from donors with European-ethnicity names are included.

Appendix Table A2: 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 $20,000	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 Table A3: Effect of ancestral exposure on donations, separated by charity

	(1) IHS(# donations)	(2) Donations (dummy)	(3) IHS($ donations)
Panel A: Charity 1			
IHS(Ancestry)	0.043***	0.017***	—
	(0.013)	(0.004)	—
First-stage F-statistic	52.95	52.95	—
Dep. var. mean	0.009	0.007	—
Dep. var. sd	0.128	0.082	—
Observations	2,195,559	2,195,559	—
Panel B: Charity 2			
IHS(Ancestry)	0.073**	0.036**	0.219**
	(0.031)	(0.015)	(0.092)
First-stage F-statistic	273.6	273.6	273.6
Dep. var. mean	0.013	0.010	0.052
Dep. var. sd	0.146	0.102	0.532
Observations	9,275,373	9,275,373	9,275,373
Foreign country × quarter FE	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes

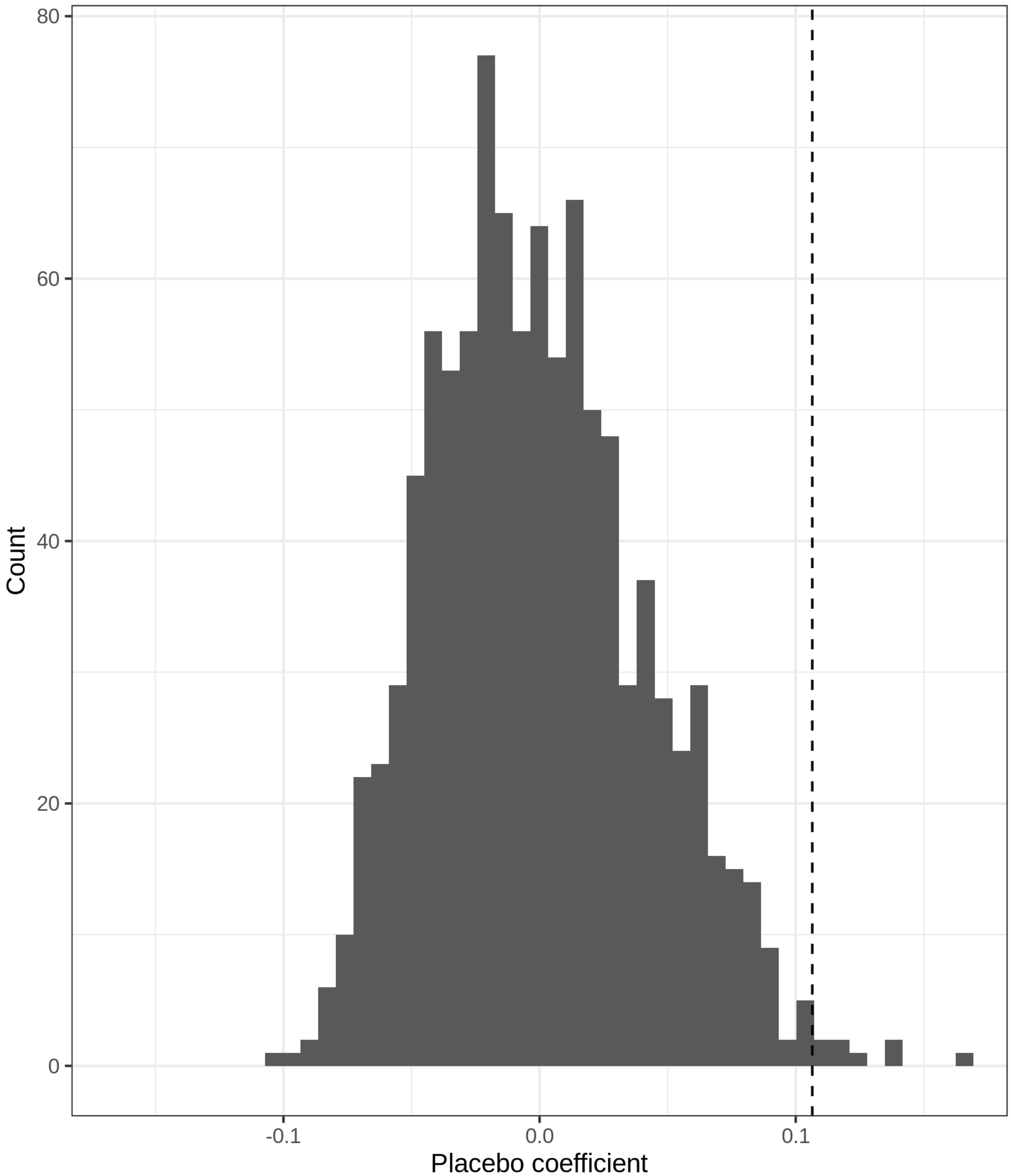
Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. The dependent variable in Column 1 is the IHS-transformed number of donations from county to country in a quarter. The dependent variable in Column 2 is a dummy for the presence of at least one donation 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 the year 2000 for Charity 1 (Panel A) and in the year 2010 for Charity 2 (Panel B). In both panels and all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2000}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments, additionally including $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=2010}$ for Charity 2 (Panel B). 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A4: Effect of ancestral exposure on donations, different choices of clustering

