The Immigrant Next Door: Exposure, Prejudice, and Altruism*

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February 2021

Abstract

We study how decades-long exposure to individuals of a given foreign descent shapes natives' attitudes and behavior toward that group, exploiting plausibly exogenous shocks to the ancestral composition of US counties. We combine several existing large-scale surveys, cross-county data on implicit prejudice, a newly-collected national survey, and individualized donations data from large charitable organizations. We first show that greater long-term exposure to Arab-Muslims: i) decreases both explicit and implicit prejudice against Arab-Muslims, ii) reduces support for policies and political candidates hostile toward Arab-Muslims, iii) increases charitable donations to Arab countries, iv) leads to more personal contact with Arab-Muslim individuals, and v) increases knowledge of Arab-Muslims and Islam in general. We then generalize our analysis, showing that exposure to *any* given foreign ancestry leads to more altruistic behavior toward that group.

JEL Classification: D83, D91, P16, J15

Keywords: contact, attitudes, immigration, prejudice, altruism

*We thank Isaiah Andrews, Michela Carlana, Ray Fisman, Ross Mattheis, Nathan Nunn, Kevin Lang, Ricardo Perez-Truglia, Gautam Rao, Chris Roth, Andrei Shleifer, Marco Tabellini, Romain Wacziarg, David Yang, and numerous seminar participants for helpful comments and suggestions. We thank Andrew Kao, Andrei Kim, Lisa Tarquinio, Thomas Yu, and Alison Zhao for outstanding research assistance. We are grateful for financial support from the Sloan Foundation. Chaney is grateful for financial support from the European Research Council (ERC grants N°337272 and N°884847). All remaining mistakes are our own.

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1 Introduction

What is the impact of decades-long exposure to individuals of a given foreign ancestry on natives' attitudes and behavior toward that group? In this paper, we investigate this question using cross-county data from the United States and an identification strategy exploiting the confluence of "push" and "pull" factors in historical migrations to generate plausibly exogenous variation in the ancestral composition of US counties. Our findings indicate that long-term exposure to a given ethnic group reduces explicit and implicit prejudice against that group, reduces support for policies hostile toward that group, and increases altruism toward that group's ancestral country.

We begin our analysis with one of the most targeted minorities in the recent surge of nationalist authoritarianism in the United States: Arab-Muslims. 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 received substantial attention in the policy debate (Beydoun, 2018).[1] For example, President Trump floated the idea of implementing a registry of Muslims entering the country (though his campaign later walked back these comments), and in 2017, the administration issued a series of executive orders banning travelers from several Arab-Muslim countries from entering the country.[2] 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.[3]

In the first part of our analysis, we investigate how long-term exposure to people of Arab-Muslim ancestry affects the attitudes of the "majority group" — White, non-Muslim Americans — toward Arab-Muslims. Using two large, cross-county datasets, we find that exposure leads to more positive *attitudes*, as measured by both explicit questions and the Implicit Association Test (IAT): White, non-Muslim respondents who reside in US counties with (exogenously) larger populations of Arab ancestry are less explicitly and implicitly prejudiced against Arab-Muslims.[4] Having documented

[1] We plot the distribution of Arab-ancestry individuals across US counties in Appendix Figure A2.

[2] See, for example, "Protecting the Nation from Foreign Terrorist Entry into the United States", *federalregister.gov*, Jan 27, 2017; Timeline: Legal fight over Trump's 'Muslim ban' and the Supreme Court ruling, *Chicago Tribune*, Jun 26, 2018.

[3] We focus on Arab-Muslims for three additional reasons. First, it allows us to use several large-scale datasets, including Implicit Association Test (IAT) data, which specifically elicit attitudes toward Arab-Muslims and allow us to provide evidence on a wide range of outcomes and mechanisms. Second, Arab-Muslims represent a relatively well-defined and easily observable ancestral group, in contrast to, for example, Latinos. Finally, insofar as it leverages migration flows leaving out the group of interest, our identification strategy is most convincing when applied to relatively small groups.

[4] This finding is robust to controlling for a set of observable characteristics of test-takers and the counties in which they live. We also show that measured prejudice against Arab-Muslims does not simply proxy for more or less racial prejudice in general or more or less conservative political views: controlling for the overall racial prejudice of respondents in a county does not significantly affect the estimated impact of exposure to Arab-Muslim neighbors on prejudice against Arab-Muslims, nor does controlling for the Republican vote share in 2008 and 2012.

these effects on *attitudes*, we then show that they carry over into measures of *political preferences*: non-Muslim Whites in counties with greater exposure to people of Arab ancestry are less supportive of the "Muslim Ban" restricting travelers from Iran, Iraq, Libya, Somalia, Sudan, Syria, and Yemen. In 2016, they were also less likely to vote for Donald Trump, who aggressively promoted legislation targeting Arab-Muslims during his campaign.[5] These results hold even when we control for county-level voting behavior in 2012, suggesting that exposure not only makes voters less likely to support conservative policies in general, but also decreases support for anti-Muslim policies in particular.

We next turn toward a *revealed preferences* measure of generosity toward Arab-Muslims. Using large-scale individualized datasets from two charity organizations, we document that individuals from counties with (exogenously) larger populations of Arab ancestry are more likely to donate, and donate larger sums, to charitable causes in Arab countries.[6] Importantly, we can remove from our sample donors with Arab names, ensuring that our estimates are indeed capturing individuals of non-Arab ancestry donating to Arab countries.[7]

We conclude our analysis of Arab-Muslims by shedding light on the mechanisms underlying our estimated effects. In December 2020, we conducted a large-scale custom survey to measure two potential mechanisms: 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. While neither mutually exclusive nor jointly exhaustive, these mechanisms may be important in determining treatment effects on attitudes and behavior. The results of our survey analysis indicate that an (exogenously) larger Arab-Muslim population in a respondent's county substantially increases both the probability that the respondent has visited a Middle Eastern restaurant and the probability that the respondent knows 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 probability that respondents believe that "holy war against non-believers" or the "subservience of women and children to men" are among the fundamental tenets of the faith.

We then expand our analysis beyond the context of Arab-Muslims: *in general*, does exposure to a local population of a given foreign ancestry increase generosity toward that country? We show it does. We exploit the bilateral (dyadic) structure of our donations data, with donations flowing

[5]See, for example, "'I think Islam hates us': A timeline of Trump's comments about Islam and Muslims", *The Washington Post*, May 20, 2017.

[6]Every Arab country is predominantly Muslim, so we do not refer to them as "Arab-Muslim" countries.

[7]We follow a strict protocol to protect donor anonymity; importantly, we never directly observe donors' names or other personally identifying information. See Appendix D for details.

from many US counties to many different foreign countries. This allows us to include origin county and destination country fixed effects, ruling out that our estimates are driven solely by particular immigrant groups making residents more altruistic in general or immigrants making residents more altruistic toward particular countries. Our estimated effects of exposure operate on both the extensive and the intensive margin of donations: for example, White residents of counties with an exogenously larger Haitian minority are more likely to donate to causes in Haiti (relative to their donations to other countries in general) while White residents of counties with an exogenously larger Dominican minority are more likely to donate to causes in the Dominican Republic (relative to their donations to other countries in general). The estimated effect is positive for all recipient countries across our two datasets. Moreover, the positive effect of exposure is stronger for ancestries that are genetically more distant, which we interpret as suggestive evidence that the effects of contact may be largest for populations that look most visibly "foreign."

Identifying a long-term effect of exposure to outside groups on beliefs about and behavior towards those groups, particularly at a more aggregate level, presents two primary challenges to identification. First, immigrants from a given country may disproportionately migrate to counties that are more tolerant toward that country or toward foreigners in general. To address this selection concern, we build on the approach from Burchardi, Chaney, and Hassan (2019a) to isolate quasi-random variation in the ancestral composition of present-day US counties stemming exclusively from the historical interaction of two forces: (i) time-series variation in the relative attractiveness of different destinations within the United States for the average migrant arriving at the time and (ii) the staggered arrival of migrants from different origins. Taken together, the interaction of these two historical forces allows us to identify variation in the composition of foreign ancestry inherited from plausibly exogenous shocks to historical migrations to the United States going back as far as 1880. That is, we leverage plausibly exogenous variation in historical migrations to a US county — a flow variable — to isolate quasi-random variation in the present-day ethnic composition of that county — a stock variable. In this sense, our analysis is focused on the long-term impact of ancestral composition on attitudes and behavior towards ethnic minorities, not on the short-term impact of the most recent migrations. Our contribution is therefore not to develop a new identification strategy, but rather to build upon an existing approach (subsequently used in other work, such as Arkolakis et al. 2020) in order to investigate the long-term effects of exposure to members of particular foreign ancestral groups on natives' attitudes, political preferences, and altruism toward those groups. More fundamentally, our findings shed light on how preferences are shaped by social interactions.

Second, even when conditioning on quasi-random variation in the ancestry 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. For example, one type of White American may dislike Arabs and like Haitians, while another type might have the reverse (selectively bigoted) preference. In this sense, the composition of the White Americans in a given county might respond endogenously to the arrival of specific minorities through out-migration of natives, even if immigration is plausibly exogenous. We show that none of our results are attributable to such "selective White flight". Using thirty years of detailed census data, we show that White Americans who leave a county with a large community descended from a given foreign origin are not disproportionately likely to move to places with a small community from that same origin. Taking the evidence together, we conclude that the more positive explicit and implicit attitudes, the lower support for anti-immigrant political policies, and the greater altruism of residents towards ethnic groups to which they are disproportionately exposed (but do not themselves belong) are driven by the long-term presence of that ethnic group itself.

We add three primary caveats to this interpretation. First, our most tightly identified results speak to relative differences in attitudes and generosity towards different ethnicities, not to the overall (average) level of bigotry in a given location. Second, 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 is highly complex and deserves careful, separate, attention. Third, our focus is on the types of long-run effects of the presence of foreign ethnicities 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 bias, 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 periods of time stretching decades.