	(1) All countries	(2) Arab countries (pooled)
	IHS(# donations)	
IHS(Ancestry)	0.106	0.401
Robust SE	(0.004)	(0.018)
Clustering: Foreign country	(0.044)	—
Clustering: Domestic county	(0.009)	(0.060)
Clustering: Domestic state	(0.012)	(0.084)
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	4,708,359	150,048
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 1, donations are dropped when the first-best or second-best classification of their name's ethnicity matches the receiving country, and only donations from donors with European-ethnicity names are included. In Column 2, only donations to Arab League countries from donors with European-ethnicity names 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 d, while it is the IHS-transformed population with ancestry from Arab countries in Column 2. In both columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ 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. 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 A1: Effect of ancestral exposure on donations, permutation test



Notes: Figure A1 presents the results of a permutation test in which we permute ancestry and the excluded instruments, such that our regression estimates a average of the effect of exposure to one ancestral groups 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 A5: Effect of ancestral exposure on donations, excluding different countries and Census regions

	Dependent variable:			
	IHS(# donations)			
Panel A: Excluding Different Countries				
Countries excluded:	None	Arab	Latino	non-Arab African
IHS(Ancestry)	0.106**	0.082**	0.079**	0.266*
	(0.043)	(0.036)	(0.034)	(0.148)
Dep. var. mean	0.019	0.022	0.019	0.019
Dep. var. s.d.	0.182	0.198	0.182	0.183
First-stage F-statistic	331.3	350.8	729.7	157.0
Observations	4,708,359	3,609,009	4,300,029	3,027,924
Panel B: Excluding Different Census Regions				
Census region excluded:	Northeast	South	Midwest	West
IHS(Ancestry)	0.112**	0.111**	0.110**	0.089**
	(0.042)	(0.046)	(0.047)	(0.039)
Dep. var. mean	0.015	0.025	0.024	0.016
Dep. var. s.d.	0.162	0.208	0.204	0.165
First-stage F-statistic	265.8	254.1	319.5	330.4
Observations	4,383,076	2,572,284	3,126,914	4,042,803
Foreign country × quarter FE	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes	Yes

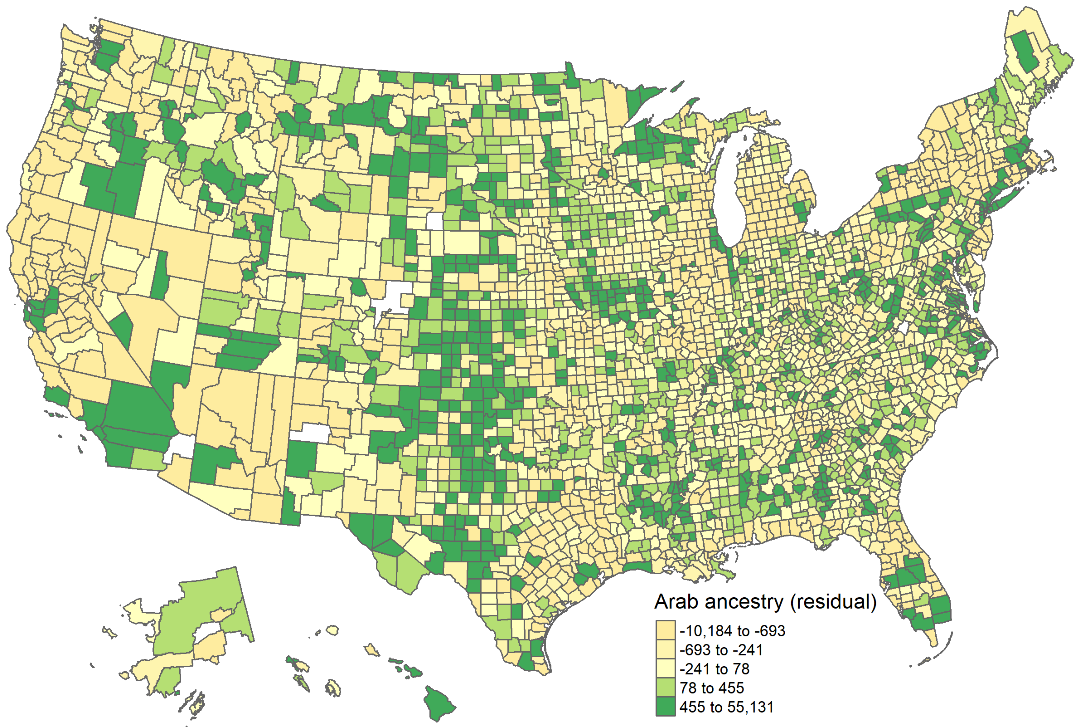
Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donations from donors with European-ethnicity names are included. The main variable of interest is the IHS-transformed population with ancestry from country d. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. In Panel A, Column 1 includes all countries; Column 2 excludes Arab League countries; Column 3 excludes Latin countries; and Column 4 excludes African countries that are not a part of the Arab League. In Panel B, Column 1 excludes the Northeast; Column 2 excludes the South; Column 3 excludes the Midwest; and Column 4 excludes 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A6: Effect of ancestral exposure on donations, percent functional form

	(1) Standard	(2) Standard	(3) Standard	(4) Standard	(5) Excl. corr. dest.	(6) Excl. corr. origins	(7) Eur. only pull
Percent country ancestry	0.020*	0.016**	0.017***	0.019**	0.016**	0.020*	0.022**
	(0.012)	(0.006)	(0.006)	(0.007)	(0.007)	(0.012)	(0.010)
First-stage F-statistic	241.8	252.8	283.2	284.7	150.1	338.8	235.6
Dep. var. mean	0.020	0.020	0.020	0.020	0.020	0.020	0.020
Dep. var. sd	0.472	0.472	0.472	0.472	0.472	0.472	0.472
Observations	4,701,998	4,699,466	4,699,466	4,701,998	4,701,998	4,701,998	4,701,998
Foreign country × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	No	Yes	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	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donations from donors with European-ethnicity names are included. 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 share of the county's 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 5 uses an alternative construction of the instrument that excludes counties with correlated migrant flows. Column 6 uses an alternative construction of the instrument that excludes countries with correlated migrant flows. Column 7 uses an alternative construction of the instrument that calculates the pull factor based only on European emigrants. Columns 1–3 control for log 2010 population. Columns 2–7 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