Related literature Our paper contributes to a large literature in sociology, social psychology, and economics 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 work in this literature takes the form of randomized experiments.[8]

[8]See, for example, 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)

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).[9]

One important theme in this literature is persistence. Schindler and Westcott (2020) study the deployment of African-American soldiers in the United Kingdom during World War II, finding that residents of locations with more soldiers deployed remained less explicitly and implicitly prejudiced against minorities over sixty years later. Some studies (Bazzi et al., 2019; Bagues and Roth, 2020) similarly find that the effects of contact persist over long periods, while others (e.g. Dahl et al., 2020) find that effects fade out relatively quickly. Recent work has also explored *heterogeneity*: contact may lead to more positive social preferences in some contexts, but have no effects or even negative effects in others. For example, Lowe (2020) and Mousa (2020) randomize the composition of sports teams: although both find that cooperative contact (playing on the same team) leads to more positive social preferences, Lowe (2020) finds that adversarial contact (playing on an opposing team) has the opposite effect, and Mousa (2020) finds that these more positive social preferences do not translate to contexts beyond the sports pitch. Bazzi et al. (2019), which exploits a population resettlement program to identify the long-run effects of intergroup contact on national integration in Indonesia, finds 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, an crucial class of remaining questions 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, how does this exposure translate into real-world outcomes, and to what extent are these effects consistent across different out-groups? Our identification strategy and data afford us unique insight into these questions. We 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 the course of decades — and we generalize our results to examine exposure to over one hundred ancestral groups.

Our paper also complements a growing body of work on the relationship between immigration and political attitudes and voting behavior. Some recent papers have found evidence that higher immigration flows lead to stronger support for right-wing parties (see, for example, Barone et al.,

find that there are no randomized studies that show the effects of interracial and interethnic contact on adults over the age of 25, and there are only three such studies that quantify the effects more than a single day after treatment.

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

2016; Halla et al., 2017; Dustmann et al., 2019; Brunner and Kuhn, 2018; Becker and Fetzer, 2016), while other work has found evidence going in the opposite direction (see Dill, 2013; Steinmayr, 2016). Tabellini (2020) uses historical data to show that increased immigration to US counties led to the election of more conservative legislators, higher support for anti-immigration legislation, and lower redistribution — despite the economic benefits immigrants generate for non-immigrants, as also documented in Sequeira et al. (2020), Burchardi et al. (2019b), Kerr and Kerr (2016), and Arkolakis et al. (2020). 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. (2018b) experimentally find that priming subjects to think of immigration lowers support for redistribution. Though right-wing voting is often associated with negative views toward out-groups (and especially so with the recent surge of nationalist populism), comparing right-wing platforms across countries reveals substantial heterogeneity along economic, social, and political dimensions: 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 also by extending the results to dozens of different nationalities. More generally, there are several reasons to think that 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 results in more positive attitudes and political preferences and greater altruism toward the out-group.[10]

Recent contributions in economics have used Implicit Association Test (IAT) scores as a *predictor* of biased behaviors.[11] 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 mathematical performance.[12] Our work instead uses the implicit attitudes as an *outcome* and provides

[10] Fouka et al. (2020b) finds that the Great Migration, which led millions of African-Americans to migrate out of the rural South, improved Whites' views of immigrants and facilitated social integration of European immigrant groups. In the same context, Derenoncourt (2019) finds that migration of African-Americans increased police spending, crime, and incarceration in destination counties. Similarly, Fouka et al. (2020a) find that Mexican immigration improves Whites' attitudes and behavior towards blacks.

[11] 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 2.2.

[12] Alesina et al. (2018a) find evidence that informing teachers of their implicit bias against immigrants increases the grades they assigned to immigrants.

novel evidence that implicit bias can be shaped by long-term exposure to out-groups, complementing recent work in other contexts (e.g. Lowes et al. 2015, 2017; Schindler and Westcott 2020). Moreover, our findings also provide additional validation of IAT scores as a measure of bias, given the robust county-level correlation we observe between IAT scores and measures of explicit bias and between IAT scores and revealed altruism.

Finally, our work also contributes to an extensive literature on cultural persistence and change (see, for example, Alesina et al., 2013; Grosjean and Khattar, 2019; Giuliano and Nunn, 2017) by showing that local exposure changes long-term attitudes toward out-groups.[13] This relates 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 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 discusses our econometric approach. In Section 4, we show that exposure to a local population of Arab ancestry reduces both explicit and implicit prejudice against Arabs, reduces support for political policies targeting Arab-Muslims, increases altruism toward Arab countries, increases direct contact with people of Arab ancestry, and increases knowledge of Arab-Muslims and Islam. In Section 5, we generalize our results to show that exposure to a local population of *any* foreign ancestry increases altruism toward that ancestry, we probe the robustness of our results, and we explore heterogeneity. Section 6 concludes.

2 Data

We collect several series of data broadly corresponding to exposure, prejudice, hostility, and altruism. Throughout the analysis, we denote domestic US counties by d and foreign countries by f. In analyses with individual-level data (all of which are cross-sectional), our variables are therefore generically defined as $X_{i,d,f}$, denoting the outcome X pertaining to foreign country f of individual i residing in domestic county d. In some instances, f refers not to a single country but to a group of foreign countries: countries in the Arab League.[14] In analyses with county-level data, our variables are generically defined as $X^t_{d,f}$, denoting outcome X pertaining to foreign country f, measured at time t

[13] 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.

[14] The Arab League consists of Algeria, Bahrain, Comoros, Djibouti, Egypt, Iraq, Jordan, Kuwait, Lebanon, Libya, Mauritania, Morocco, Oman, Palestine, Qatar, Saudi Arabia, Somalia, Sudan, Tunisia, the United Arab Emirates, and Yemen. Syria was suspended from the Arab League in 2011; we nonetheless include Syria given that our latest Census data dates to 2010. All of these countries are majority Muslim.

in domestic US county d.

2.1 Historical Migrations, Ancestry, and Exposure

To quantify long-term exposure to members of a given ethnicity, we collect data on the historical ancestral composition of US counties. We assume implicitly 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. In order to isolate plausibly exogenous variation in the composition of local ancestry (see Section 3.2), we also use data on historical migrations. We follow Burchardi et al. (2019a) 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). We weight observations using the personal weight from these data providers. We provide additional details on these datasets in Appendix B.1.

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). For the initial 1880 census, which did not report the immigration date, we measure instead the total number of respondents in d who were either born in f or whose parents were born in f. Starting in 1980, respondents are also asked about their primary ancestry in both the US Census and the ACS. Our stock ancestry variable, $Ancestry^t_{f,d}$ corresponds to the number of respondents in d at t who report ancestry from f. The resulting dataset covers 3,141 domestic US counties, 195 foreign countries, and 10 census waves.

2.2 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 website run by Harvard University researchers through which respondents can complete Implicit Association Tests (IATs) quantifying implicit prejudice against different groups.[15] IAT scores are generally regarded as difficult to manipulate (Egloff and Schmukle, 2002),

[15]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, White 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 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.

and a number of studies have correlated these scores with real-world psychological responses and decision-making (Bertrand et al., 2005). For example, IAT scores are linked to employment callback decisions for minorities (Rooth, 2011), voting behavior (Friese et al., 2007), the gender gap in math performance (Carlana, 2019), and discrimination in the workforce (Glover et al., 2017).[16]

After completing the IAT, subjects are asked to respond to a number of additional questions, including an "Arab-Muslim Thermology" question which asks subjects to rate their feelings towards Arab-Muslims on a scale of 0 ("very cold") to 10 ("very warm"). This is the first question on explicit attitudes that is asked and the most directly relevant to our investigation, so we use this as our first measure of explicit attitudes.[17]

We make use of data from all Arab-Muslim and Race (Black/White) tests taken by July 1, 2020 (we use the Race IAT as a county-level control).[18] Subjects taking the IAT indicate their race and the reason for which they are taking the test. In order to assuage concerns about respondents endogenously selecting into taking the IAT, we classify those taking the test as an "Assignment for work," an "Assignment for discussion group," or an "Assignment for school" as "forced respondents" and conduct our primary analyses with the 58,987 White, non-Muslim respondents to the Arab-Muslim IAT in this subsample (though our results, displayed in the Appendix, are qualitatively unchanged and quantitatively similar if we also include the additional 80,179 "unforced respondents").

Although a wide range of institutions, from law firms to tech companies to police forces to schools and universities,[19] administer the IAT as part of diversity trainings and other initiatives, limiting the sample to "forced respondents" does not eliminate selection concerns entirely: the type of institution that requires people to take the IAT or the type or number of people associated with such institutions may be endogenous to the Arab-Muslim population.[20] Thus, 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 in partnership with the University of California, Los An-

[16]However, the IAT has also drawn criticism on multiple dimensions (see, for instance, Clayton et al. 2020; Schimmack 2019.) Blanton et al. (2007) argue that the mapping from the continuous IAT score to discrete qualitative feedback (e.g. "This respondent has a slight preference for European-Americans over Arab-Muslims") is arbitrary and has little basis in empirical evidence. Our analysis uses only continuous IAT scores, thus circumventing this concern. Cunningham et al. (2001) document that the IAT exhibits substantial measurement error, with even the same subject performing differently in subsequent repetitions of the test. Because our analysis uses IAT scores as an outcome, rather than as a predictor, classical measurement error will not bias our estimates.

[17]There are four other questions measuring explicit attitudes toward Arab-Muslims and social norms surrounding treatment of Arab-Muslims; we show estimates using these alternative outcomes in the Appendix.

[18]See `https://implicit.harvard.edu`. Last accessed: December 14, 2020.

[19]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.