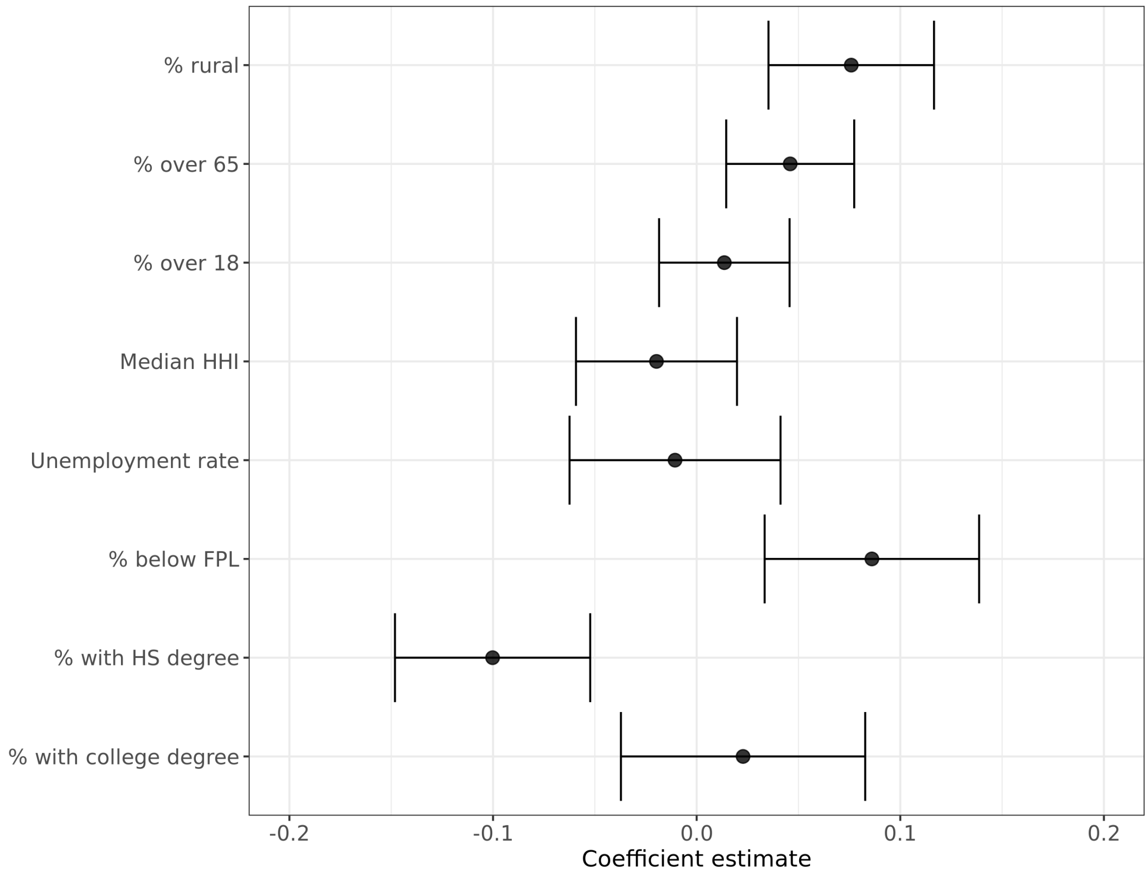
Appendix Figure A2: Residualized predicted values of Arab-Muslim ancestry



Notes: Figure A2 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 and log population.

Appendix Figure A3: Arab-ancestry population across counties

Appendix Figure A4: Balance test of Arab-Muslim instruments



Notes: Figure A4 presents coefficient estimates from regressions of a number of demographics 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. Standard errors are robust.

Appendix Table A7: Effect of exposure to 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^{**}	0.045^{**}	0.053^{**}
	(0.005)	(0.017)	(0.019)	(0.023)	(0.026)	(0.018)	(0.022)
IHS(non-Euro ancestry)					-0.023		
					(0.016)		
Avg. race IAT score						0.031^{***}	
						(0.004)	
2012 Rep. vote share							-0.122^{***}
							(0.044)
AP F-statistic	—	14.15	11.02	6.579	6.720	6.759	6.094
Weak IV-robust p-value	—	> 0.10	< 0.01	< 0.01	< 0.05	< 0.05	< 0.05
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^{***}	0.109^{***}	0.111^{***}	0.081^{***}	0.090^{***}
	(0.007)	(0.023)	(0.030)	(0.032)	(0.032)	(0.026)	(0.031)
IHS(non-Euro ancestry)					-0.045^{**}		
					(0.020)		
Avg. race IAT score						0.050^{***}	
						(0.005)	
2012 Rep. vote share							-0.255^{***}
							(0.060)
AP F-statistic	—	14.16	11.02	6.538	6.690	6.732	6.053
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 $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\dots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age $\times$ 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A8: Effect of exposure to 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	9.887	10.03	9.722
Weak IV-robust p-value	< 0.01	< 0.05	< 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 × male. Standard errors are given in parentheses. Standard errors are clustered at the congressional district level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A9: Effect of exposure to 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)	0.044*	0.012	0.083**	0.088**
	(0.026)	(0.030)	(0.037)	(0.042)
AP F-statistic	9.843	9.828	9.834	9.850
Weak IV-robust p-value	< 0.10	> 0.10	< 0.05	< 0.05
Panel B: OLS				
IHS(Arab ancestry)	0.023***	0.017***	0.035***	0.032***
	(0.005)	(0.005)	(0.007)	(0.007)
Observations	107,732	107,657	108,012	108,117
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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A10: Effect of exposure to Arab ancestry on attitudes toward different groups

	(1) Arab-Muslims	(2) Asians	(3) Blacks
Panel A:		*Score on IAT*	
IHS(Arab ancestry)	0.075***	0.034	0.018
	(0.027)	(0.028)	(0.017)
Age	−0.004**	0.008***	0.014***
	(0.002)	(0.002)	(0.001)
Age squared	−0.0001***	−0.0002***	−0.0002***
	(0.00002)	(0.00003)	(0.00001)
Male	−0.146***	−0.059***	−0.102***
	(0.018)	(0.019)	(0.006)
AP F-statistic	7.360	7.924	7.581
p-value (coef = Column 1 coef)	—	0.293	0.07
Observations	107,399	74,152	1,118,084
Panel B:		*Warmth*	
IHS(Arab ancestry)	0.136***	0.037	0.030**
	(0.033)	(0.028)	(0.015)
AP F-statistic	7.365	7.175	7.570
p-value (coef = Column 1 coef)	—	0.022	0.003
Observations	107,292	34,605	1,117,484
State FE	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variables in Panel A are the scores on the different IATs (from Project Implicit); the dependent variables in Panel B are the stated warmth toward different groups (also from Project Implicit). Column 1 explores attitudes toward Arab-Muslims, Column 2 — toward Asians, and Column 3 — toward Blacks. 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 $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\dots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Standard errors are given in parentheses. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A11: Effect of exposure to Arab ancestry on political preferences, indiviudal Romney control