[20]For this to generate a positive bias, we would require that IAT respondents in counties with a large Arab-Muslim population are more tolerant toward Arab-Muslims relative to others in the county, while respondents in counties with a small Arab-Muslim population are less tolerant toward Arab-Muslims relative to others in the population.

geles. Nationscape was fielded online to over 300,000 respondents between July 2019 and July 2020 and is broadly representative of the US population in terms of 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. Our second measure of explicit prejudice is the average response among respondents in a given county to the following question: "Here are the names of some groups that are in the news from time to time. How favorable is your impression of each group or haven't you heard enough to say... Muslims." 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. Unfortunately, the Democracy Fund Voter Study Group does not make individuals' county-level identifiers publicly available; the most granular available geographical identifier is congressional district c (of which there are 435). Because our instrument is at the county level d, we proceed by duplicating observations and assigning 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 we cluster standard errors at the district rather than the county level. We again restrict the sample to White, non-Muslim respondents.

For ease of comparability, we normalize all three measures — implicit bias against Arab-Muslims (Project Implicit), warmth toward Arab-Muslims (Project Implicit), and favorability toward Muslims (Nationscape) — to mean zero and standard deviation one, with higher values representing more positive attitudes.

2.3 Political Preferences

We assess how exposure to Arab-Muslims shapes political preferences using two distinct outcomes. First, we examine the effect of exposure to individuals of Arab-Muslim ancestry on support for the "Muslim Ban". During his 2016 presidential campaign, Donald Trump repeatedly called for a "total and complete" ban on Muslims entering the country. Among Trump's first executive orders upon entering office in January 2017 was Executive Order 13769, "Protecting the Nation From Foreign Terrorist Entry Into the United States," which severely restricted travel from Iran, Iraq, Libya, Somalia, Sudan, Syria, and Yemen. Although it was not officially framed as 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.

We use two datasets to assess support for the Muslim Ban. Nationscape, described in Section 2.2, asks participants to indicate whether they agree or disagree with the policy to "Ban people from predominately Muslim countries from entering the United States". As a second measure, we use the Cooperative Congressional Election Study (CCES), a widely-used representative stratified survey tracking public opinion and political attitudes fielded annually by YouGov. The 2017 and 2018 waves include a question on the executive order: respondents are asked to indicate whether they support or oppose the order, which they are told "bans immigrants from Iran, Somalia, Sudan, Yemen, Syria, and Libya from coming to the United States for 90 days" and "permanently prohibits Syrian refugees from entering the country". As before, for all outcomes, we restrict the sample to White, non-Muslim respondents.

As our second measure of political preferences, we study voting behavior in the 2016 Presidential Election. Both Nationscape and CCES ask participants to indicate whom they voted for in 2016; CCES asks this question in every year since 2016. Even 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. 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.[21] We thus attribute at least part of the Republican vote share in the 2016 election as an indication of hostility toward Arab-Muslims. We of course recognize that a ballot cast for candidate Trump is not *just* a ballot against Arab-Muslims, so we systematically control for other predictors of the Republican voting. Most importantly, we control for Republican voting in 2012, during which anti-Muslim sentiment was arguably a less salient campaign issue.

2.4 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 ($n = 6536$) is broadly representative of the targeted population in terms of age, gender, income, Hispanic ethnicity, and education (Appendix Table A4). We include the survey questionnaire in Appendix E.

In addition to eliciting demographics, voting behavior in 2012, 2016, and 2020, and county of

[21] 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

residence, we survey respondents about their *contact* with Arab-Muslims and about their *knowledge* of Arab-Muslims and Islam in general. To measure contact, we ask respondents to indicate whether they have interacted with Arab-Muslims in any of three capacities: as friends, as neighbors, and as workplace acquaintances. To measure knowledge of Arab-Muslims, we ask three questions. First, we ask respondents to select the correct definition of Ramadan among one correct and three incorrect definitions. Second, we ask a multiple-choice, multiple-response question asking them to highlight 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). As an auxiliary measure of exposure, we ask respondents whether they have ever dined in a Middle Eastern restaurant.

In our analysis, we report specifications using each of the three contact-related questions separately, and we also report specifications that instead estimate effects on a single indicator of contact taking value one if the respondent reported interacting with Arab-Muslims in *any* of the three capacities. Similarly, we report specifications using each of the three knowledge-related questions separately, and we also report specifications that estimate effects on an index of knowledge about Arab-Muslims: we construct this index by scaling each of the three knowledge questions to mean zero and standard deviation one, summing the scaled values, and dividing the sum by three (such that the resulting index has mean zero and standard deviation one).[22]

2.5 Charitable Donations and Altruism

To measure *altruism* 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).[23] While both organizations sometimes donate to US based causes, they primarily 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 who we are unable to match to a unique county of residence, we are left with 80,584 individual donations spanning from 2004 to 2017 for Charity 1 and 715,663 individual donations spanning from 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.

[22] Results are broadly similar if we instead define the index as the first principal component of the three answers.

[23] Charity 1 requested anonymity. Charity 2 is GlobalGiving (https://www.globalgiving.org).

For both charitable organizations, we construct a panel dataset of donations at the county-country-quarter level. We begin by removing any donation to foreign country f by an individual who, based on their first and last name, is likely of ancestry from f. This ensures that we are not identifying a natural tendency of individuals of foreign origin to donate to their ancestral country.[24] 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 White Americans are matched to European countries.[25] In some specifications, we focus on donations from donors of likely European origin (as all donations in our dataset go towards countries outside of Europe), or on donations made by donors with likely ancestral origin from a continent other than the continent receiving the donation. We then aggregate donations at the county d $\times$ foreign country f $\times$ quarter t level.

Figures A1 and 1 map the US distribution of donors and the worldwide distribution of the receiving countries for Charity 1 and Charity 2, respectively. Both figures show significant variation in the total number of donations across counties within the US and across foreign countries, with a substantially wider sample of destination countries for Charity 2 donations.

2.6 Other Data

Finally, we use demographic data from several sources. 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 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.[26] References to distance as a control include both distance and latitude difference.

[24] To identify the likely foreign 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. To ensure donors' privacy, individual donor names are never revealed to us researchers, and details about donations are never revealed to NamSor. Instead, we follow a three-way protocol such that NamSor sees only a list of names, but no information on donations; the charitable organization provides us with de-identified donations data; and we see only anonymized donations data and NamSor's classifications. See Appendix D for details.

[25] For example, "Kenneth Arrow" is matched to the United Kingdom.

[26] 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.

We source data for genetic, cultural, and linguistic distance from Spolaore and Wacziarg (2016). A higher value for each of these indices corresponds to a greater degree of separation between the United States and the given country.

2.7 Summary Statistics

We provide summary statistics in Appendix Table A1. To build intuition about the magnitudes of the estimated coefficients in the first section of our empirical analyses, we plot counties by their IHS-transformed Arab population in Appendix Figure A2.

3 Econometric Specification

3.1 Main Specification

Our aim is to estimate the causal impact of exposure to a local population of foreign ancestry on outcomes relevant to these ancestries: prejudice, political preferences, and charitable donations.

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 or a group of foreign countries f, $IHS\left(\text{Ancestry}_{d,f}\right)$.[27] We always control for logged county population to ensure that our estimates do not simply capture differences between small and large counties. This functional form places an emphasis on the *absolute size* of the community with ancestry from f, capturing the intuition that what may matter for changing the social preferences of residents in d toward group f is a "critical mass" from group f. For example, a large enough population with ancestry from a given country supports grocery stores, restaurants, cultural events and centers, etc. However, one might instead think that the *share* of the population in county d with ancestry from f 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, we replicate all of our analyses using ancestral shares, rather than IHS-transformed ancestral population in Appendix C. With one exception, which we flag below, our conclusions are qualitatively identical, and almost all coefficients which are statistically significant in the main analyses are statistically significant when estimated through this alternative approach.[28]

In our main analyses based upon survey and IAT data, we estimate the effect of exposure at the

[27] The inverse hyperbolic sine (IHS) approximates the natural logarithm function, but is well defined at zero.

[28] Because our focus is mostly on small minorities, such as Arab-Muslims, it is unsurprising that the two approaches yield very similar results.

individual level, running various specifications of the following equation:

$$Y_{i,d,f} = \beta IHS\left(\text{Ancestry}_{d,f}\right) + \text{Controls}_{i,d,f} + \epsilon_{i,d,f}. \tag{1}$$

Our outcomes, $Y_{i,d,f}$, include explicit and implicit prejudice against Arab-Muslims, preferences for policies targeting Arab-Muslims, Trump voting in 2016, and various measures of contact with and knowledge about Arab-Muslims. In these specifications, f refers collectively to Arab-Muslims (see Section 4).

In our main analyses based upon donations data, we estimate this effect at the origin county $\times$ destination country $\times$ quarter level, running various specifications of the form:

$$Y_{d,f}^{t} = \beta IHS\left(\text{Ancestry}_{d,f}^{t}\right) + \delta_d + \delta_f + \delta_t + \text{Controls}_{d,f}^{t} + \epsilon_{d,f}^{t}. \tag{2}$$

Our measures of donations take the form $\mathbb{1}[\text{Donations}_{d,f}^{t} > 0]$, an indicator taking value one if this number is nonzero; $IHS\left(\#\text{Donations}_{d,f}^{t}\right)$, the number of donations from residents in county d to country f in period t; and $IHS\left(\$\text{Donations}_{d,f}^{t}\right)$, the total dollar value of these donations. Whenever possible, we include a set of fixed effects for domestic county d (δ_d), foreign country or group of countries f (δ_f), and time periods (δ_t), as well as various $d \times f$ controls. In specifications with IHS transforms on both sides, the coefficient β in (2) approximates the elasticity of donations with respect to ancestry.

We may be worried that unobserved factors affect both the existing stock of ancestry and our measures of prejudice, political preferences, and altruism. For instance, it is possible that foreign migrants endogenously prefer settlement in US counties that are and always have been more tolerant towards foreigners, generating a correlation between $\text{Ancestry}_{d,f}$ and our outcome variables even in the absence of a causal effect of exposure on the outcomes we study. Controlling for a county d fixed effect (δ_d) partly addresses this concern, as it controls for the overall level of tolerance towards all foreigners. Yet we are not always able to control for county fixed effects: in some specifications, we consider a single group of foreign countries (e.g. Arab-Muslim countries). Second, it remains possible that some counties are tolerant toward some specific foreign origins, but not others. To address this concern, we construct an instrument for the present-day ancestry composition of US counties and, in addition, test explicitly whether Whites may be moving between counties to avoid specific minorities which they happen to dislike.