	(1) OLS	(2) IV	(3) IV	(4) IV
	Voted for Trump in 2016			
IHS(Arab ancestry)	-0.012^{***}	-0.052^{***}	-0.061^{***}	-0.037^{*}
	(0.003)	(0.014)	(0.021)	(0.020)
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	10.22	5.476
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.10
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 $\times$ 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A12: 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(Arab ancestry)	0.075^{***}	0.136^{***}	-0.076^{***}	-0.073^{***}	0.129^{***}	0.377^{***}
	(0.027)	(0.033)	(0.024)	(0.027)	(0.038)	(0.103)
AP F-statistic	9.814	9.820	10.46	10.49	8.464	8.053
Weak IV-robust p-value	< 0.01	< 0.01	< 0.10	< 0.01	< 0.01	< 0.01
Panel B: Ancestry from Muslim Ban countries						
IHS(Ancestry from Muslim Ban countries)	0.060^{**}	0.125^{***}	-0.088^{***}	-0.069^{*}	0.096^{***}	0.132^{*}
	(0.027)	(0.034)	(0.030)	(0.037)	(0.035)	(0.080)
AP F-statistic	3.786	3.751	7.849	7.135	17.59	17.46
Weak IV-robust p-value	< 0.05	< 0.01	< 0.01	< 0.10	< 0.05	< 0.10
Panel C: Ancestry from Muslim-majority countries						
IHS(Muslim ancestry)	0.072^{***}	0.131^{***}	-0.074^{***}	-0.078^{***}	0.123^{***}	0.329^{***}
	(0.023)	(0.027)	(0.022)	(0.022)	(0.025)	(0.055)
AP F-statistic	14.81	14.87	14.00	14.60	15.08	15.52
Weak IV-robust p-value	< 0.01	> 0.10	> 0.10	< 0.01	< 0.01	< 0.01
Dep. var. mean	0.017	0.033	0.530	0.464	0.396	0.000
Dep. var. sd	0.990	0.995	0.499	0.499	0.489	1.000
Observations	107,399	107,292	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 variable in Column 1 is the score on the Arab-Muslim IAT (from Project Implicit); the dependent variable in Column 2 is the stated warmth toward Arab-Muslims (also from Project Implicit). 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 dependent variable in Column 3 is stated support for the Muslim Ban (from the CCSE); the dependent variable in Column 4 is self-reported Trump votership (also from the CCSE). The dependent variable in Column 5 is an indicator taking value one if any of the indicators for whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor take value one. The dependent variable in Column 6 is a normalized sum of three scaled to mean zero and standard deviation one values: the respondent's total score on the "pillars" question (ranging from 0 to 7), an indicator for whether the respondent correctly answered the Ramadan question, and 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 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. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A13: Effect of exposure to 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**	0.074***	0.177***	−0.129***	−0.151***	0.260**	0.592***
	(0.058)	(0.027)	(0.052)	(0.049)	(0.051)	(0.104)	(0.226)
Dep. var. mean	0.044	0.017	0.033	0.530	0.464	0.396	0.000
Dep. var. sd	0.533	0.990	0.995	0.499	0.499	0.489	1.000
Observations	150,048	107,399	107,292	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 IHS-transformed number of donations from the county to Arab League countries in a quarter. Only donations to Arab League countries from donors with European-ethnicity names are included. The dependent variable in Column 2 is the score on the Arab-Muslim IAT (from Project Implicit); the dependent variable in Column 3 is the stated warmth toward Arab-Muslims (also from Project Implicit). 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 dependent variable in Column 4 is stated support for the Muslim Ban (from the CCSE); the dependent variable in Column 5 is self-reported Trump votership (also from the CCSE). The dependent variable in Column 6 is an indicator taking value one if any of the indicators for whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor take value one. The dependent variable in Column 7 is a normalized sum of three scaled to mean zero and standard deviation one values: the respondent's total score on the "pillars" question (ranging from 0 to 7), an indicator for whether the respondent correctly answered the Ramadan question, and 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 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. Individual demographics include age, male, age squared, and age × male. Standard errors are given in parentheses. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A14: Stability of estimated effect of exposure to arab ancestry on attitudes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	*Score on Arab-Muslim IAT (std., higher score = less prejudiced)*					
IHS(Arab ancestry)	0.075***	0.072**	0.082***	0.090***	0.110***	0.090***
	(0.027)	(0.030)	(0.028)	(0.027)	(0.033)	(0.031)
% rural		−0.0002				0.0002
		(0.0004)				(0.0004)
% above 65			0.001			0.002
			(0.002)			(0.002)
% below poverty line				0.004***		0.005***
				(0.001)		(0.001)
% with HS degree					−0.002*	0.001
					(0.001)	(0.001)
Observations	107,399	107,399	107,399	107,399	107,399	107,399
Panel B:	*Warmth toward Arab-Muslims (std., higher score = more favorable)*					
IHS(Arab ancestry)	0.136***	0.095***	0.148***	0.159***	0.169***	0.118***
	(0.033)	(0.035)	(0.035)	(0.038)	(0.042)	(0.036)
% rural		−0.002***				−0.001**
		(0.001)				(0.0005)
% above 65			−0.001			0.0002
			(0.002)			(0.002)
% below poverty line				0.006***		0.009***
				(0.001)		(0.002)
% with HS degree					−0.001	0.005***
					(0.001)	(0.002)
Observations	107,292	107,292	107,292	107,292	107,292	107,292
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county level. 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. Standard errors are given in parentheses. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A15: Stability of estimated effect of exposure to arab ancestry on political preferences