3.2 Isolating Exogenous Variations in Foreign Ancestry

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. (2019a).[29] This model purposefully excludes any determinant of migration correlated with the endogenous response of foreign migrants to natives' attitudes towards specific foreign groups (such as prejudice, hostility, or altruism toward specific groups), thus ensuring that the migrations isolated by our instruments are driven solely by factors that are plausibly independent from unobservables affecting our outcomes of interest.

In this model, the allocation of foreign migrants across domestic counties over time 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.[30] 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 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.[31] 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 of migrants and their descendants from f. We label this third source of variation a 'social pull factor,' which varies across domestic counties d, across foreign countries f, and over time t. 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 country at t, *and* d hosts a large preexisting stock with ancestry from f. Finally, we use the fact that the preexisting stock of ancestries at any time is itself inherited from previous migration waves in earlier periods. Iterating our model forward then allows us isolate (exogenous) variation in the distribution of ancestries which results purely from the historical interaction of economic push and pull factors. Burchardi et al. (2019a) show the first-stage expression for the contemporaneous stock of residents in domestic county d with ancestry from foreign country

[29] Variants of this approach have since been employed by Burchardi et al. (2019b) and Arkolakis et al. (2020), among others.

[30] To further alleviate endogeneity concerns, we leave out from the push factor migrants from f settling in the Census region where county d is located when predicting ancestry from f in d.

[31] To address endogeneity concerns, we leave out from the economic pull factor migrants from the same continent as f when predicting ancestry from f. We also explore various alternative leave-out strategies as robustness checks and obtain similar results (see Section 5.3).

f at a given time t can then be written as

$$IHS\left(\text{Ancestry}_{d,f}^{t}\right) = \sum_{s=1880}^{t} \delta_s I_{f,-r(d)}^{s} \frac{I_{-c(f),d}^{s}}{I_{-c(f)}^{s}} + \boldsymbol{\delta} \cdot \text{PCs}_{d,f}^{t} + \delta_d + \delta_f + \text{Controls}_{d,f}^{t} + \eta_{d,f}^{t}, \quad (3)$$

where δ_d and δ_f are origin and destination fixed effects. $\text{Controls}_{d,f}^{t}$ includes 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 $(-c(f))$; and the vector $\text{PCs}_{d,f}^{t}$ are principal components summarizing the information contained in higher order interactions of push and economic pull factors, which enter the equation by iteratively substituting for preexisting ancestry. (Collectively, these terms summarize the effect of the social pull factor on the allocation of migrants across counties.)[32]

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

The push-pull interaction terms in Equation (3) — $I_{f,-r(d)}^{s} \frac{I_{-c(f),d}^{s}}{I_{-c(f)}^{s}}$ for $s = 1880 \ldots 2010$ and $\text{PCs}_{d,f}^{t}$ — are the excluded instruments we use in every IV specification of our main estimating equations, Equation (1) and Equation (2). Our identifying assumption is

$$\text{Cov}\left(I_{f,-r(d)}^{s} \frac{I_{-c(f),d}^{s}}{I_{-c(f)}^{s}}, \epsilon_{d,f}^{t} \middle| \text{controls} \right) = 0, \forall s \leq t, \quad (4)$$

[32]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. In practice, including these controls makes has relatively little impact on our estimates.

where $\epsilon^t_{d,f}$ are the residuals from (2). That is, we require that any unobservable factor that makes residents in a domestic location d more or less prejudiced, hostile, or altruistic toward people with ancestry from f in period t ($\epsilon^t_{d,f}$ in (2) large) is conditionally uncorrelated with migrations from f to the *entire* United States (excluding f's migrants to d's region) interacted with migrations from *all* foreign countries to d (excluding foreign migrants f's continent) at some earlier time $s \leq t$.

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 fixed effect δ_d in (2) — and that Mexico may be a preferred destination for donations from *all* US donors — the Mexico fixed effect δ_f in (2). Our first stage predicts a large population of Mexican ancestry in 2010 in Detroit because many Mexicans migrated to the US in 1920 (excluding the Midwest) and Detroit was attracting a large share of foreign migrants in 1920 (excluding Latin Americans). Our identifying assumption requires that this interaction of large Mexican out-migrations and large Detroit in-migrations in 1920 affects donations from non-Mexican Detroiters to Mexico in 2010 through its effect on Mexican settlement in Detroit, and not through any other channel.

To better understand this identifying assumption, it is useful to spell out a (stylized) example where it would fail. For instance, natives in a city (e.g. non-Mexicans in Detroit) may systematically pay more attention to foreign news at times when their city welcomes large number of foreign migrants (e.g. 1920), but ignore foreign news at other times. If at that precise time a given foreign country suffers from domestic instability (e.g. the Mexican revolution and assassination of Zapata in 1919), natives may develop disproportionate empathy towards that country, purely because they happen to pay more attention to world events at that time. This disproportionate empathy may then be passed down across generations of natives, affecting their donation behavior in 2010. Under such a scenario, the same historical push and pull forces that drive historical migration (the 1920 Mexico push interacted with the 1920 Detroit pull) also affect unobservables (particular empathy of Detroiters for Mexico today) that may drive altruism 90 years later (charitable donations from Detroit to Mexico today), even if exposure to residents of foreign descent has no effect (Mexican ancestry in Detroit). We partly address this concern by showing evidence of direct contact with local residents of foreign ancestry (see Section 4.4 and Table 4). We also consider alternative specifications with various definitions of the pull factor and, reassuringly, find similar results (seeSection 5.3 and Appendix Table A5).

3.3 Ruling Out "Selective White Flight"

One remaining identification concern is "selective White flight": respondents who are not generally more xenophobic but instead intolerant only toward a specific ethnic group may, in response to a higher population of immigrants from that group, move to another county. For instance, if White residents in Detroit who specifically dislike Mexicans (but not other minorities) may leave Detroit as the Mexican community grows and move to places with small Mexican communities, then Detroit would display disproportionately positive attitudes and altruism toward Mexicans, biasing our estimated effects of contact upward.

A priori, there are several reasons to doubt that selective White flight significantly biases our estimates. First, mobility has sharply declined in recent decades: around 10 percent of Americans moved between 2018 and 2019, compared with around 18 percent of Americans between 1985 and 1986. This period coincides with a sharp rise in immigration to the United States: around 7 percent of the population was foreign-born in 1985, compared to 14 percent in 2019.[33] Second, the majority (65%) of moves are *within-county*: given that our geographical variation is at the county level, these moves will not affect our estimates.[34] Third, with one exception (Mexican immigrants), immigrants from the countries in our dataset represent very small shares of the population For example, the maximum Arab share of the population — in Wayne County, containing Detroit and Dearborn — is less than 4 percent, and the median Arab share is less than half of one percent. It is thus unlikely that such small populations would meaningfully affect Whites' migration decisions. Finally, to bias estimates in our dyadic donations data, such selective white flight would have to operate at a highly granular scale, where for example some types of white Americans dislike Somalis but not Nigerians, while others dislike Nigerians but not Nicaraguans, and this dislike would have to be intense enough to prompt these types of white Americans to move in implausibly large numbers to sway survey results.

Nonetheless, we systematically test for 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) settle in places with larger or smaller communities with ancestry from f (e.g. Mexico) relative to its national average:

$$\text{WhiteFlightIndex}_{d,f}^{t} = \sum_{d'} Out_{d,d'}^{t} \frac{Ancestry_{d',f}^{t} / Ancestry_{d'}^{t}}{\mathbb{E}\left[Ancestry_{d'',f}^{t} / Ancestry_{d''}^{t} | f\right]}. \tag{5}$$

$Out_{d,d'}^{t}$ is the number of White natives who move from d to d' in period t; $Ancestry_{d',f}^{t} / Ancestry_{d'}^{t}$ is the population share in d' with ancestry from f; it is compared to its national average, $\mathbb{E}\left[Ancestry_{d'',f}^{t} / Ancestry_{d''}^{t} | f\right]$, the average population share with ancestry from f across all US

[33] https://www.migrationpolicy.org/programs/data-hub/charts/immigrant-population-over-time

[34] Source: authors' tabulations from the 2019 Current Population Survey.

counties. In other words, our index is a weighted average of out-migrations of White natives from d to other destinations, with high (low) weights given to a destination if it hosts a large (small) concentration of residents from foreign origin f. For instance, for $d = Detroit$ and $f = Mexico$, this index takes a high value if many White natives from Detroit move to domestic locations d' ($Out_{Detroit,d'}$ large) where Mexican ancestry ($Ancestry^t_{d',Mexico}/Ancestry^t_{d'}$) is large relative to its national average ($\mathbb{E}\left[Ancestry^t_{d'',Mexico}/Ancestry^t_{d''}|Mexico\right]$). We then estimate various specifications of

$$IHS\left(\text{WhiteFlightIndex}^t_{d,f}\right) = \beta IHS\left(\text{Ancestry}^t_{d,f}\right) + \delta_t + \delta_d + \delta_f + \text{Controls}^t_{d,f} + \epsilon^t_{d,f}, \tag{6}$$

where we again instrument for ancestry using (3). 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. If the hypothesis is true, we would expect the estimated coefficient β in (6) to be *negative* and significant. Throughout the empirical analysis, we estimate this coefficient for the main specification in every dataset we consider to rule out selective White flight as a driver of our results.

4 Exposure to Arab-Muslims

4.1 Attitudes toward Arab-Muslims

Our first main finding is that long-term exposure to local populations of Arab ancestry reduces prejudice against Arab-Muslims. We estimate various specifications of the form

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

where we instrument the number of residents of Arab ancestry (i.e. the number of residents claiming ancestry from countries in the Arab League) in individual i's county of residence d, $IHS\left(\text{Ancestry}_{d,Arab}\right)$, using (3). This specification uses a single cross-section, so we omit the time subscript. A higher score for $\text{Attitude}_{d,Arab}$ signifies lower prejudice against Arab-Muslims. We present a binned scatter plot of the first-stage fitted values in Panel A of Appendix Figure A3.

All specifications control for logged population size to ensure that our results are not simply driven by differences between small and large counties. The choice of whether to include additional county-level demographic controls and, if so, which controls to add, is not straightforward. Because our instrument is constructed based on migration flows as early as the 1880s, controlling for any post-1880 county characteristics — such as education or population density — may in theory introduce a "bad controls" problem to the extent that these controls are themselves partially determined by the ancestral composition of migrants (Angrist and Pischke, 2009). Yet given that the population of Arab ancestry is relatively small, many controls, such as those described above, are unlikely to be substantially affected by Arab migration, and including them to soak up excess variation may be desirable. Other covariates, such as the 2012 Republican vote share of the county, are more likely to be directly affected by the local Arab population, yet we are also interested in assessing the extent to which our estimated coefficient changes as we include these bad controls. Given this tradeoff, our preferred approach is to display in our regression tables both a fully parsimonious specification and a series of specifications including various combinations of controls — including potentially bad controls. In the appendix, we display, in figure form, estimates of our coefficient of interest under a every possible combination of a wide range of additional county-level controls.

Panel A of Table 1 displays results on the IAT score from Project Implicit (implicit bias against Arab-Muslims); Panel B displays results on the explicit measure of prejudice from Project Implicit (warmth toward Arab-Muslims); and Panel C displays results on the Nationscape measure (explicit favorability toward Muslims). The key coefficient of interest represents the effect (in standard deviations) of a one-unit increase in IHS(Arab ancestry) on the prejudice measure. The first-stage F-statistics in our more parsimonious specifications are greater than 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).[35]

We find that our estimated coefficients are statistically significant and economically meaningful: in our preferred specification with state fixed effects and individual controls (Column 3), a one-unit increase in the IHS-transformed population of Arab ancestry in a county (approximately half a standard deviation) causes a 0.077 (s.e.=0.035) standard deviation increase in average Arab-Muslim IAT scores (Panel A), a 0.135 (s.e.=0.035) standard deviation increase in explicitly stated warmth towards Muslims (Panel B), and a 0.039 (s.e.=0.020) standard deviation increase in favorability toward Muslims. 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 Orange County, CA to that of Cook County, IL, or going from the Arab-ancestry population of St. Louis County, MO to San Mateo County, CA (see Appendix Figure A2).

Notably, our Project Implicit estimates remain stable with and without state fixed effects (Column 2 vs. Column 3). The magnitude of our Nationscape estimates is cut in half when we introduce state fixed effects, which we attribute to the nature of our county imputation procedure, though the

[35] 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.

coefficient remains statistically significant at the 10% level. The coefficients from both Project Implicit and Nationscape also remain stable as we introduce a series of "bad controls." 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-Whites in general, but specific to Arab-Muslims. Column 6 instead controls for the average Race IAT score within county d, 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. Instead, the positive attitudes engendered by the presence of the Arab-Muslim population is specific to Arab-Muslims themselves.

Of course, it is possible that some other unobservable county characteristic affecting attitudes towards Arabs still correlates with our instrument. We cannot fully rule out this possibility when considering only outcomes relating to a single group (Arab-Muslims). However, we will be able to rule out this kind of subtle concern when we generalize our results to all groups in Section 5.

Quantile regressions In the top panes of Figure A4, we estimate quantile treatment effects (QTEs) and the associated standard errors (following Chernozhukov and Hansen, 2005). Given the sizeable standard errors, these results should be interpreted cautiously. Nonetheless, we find no consistent evidence of a "backlash" effect by which exposure exacerbates the implicit or explicit biases of individuals who were predisposed toward prejudice. Instead, exposure to a local population of Arab-Muslims appears to shift the entire distribution of attitudes towards them into more positive territory.

All respondents Appendix Table A2 presents our Project Implicit results if, instead of restricting to respondents who were forced to take the Implicit Association Test as part of a job, school, or university training, we include all respondents. All of our results remain statistically significant at the 1% level 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.

Auxiliary explicit outcomes Appendix Table A3 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 (Panels C and D), 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 (Panels A and B); indeed, in all specifications with state fixed effects, these coefficients are statistically indistinguishable from zero at a 10% significance level. 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.

4.2 Exposure to Arab-Muslims and Political Preferences

To what extent do these effects on attitudes translate into political preferences? We consider two outcomes: support for the Muslim Ban and 2016 voting for Donald Trump. Analogous to our specification for attitudes, we run county-level analyses of the form:

$$\text{PoliticalPreference}_{i,d} = \beta IHS\left(\text{Ancestry}_{d,Arab}\right) + \text{Controls}_{i,d} + \epsilon_{i,d}, \tag{8}$$

where $\text{PoliticalPreference}_{i,d}$ assesses respondent i's support for the Muslim Ban or support for Donald Trump in 2016.

We present our coefficient estimates in Table 2. Estimates based upon the CCES data (Columns 1–5) suggest that exogenous exposure to people of Arab ancestry significantly reduces both support for the Muslim Ban and voting for Donald Trump in 2016. Controlling for state fixed effects and individual demographic characteristics lowers our estimates somewhat (Columns 3 and 4), but they remain significant at the 5% level. The same is true if we add a control for the 2012 Republican vote share of the respondent's county (Column 5), which suggests that Trump, the most saliently anti-Muslim presidential candidate in recent memory, activated latent political preferences concerning Arab-Muslims in a way that candidates McCain and Romney did not. The results imply that a one-unit increase in the IHS of Arab ancestry (approximately half a standard deviation increase) reduces the likelihood that an individual supports the Muslim Ban by around 5% and the likelihood that an individual voted for Trump (conditional on voting in 2016) by around 6%.

We replicate these results qualitatively in the Nationscape data (Columns 1–5), although for some specifications in Panel B, the coefficients are not significant at the 10% level. The effects estimated based upon the CCES data are larger in magnitude than those estimated based on the Nationscape data, which may again reflect the measurement error associated with our assignment of Nationscape respondents to counties. Nonetheless, our estimated effects of exposure on Muslim Ban and Trump support are consistently negative, and they are statistically significant in the more parsimonious specifications.

Quantile regressions We plot quantile treatment effects in the bottom two panes of Appendix Figure A4. We again do not find a consistent "backlash" effect, although treatment effects appear somewhat less pronounced in the upper half of the distribution of support for the Muslim Ban and for Trump.

4.3 Charitable Donations toward Arab-Muslim Countries

Having documented that exposure to a local population of Arab descent induces a positive effect on attitudes towards and political preferences concerning Arab-Muslims, we now test whether this exposure also affects *revealed altruism*, as measured by charitable donations. We estimate various specifications of

$$\text{Donations Measure}_{d,Arab}^{t} = \beta IHS\left(\text{Ancestry}_{d}^{t}\right) + \delta_t + \text{Controls}_{d}^{t} + \epsilon_{d,Arab}^{t}, \tag{9}$$

where 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 (3). Our variation is thus at the time $\times$ county level. All specifications again control for a time fixed effect (δ_t), and for logged county population in 2010.

We present results in Table 3 separately for the Charity 1 and Charity 2 data. In both datasets, we examine effects on an indicator for the existence of a donation from county d to country f in quarter t and on the IHS-transformed number of donations from county d to country f in quarter t. In Charity 2, we can additionally examine the IHS-transformed dollar value of donations from county d to country f in quarter t (data on the value of donations is unavailable in Charity 1). In all specifications, we restrict to donors who have European-ethnicity names to ensure that we are not capturing a natural tendency of people of Arab-Muslim descent to donate to their home countries (see Section 2.5 for further details).

We find that an exogenously larger Arab population in county d substantially increases the flow of donations from d to Arab countries. This finding holds at the extensive and intensive margins for both donations datasets. It is also robust to controlling for a battery of county-level demographic controls (2010 population density, the share of the 1970 prime-age population with a high school education, and the share of the 1970 prime-age population with a college education) and state fixed effects. Controlling for demographics and state fixed effects does not significantly affect the estimated coefficients or their statistical significance. The effects are substantial: in our preferred specification (Column 4), a one-unit increase in the IHS-transformed Arab population (approximately half a standard deviation increase) causes a 0.136 increase in the IHS-transformed number of donations in the Charity 1 dataset and a 0.489 increase in the corresponding outcome in the Charity 2 dataset, corresponding to increases of 0.74 and 1.69 standard deviations, respectively. Although the first-stage F-statistics are below 10, using weak instrument-robust inference, we are able to reject the null of a zero coefficient for every specification. Our estimates are stable to including a wide range of additional controls; we defer a discussion of robustness to Section 5.1, where we make fuller use of the dyadic nature of our donations data.

Do these results reflect a fundamental change in social preferences toward groups to which counties have greater exposure, or do they simply reflect a change in awareness and salience of causes associated with these groups' ancestral countries? We cannot fully separate these possibilities, but the results on explicit and implicit prejudice and on political attitudes strongly suggest a role for greater exposure changing underlying social preferences.

4.4 Mechanisms: 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 preferences, 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, preferences, and 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 improved attitudes, preferences, and knowledge, 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 4, we estimate the effects of the IHS-transformed Arab population in a respondent's county on several binary outcomes: whether the respondent has eaten in a Middle Eastern restaurant (Column 1), whether the respondent is friends with an Arab-Muslim (Column 2), whether the respondent is acquainted with an Arab-Muslim through work (Column 3), whether the respondent has an Arab-Muslim neighbor (Column 4). Columns 5–7 report effects on a binary variable taking value one if any of the binary variables in Columns 2–4 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. The point estimates are stable to controlling for individual demographics (male, age, age squared, age × male), county-level demographics (2010 population density, share of 1970 prime-age population with high school education, and share of 1970 prime-age population with college education), and individual and county-level political controls (the respondent's vote in 2012 and the 2012 county Republican share).[36] 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.