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			*Voted for Trump in 2016*			
IHS(Arab ancestry)	-0.073^{***}	-0.041	-0.082^{***}	-0.105^{***}	-0.113^{***}	-0.074^{**}
	(0.027)	(0.029)	(0.028)	(0.029)	(0.036)	(0.032)
% rural		0.001^{***}				0.001^{*}
		(0.0003)				(0.0003)
% above 65			0.001			0.0001
			(0.001)			(0.001)
% below poverty line				-0.004^{***}		-0.009^{***}
				(0.001)		(0.001)
% with HS degree					0.0001	-0.006^{***}
					(0.001)	(0.001)
Dep. var. mean	0.464	0.464	0.464	0.464	0.464	0.464
Dep. var. sd	0.499	0.499	0.499	0.499	0.499	0.499
Observations	97,576	97,576	97,576	97,576	97,576	97,576
Panel B:			*Support for the Muslim Ban*			
IHS(Arab ancestry)	-0.076^{***}	-0.043^{*}	-0.084^{***}	-0.096^{***}	-0.090^{**}	-0.062^{*}
	(0.024)	(0.026)	(0.025)	(0.029)	(0.036)	(0.033)
% rural		0.001^{***}				0.001^{**}
		(0.0003)				(0.0003)
% above 65			0.001			0.001
			(0.001)			(0.001)
% below poverty line				-0.003^{***}		-0.008^{***}
				(0.001)		(0.001)
% with HS degree					-0.002	-0.007^{***}
					(0.001)	(0.001)
Dep. var. mean	0.530	0.530	0.530	0.530	0.530	0.530
Dep. var. sd	0.499	0.499	0.499	0.499	0.499	0.499
Observations	56,837	56,837	56,837	56,837	56,837	56,837
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county level. 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. Standard errors are given in parentheses. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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.[38] References to distance as a control include both distance and latitude difference.

[38] 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 validate the results of NamSor's name classification procedure using a random 250,000 person sample from the North Carolina Voter Registration Data[39], which contains registrants' first and last names alongside self-reported ethnicity (Asian, Black/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 the country to which they are donating, we are primarily concerned with classification errors of the type (Reports Other, Classified as European/Black). 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

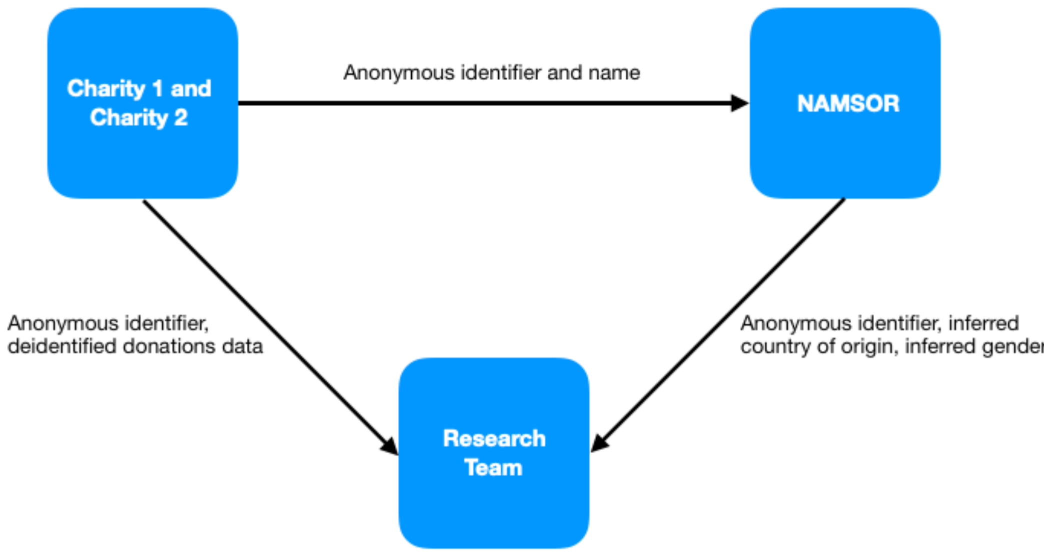
5. The research team uses the donation ID to match up the donation data from the charitable organization and the origin country data from NamSor

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

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

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!