Knowledge of Arab-Muslims In Panel B of Table 4, 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 US (Column 4), and an index of these three outcomes (Columns 5–7). 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, if statistically weaker, effects. A one-unit increase in the IHS of the Arab population (approximately half a standard deviation) translates into a 0.10 decrease in the measure of negative beliefs about Islam, a 0.13 standard deviation change. It also translates into a 0.31 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 8.6% greater probability that the respondent will correctly define Ramadan. To put these magnitudes into perspective, the corresponding gaps between respondents with

[36] We avoid using 2016 voting given the direct evidence presented in Table 2 that greater exposure to Arab-Muslim populations significantly reduces both individual and county-level voting for Trump.

college degrees and respondents 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 1.6% greater accuracy, approximately 0.13 standard deviations, though this estimate is not statistically significant at the 10% level. Turning to the index, a one-unit increase in the size of the Arab population increases scores by between 0.10 and 0.25 standard deviations, although these estimates do not remain statistically significant at the 10% level when we control for county-level demographics or for individual-level and county-level voting in the 2012 election.

4.5 Robustness

"Selective White flight" One concern is that natives who have disproportionately negative attitudes toward Arabs, and those who are disproportionately less likely to donate, move to other counties with smaller Arab communities — "selective White flight". Column 1 of Table 5 tests this hypothesis using the specification described in Section 3.3. We find no evidence that natives in counties with greater Arab populations endogenously relocate to counties with small Arab populations. Indeed, if anything, exposure to Arab communities makes residents more likely to relocate to areas with *large* Arab populations, conditional on moving at all.

Binned scatter plots We graphically present the results in Tables 1 and 2 in Figure 2. We construct binned scatter plots by residualizing our instruments, our endogenous variable (IHS-transformed Arab ancestry), and our outcomes by age, age squared, a male indicator, state fixed effects (the specification of Column 3 of Table 1 and Columns 3 and 8 of Table 2). We then estimate a two-stage least-squares regression of the (residualized) outcome on the (residualized) endogenous variable, instrumented by the (residualized) instruments, thus recovering the coefficient from the full regression. We then bin our data into forty equally-sized groups based on the fitted values of the endogenous variable and plot means. To assess the extent to which our estimated coefficients are driven by counties with extremely high or extremely low fitted values of Arab ancestry, we also drop observations with fitted values in the top and bottom 2.5% (i.e. the top and bottom bin) and re-estimate the model. The slopes from the regression line fit on the full data (in black) and from the regression line fit on the middle 95% of data (in red) are very close for all four outcomes, confirming that results are not driven by counties in the tails of the distribution of fitted ancestry.

We follow the same approach for the survey outcomes presented in Table 4, presenting the associated binned scatter plots in Figure 3. We residualize by the same set of individual covariates, but we omit the state fixed effects given the smaller sample (thus matching the specifications of Columns

1–5 in Table 4). For the restaurant and contact outcomes, the slope of the regression line fit on the middle 95% of data is extremely similar to that of the line fit on the full dataset; for the negative beliefs about Islam and knowledge outcomes, the slope is steeper if we drop the top and bottom 2.5% of fitted values.

Coefficient stability To probe the robustness of our estimates to the inclusion of different controls, we run specifications of Equation 7 under a wide variety of individual-level and county-level controls, following the procedure of Bursztyn et al. (2020): individual age, age squared, gender, age × gender, and education; county population density in 2010; the share of the population below 18, the share of the population below 65, the median age, and the sex ratio in 2018; the share of prime-age men and women with a high school education, the share of prime-age men and women with a college education, the share of the population below the poverty line, and the log median income in 2018; the inverse hyperbolic sine-transformed population of non-European descent in 2010; the average race IAT score; the 2008 and 2012 Republican vote shares; and state fixed effects. We report results in Panels A and B of Appendix Figure A5. Almost all of our estimates remain significant at the 1% or 5% level, and our coefficient estimates are relatively stable under different combinations of covariates, confirming that our estimates are not driven by arbitrary choices of controls. However, the estimated effects on support for the Muslim Ban and for support for Trump are smaller in magnitude and less statistically significant when simultaneously controlling for multiple "bad controls," such as the 2012 Republican vote share and the race IAT score.

5 Generalized Exposure to Foreign Ancestries

Are the positive effects of exposure specific to the context of Arab-Muslims, or does exposure to *any* given foreign ancestral group change social preferences toward that group? To shed light on this question, we now generalize our analysis to *all* foreign ancestries. For this analysis, we exploit the dyadic structure of our donations dataset — that is, the fact that we observe donation flows originating from many different US counties and flowing to many different countries.

5.1 Donations to Individual Arab-Muslim Countries

To maximize comparability with our previous estimates, we begin by examining the effects of exposure to Arab-Muslims on donations to Arab-Muslim countries. This analysis differs from the analysis presented in Section 4.3 in that rather than pooling all Arab-Muslim countries into a single group (not distinguishing between attitudes toward, for example, Syrians vs. Lebanese), we now exploit variation in donations toward *different* Arab countries, since our variation is at the county-country-time level ($d \times f \times t$). As before, we restrict our set of donors to those with European-ethnicity names.

Our estimates, presented in Table 6, are broadly similar to those presented in Table 3. An exogenously larger population with ancestry from country f in county d increases the flow of donations from d to f, both on the extensive and intensive margin and in both Charity 1 and the Charity 2 data. This finding is robust to controlling for the logged distance from d to f and the absolute d-f latitude difference, county-level demographic controls (2010 population density, the share of the 1970 prime-age population with a high school education, and the share of the 1970 prime-age population with a college education), state fixed effects, and destination country fixed effects. A one-unit increase in the IHS of ancestry from country f (approximately half a standard deviation) leads to an estimated 0.02 increase in the IHS of the number of donations to f (approximately 25% of a standard deviation). Interpreting this estimate as a partial elasticity, doubling the size of the ancestral group in a county increases the number of donations from European-ancestry residents of that county by 1.5% and the total dollar value of donations by 6%. Though still sizeable, these country-specific elasticities are substantially smaller than the "pooled elasticities" ($\Delta ArabDonations/\Delta ArabPopulation$) reported in Table 3, suggesting a substantial role of spillovers between Arab countries: an exogenously larger population from one Arab country improves natives' attitudes and generosity toward *all* Arab countries, not just the country to which they are exposed.[37]

5.2 Charitable Donations to All Countries

We now generalize our results to *all* foreign countries, not just Arab countries. This approach gives us enough variation across county-country pairs to include both domestic county and foreign country fixed effects. Those fixed effects control for the possibility that donors residing in some counties may be more generous (towards any foreign country), and for the possibility that some countries may be more likely to be struck by disaster and attract more donations (from any domestic county). Our specifications take the form:

$$\text{Donations Measure}^t_{d,f} = \beta IHS\left(\text{Ancestry}^t_{d,f}\right) + \delta_d + \delta_f + \delta_t + \text{Controls}^t_{d,f} + \epsilon^t_{d,f}, \tag{10}$$

where δ_d, δ_f are domestic county and foreign country fixed effects.

Table 7 presents our main results. In Column 1, we restrict the population of donors to those

[37] As before, the appendix shows robustness for these results: Appendix Figure A6, shows the stability of coefficients when introducing every possible combination of the same county-level controls used above; Figure 4 presents binscatters displaying the relationship between residualized ancestry and residualized IHS-transformed number of donations, where the residuals are taken with respect to the controls and fixed effects in Column 5 of Table 6. As before, the coefficient estimates appear robust and remain highly significant when dropping tails of fitted values.

with European-origin names; in Column 2, we restrict to donors whose names originate from countries on a different continent than foreign country f; and in Column 3, we restrict to donors whose name originates from a country other than country f. The estimates are highly stable across these different populations. Relative to our estimates when we restrict to Arab countries, our estimated coefficients are even larger: 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 0.052-0.058, approximately 36-45% of a standard deviation depending on the specification. The estimated partial elasticity of the number of donations to f with respect to the size of the ancestral group from f is 5-6%, while the corresponding partial elasticity of the total dollar value of donations to f is 15-16%. The estimated elasticities are very close to those estimated for just donations to Arab countries (i.e. those presented in Table 6, and they are also strikingly similar across both the datasets from Charity 1 and Charity 2.

The inclusion of county fixed effects also allow us to rule out lingering concerns from Section 4 about county-level unobservables being correlated with our instrument. More generally, the fact that estimates remain positive and significant even after including these fixed effects indicates that exposure to immigrant *in general* does not simply increase altruism toward foreign countries: instead, exposure to a *specific* immigrant group over a period of years or decades increases altruism toward that group's ancestral country.

Binscatters Figure 5 presents binscatters displaying the relationship between the population with ancestry from country f in county d and the IHS-transformed number of donations from d to f; we restrict to European-origin donors and residualize by quarter, domestic county, and foreign country fixed effects. Once again, we find that although the relationship is most pronounced in the upper half of the distribution of ancestry, it is mostly increasing throughout the entire distribution.

Split-sample regressions We can also run split-sample versions of Equation 10, estimating the effects of ancestry on donations separately for each foreign country in our two datasets. Our variation in these specifications is at the county $\times$ time level, so we omit domestic county and foreign country fixed effects, but we control for the county's logged population and for time fixed effects. Figure 6 presents the corresponding coefficient estimates for Charity 2 and for Charity 1, focusing on the IHS-transformed number of donations in each quarter. All coefficient estimates are positive and all but eight are statistically significant at the 5% level. These results confirm that the results are not driven by a small number of countries or a small number of regions, but rather reflect a general tendency for counties with (exogenously) greater populations with ancestry from a given country to donate to that country.

5.3 Robustness

"Selective White flight" As before, we may be worried that these estimates simply reflect the endogenous out-migration of natives who are disproportionately hostile toward minorities with ancestry from foreign country f rather than a positive treatment effect. County fixed effects somewhat alleviate these concerns, as for this mechanism to generate a positive bias in our estimated coefficients, we would require that natives are *specifically* intolerant toward minorities with ancestry from f rather than intolerant of non-natives in general. Nonetheless, we test for "selective White flight" using the specification described in Section 3.3, reporting results in Table 5. Columns 5–6 restrict the sample to Arab countries only, while Columns 7–8 consider the whole dataset. As before, for all specifications, we can rule out even small negative coefficients: if anything, exposure to a local population with ancestry from f makes movers more, rather than less, likely to relocate to counties with larger populations from f.

Correlated migrations Our first set of robustness checks considers alternative instruments for our first stage Equation (3). We focus on the specification of Equation (10), as this specification allows us to control for the most exhaustive set of controls, including quarter, domestic county, and foreign country fixed effects.

One potential concern is that when predicting ancestry from f in d, even though we exclude from our "pull" factor any migrant to d coming from the same continent as f, the choices of migrants from other continents may mimic those of migrants from f, so that our pull factor may be contaminated by unobserved $d \times f$ factors. We construct two alternative instruments. First, instead of constructing our pull factor based on migrants from other continents, we do so based only on *European* migrants. Because Americans tend not to donate to causes in (rich) European countries, doing so may further alleviate endogeneity concerns. This is also a natural robustness check given that our baseline specifications include only donors who have European-origin names. Second, we directly address the concern that the migration patterns of country f may be very similar to that of another country (across time and space). Instead of leaving out migrants from any country f' in the same continent as f when predicting ancestry from f (our standard specification), we remove instead migrants from any country f' with correlated migrations. 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, we exclude f' from the construction of the pull factor.

In Column 1 of Appendix Table A5, we present our baseline estimate for comparison; Column 2 presents estimates if we exclude foreign countries with correlated migration patterns; and Column 3 presents estimates if we calculate the pull factor based only upon European migration. Our first-stage F-statistics remain strong, and none of the five second stage estimates (Charity 1 donations indicator, Charity 1 number of donations, Charity 2 donations indicator, Charity 2 number of donations, and Charity 2 dollar value of donations) are substantially smaller or less precisely estimated under these alternative specifications. Indeed, the point estimates in most specifications are slightly *larger*.

Country leave-outs Appendix Table A6 instead explores the robustness of our main finding to removing specific subsets of countries. The organization of the table is similar to table A5, with the five panels corresponding to the five different outcomes across the two datasets and the first column repeating our baseline specification. The second column removes all Arab countries. The third column removes Hispanic countries. We remove both Arab and Hispanic countries in the fourth column, and in the fifth column, we remove all (non-Arab) African countries: to the extent that communities of sub-Saharan African ancestry may be perceived as similar to African-American communities, the effect of exposure to those communities on altruism may be different than for other minority groups. Overall, while the coefficient estimates do meaningfully change across these five exclusion criteria, they remain economically large and, with the exception of the Charity 2 estimates when we remove African countries, statistically significant at least at the 10% level. We conclude that while the effect of exposure on altruism may vary between different foreign ancestries (as our results on the interaction with cultural distance in Table 8 below also suggest), no specific group of countries drives the overall effect.

Standard errors Throughout our dyadic analyses of charitable donations, we cluster standard errors at the foreign country level in order to account for the possibility of within-destination correlation in donation patterns. In Appendix Table A7, 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, two-way clustering by foreign country and domestic county, and two-way clustering by foreign country and domestic state. The standard errors change little and our estimates remain statistically significant under every choice; indeed, the table reveals that clustering at the foreign country level is one of the most conservative choices across all outcomes.

5.4 Heterogeneity

Finally, we examine whether the effects of exposure are magnified or weakened by various measures of cultural proximity. On one hand, we might expect that the effects would be stronger for more culturally proximate countries: people with ancestry from such countries may be more integrated and thus may lead natives to donate more. On the other hand, treatment effects may be stronger for more culturally distant countries: given lower awareness about events in these countries and lower base rates of altruism (Alesina et al., 2005), contact with people with ancestry from culturally distant countries may have a disproportionately large effect on natives' knowledge and altruism.

To investigate patterns of heterogeneity, we estimate an augmented version of (10),

$$\begin{aligned}\text{Donations}^t_{d,f} = \beta IHS\left(\text{Ancestry}^t_{d,f}\right) + \gamma IHS\left(\text{Ancestry}^t_{d,f}\right) \times \text{Distance Measure}_{d,f} \\ + \mu \text{Distance Measure}_{d,f} + \delta_t + \delta_d + \delta_f + \text{Controls}^t_{d,f} + \epsilon^t_{d,f},\end{aligned} \quad (11)$$

where we again instrument ancestry using (3) and the interaction of ancestry with the distance measure by the interactions of each instrument from (3) with the distance measure. We are interested in the parameter γ on the interaction between exposure to foreign ancestry and various measures of the distance from d to f.

Table 8 presents results, again focusing on the intensive margin (the number of donations from county d to country f in quarter t).[38] We consider the geographic logged distance between the United States and country f in Column (2), the genetic distance in Column (3), the linguistic distance in Column (4), and the religious distance in Column (5) (Spolaore and Wacziarg, 2016).

We first note that the estimated direct effect of ancestry β remains relatively stable and statistically significant whether or not we control for the interaction between ancestry and distance (Column 1 versus Columns 2–5). This is reassuring, and suggests our results are not driven by specific countries which may be more or less dissimilar to US counties. Second, our results suggest that the positive effect of exposure on altruism is stronger for countries which are genetically more dissimilar (Column 3): the interaction of IHS(Ancestry) with genetic distance is statistically significant at the 5% level in both Charity 1 and Charity 2. We cautiously interpret this result as suggestive evidence that exposure to local populations whose physical appearance may be more dissimilar to that of the majority local population has a stronger (positive) effect on altruism.[39] We find limited evidence that physical, linguistic, or religious distance play a role in shaping the effect of exposure.

We conclude from this exploration that exposure may reduce the perceived distance between local residents and foreigners, and that this effect may partly explain why exposure to neighbors of foreign ancestry induces more altruism towards foreigners. These results relate to findings by Fouka et al. (2020a) and Fouka et al. (2020b) indicating that perceptions of *relative* distance to an out-group might

[38] Results for the other three outcomes are similar and are available upon request.

[39] These results replicate in our alternative ancestry shares functional form (Appendix Table C8), but only for Charity 1. While the interaction of percent ancestry with genetic distance is positive in the Charity 2 dataset, it is not statistically significant at the 10% level.

shape natives' attitudes toward that group.

6 Conclusion

In this paper, we examine the effect of decades-long exposure to individuals of foreign descent on attitudes, political preferences, and altruism toward them, exploiting exogenous variation in the ancestral composition of US counties generated by historical "push" and "pull" factors in immigration alongside large-scale cross-county datasets on implicit and explicit prejudice, political preferences, and charitable donations. Beginning with a detailed analysis of beliefs about and behavior toward Arab-Muslims, 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 Donald 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 important mechanisms underlying these effects. Finally, we generalize our analysis to show that greater exposure to *any* foreign ancestry causes greater altruism toward that ancestry, with these effects particularly strong for ancestries that are genetically more distant.

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 effects of exposure on altruism 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 preferences, 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 Latinos, East Asians, or South Asians — particularly given the very different positive and negative stereotypes associated with these groups. Finally, how do 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

Table 1: Effect of Arab Ancestry on Attitudes toward Arab-Muslims

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Panel A: Project Implicit	*Score on Arab-Muslim IAT (std., higher score = less prejudiced)*						
IHS(Arab ancestry)	0.019^{***}	0.075^{**}	0.077^{**}	0.107^{***}	0.107^{***}	0.091^{***}	0.091^{***}
	(0.007)	(0.030)	(0.035)	(0.032)	(0.035)	(0.030)	(0.032)
Age			-0.015^{***}	-0.015^{***}	-0.015^{***}	-0.015^{***}	-0.015^{***}
			(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age squared			0.0001^{***}	0.0001^{***}	0.0001^{***}	0.0001^{***}	0.0001^{***}
			(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Male			-0.128^{***}	-0.128^{***}	-0.128^{***}	-0.127^{***}	-0.127^{***}
			(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
IHS(non-Euro ancestry)					-0.022		
					(0.021)		
Avg. race IAT score						0.021^{***}	
						(0.007)	
2012 Rep. vote share							-0.128^{**}
							(0.054)
AP F-statistic	—	12.87	10.09	10.44	12.02	9.962	9.986
Weak IV-robust p-value	—	< 0.05	< 0.05	< 0.01	< 0.01	< 0.01	< 0.01
Observations	58,987	58,987	58,247	58,220	58,220	58,220	58,220
Panel B: Project Implicit	*Warmth toward Arab-Muslims (std., higher score = more favorable)*						
IHS(Arab ancestry)	0.039^{***}	0.142^{***}	0.135^{***}	0.159^{***}	0.150^{***}	0.125^{***}	0.126^{***}
	(0.010)	(0.029)	(0.035)	(0.035)	(0.037)	(0.030)	(0.033)
AP F-statistic	—	13.09	10.14	10.53	12.06	10.05	10.06
Weak IV-robust p-value	—	< 0.01	< 0.10	< 0.01	< 0.01	< 0.01	< 0.01
Observations	58,796	58,796	58,068	58,040	58,040	58,040	58,040
Panel C: Nationscape	*Favorability toward Muslims (std., higher score = more favorable)*						
IHS(Arab ancestry)	0.029^{***}	0.095^{***}	0.044^{**}	0.051^{**}	0.046^{**}	0.043^{**}	0.044^{**}
	(0.006)	(0.021)	(0.020)	(0.021)	(0.021)	(0.021)	(0.019)
AP F-statistic	—	49.46	20.42	19.32	18.90	18.00	19.42
Weak IV-robust p-value	—	< 0.01	< 0.05	< 0.01	< 0.01	< 0.05	< 0.05
Observations	187,435	187,435	187,435	187,435	187,435	187,435	187,435
State FE	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); and the dependent variable in Panel C is the stated favorability toward Muslims (from Nationscape). All three 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. County-level demographic controls include the 2010 population density, the share of the 1970 prime-age population with a high school education, and the share of the 1970 prime-age population with a college education. Standard errors are given in parentheses. Standard errors are clustered at the county level in Panels A and B and are clustered at the congressional district level in Panel C. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 2: Effect of Arab Ancestry on Political Preferences

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) OLS	(7) IV	(8) IV	(9) IV	(10) IV
Panel A:	*Support for the Muslim Ban*									
	CCES					Nationscape				
IHS(Arab ancestry)	-0.033^{***}	-0.097^{***}	-0.069^{***}	-0.074^{***}	-0.053^{***}	-0.007^{**}	-0.039^{***}	-0.044^{***}	-0.032^{**}	-0.030^{**}
	(0.005)	(0.034)	(0.023)	(0.025)	(0.019)	(0.003)	(0.012)	(0.013)	(0.014)	(0.014)
Age			0.016^{***}	0.016^{***}	0.016^{***}			0.017^{***}	0.017^{***}	0.017^{***}
			(0.001)	(0.001)	(0.001)			(0.001)	(0.001)	(0.001)
Age squared			-0.0001^{***}	-0.0001^{***}	-0.0001^{***}			-0.0001^{***}	-0.0001^{***}	-0.0001^{***}
			(0.00001)	(0.00001)	(0.00001)			(0.00001)	(0.00001)	(0.00001)
Male			0.026^{*}	0.030^{*}	0.031^{**}			0.138^{***}	0.135^{***}	0.136^{***}
			(0.015)	(0.015)	(0.015)			(0.013)	(0.013)	(0.013)
2012 Rep. vote share					0.375^{***}					0.088^{***}
					(0.030)					(0.024)
AP F-statistic	—	14.98	10.15	8.674	8.805	—	49.18	20.52	19.95	20.23
Weak IV-robust p-value	—	< 0.01	< 0.05	< 0.01	< 0.05	—	< 0.10	< 0.10	< 0.05	< 0.05
Observations	56,814	56,814	56,814	56,814	56,729	58,183	58,183	58,183	58,183	58,183
Panel B:	*Voted for Trump in 2016*									
	CCES					Nationscape				
IHS(Arab ancestry)	-0.031^{***}	-0.110^{***}	-0.068^{**}	-0.092^{***}	-0.064^{***}	-0.019^{***}	-0.064^{***}	-0.017	-0.028^{*}	-0.022
	(0.005)	(0.037)	(0.029)	(0.030)	(0.021)	(0.004)	(0.015)	(0.015)	(0.016)	(0.014)
AP F-statistic	—	15.03	9.890	8.545	8.646	—	51.11	20.50	19.45	19.59
Weak IV-robust p-value	—	< 0.01	< 0.05	< 0.01	< 0.01	—	< 0.01	> 0.10	< 0.10	> 0.10
Observations	77,800	77,800	77,800	77,800	77,679	170,190	170,190	170,190	170,190	170,190
State FE	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes	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, with Columns 1–5 using data from the CCES and Columns 6–10 using data from Nationscape. The dependent variable in Panel B is self-reported Trump votership, with Columns 1–5 again using data from the CCES and Columns 6–10 data from Nationscape. 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. County-level demographic controls include 2010 population density, share of 1970 prime-age population with high school education, and share of 1970 prime-age population with college education as controls. Standard errors are given in parentheses. Standard errors are clustered at the county level in Columns 1–5 and at the congressional district level in Columns 6–10. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Effect of Ancestry on Donations, Pooling Arab Countries (European-Ethnicity Donors Only)

	(1) OLS	(2) IV	(3) IV	(4) IV
Panel A: Charity 1	Donations (dummy)		*(mean* = 0.011*, sd* = 0.106*)*	
IHS(Arab ancestry)	0.005***	0.076***	0.078***	0.085***
	(0.0005)	(0.009)	(0.012)	(0.013)
AP F-statistic	—	11.99	7.424	5.539
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01
Observations	168,102	168,102	168,102	168,102
Panel B: Charity 1	IHS(# donations)		*(mean* = 0.017*, sd* = 0.180*)*	
IHS(Arab ancestry)	0.008***	0.122***	0.122***	0.134***
	(0.001)	(0.017)	(0.021)	(0.023)
Panel C: Charity 2	Donations (dummy)		*(mean* = 0.041*, sd* = 0.198*)*	
IHS(Arab ancestry)	0.021***	0.249***	0.248***	0.290***
	(0.001)	(0.023)	(0.028)	(0.034)
AP F-statistic	—	8.996	5.865	5.930
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01
Observations	99,616	99,616	99,616	99,616
Panel D: Charity 2	IHS(# donations)		*(mean* = 0.053*, sd* = 0.296*)*	
IHS(Arab ancestry)	0.032***	0.451***	0.420***	0.506***
	(0.003)	(0.061)	(0.068)	(0.079)
Panel E: Charity 2	IHS($ donations)		*(mean* = 0.208*, sd* = 1.063*)*	
IHS(Arab ancestry)	0.116***	1.420***	1.362***	1.606***
	(0.009)	(0.152)	(0.175)	(0.203)
Demographic controls	No	No	Yes	Yes
State FE	No	No	No	Yes
Quarter FE	Yes	Yes	Yes	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 in Panels A and C is a dummy for the presence of at least one donation from the county to any Arab League country in a quarter. The dependent variable in Panels B and D is the IHS-transformed number of donations from the county to Arab League countries in a quarter. The dependent variable in Panel E is the IHS-transformed total value 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: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. All specifications control for log 2010 population. All specifications control for quarter fixed effects. Columns 3–4 include 2010 population density, share of 1970 prime-age population with high school education, and share of 1970 prime-age population with college education. Column 4 includes state fixed effects. We suppress the first-stage F-statistic and the number of observations in Panel B because they are identical to those in Panel A; we likewise suppress these statistics in Panels D and E because they are identical to those in Panel C. 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 4: Mechanisms: Contact with and Knowledge of Arab-Muslims

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Survey	*Contact with Arab-Muslims*						
	Restaurant	Friends	Workplace	Neighbors	Any (2–4)		
IHS(Arab ancestry)	0.052***	0.025	0.088**	0.090***	0.126***	0.101**	0.093***
	(0.005)	(0.024)	(0.037)	(0.025)	(0.037)	(0.043)	(0.030)
Dep. var. mean	0.439	0.098	0.286	0.198	0.396	0.396	0.396
Dep. var. std. dev	0.496	0.297	0.452	0.398	0.489	0.489	0.489
AP F-statistic	7.743	7.743	7.743	7.399	7.399	5.925	6.811
Weak IV-robust p-value	< 0.05	< 0.05	< 0.05	< 0.05	< 0.01	< 0.10	< 0.10
Observations	5,189	5,189	5,189	5,189	5,189	5,189	5,189
Panel B: Survey	*Knowledge of Arab-Muslims*						
	Subservice/war	Pillars	Ramadan	Pop. accuracy	Index (2–4)		
IHS(Arab ancestry)	−0.139***	0.392***	0.090**	2.791**	0.342***	0.259**	0.214
	(0.053)	(0.148)	(0.039)	(1.086)	(0.104)	(0.121)	(0.138)
Dep. var. mean	0.590	4.493	0.764	-15.057	0.000	0.000	0.000
Dep. var. std. dev	0.758	1.558	0.425	13.612	1.000	1.000	1.000
AP F-statistic	7.743	7.743	7.743	7.399	7.399	5.925	6.811
Weak IV-robust p-value	< 0.05	< 0.05	< 0.05	< 0.05	< 0.01	< 0.10	> 0.10
Observations	5,014	5,014	5,014	4,724	4,724	4,724	4,724
Individual demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-level demographics	No	No	No	No	No	Yes	Yes
Political controls	No	No	No	No	No	No	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. In Panel A, the dependent variable in Column 1 is an indicator for whether the respondent reports having ever eaten at a Middle Eastern restaurant; the dependent variables in Columns 2–4 are indicators for whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor, respectively; and the dependent variable in Columns 5–7 is an indicator taking value one if any of the indicators in Columns 2–4 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 Columns 5–7 is constructed by scaling the dependent variables in Columns 2–4 to mean zero and standard deviation one, summing these three scaled values, and renormalizing. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age × male. County-level demographics include the 2010 population density, the share of 1970 prime-age population with high school education, and the share of 1970 prime-age population with college education as controls. Political controls include both controls for individual voting in 2012 and the 2012 county Republican vote share. 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 5: White Flight

	(1) Pooled Arab	(2) C1 Arab	(3) C2 Arab	(4) C1	(5) C2
Panel A: 1980 cross-section		*Selective white flight index*			
IHS(Ancestry)	2.079***	3.910***	4.205***	0.384	0.007
	(0.082)	(0.659)	(0.775)	(0.284)	(0.136)
Dep. var. mean	8.665	8.662	8.717	8.148	7.980
Dep. var. s.d.	1.452	1.575	1.549	2.282	2.433
Observations	3,084	30,840	49,344	144,948	431,760
Panel B: 1980-2000 panel		*Selective white flight index*			
IHS(Ancestry)	1.764***	3.019***	3.290***	0.302	0.007
	(0.043)	(0.462)	(0.559)	(0.240)	(0.126)
Dep. var. mean	9.334	9.278	9.295	8.841	8.760
Dep. var. s.d.	1.529	1.676	1.646	2.198	2.241
Observations	9,333	93,340	149,344	401,138	1,225,360
Domestic state FE	Yes	Yes	Yes	No	No
Domestic county FE	No	No	No	Yes	Yes
Foreign country FE	No	Yes	Yes	Yes	Yes

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 3.3. 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 Column 1 is the IHS-transformed population with ancestry from Arab League countries; the endogenous variable in Columns 2–5 is the IHS-transformed population with ancestry from country d. 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 2–3 limit the sample to domestic county–foreign country pairs in which the foreign country is in the Arab League, separately for Charity 1 (C1) and Charity 2 (C2). Columns 4–5 include the full samples of county-country pairs from Charity 1 (C1) and Charity 2 (C2). Standard errors are given in parentheses. Standard errors are robust in Columns 1–4 and are clustered at the destination country level in Columns 5–8. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Effect of Ancestry on Donations, Arab Countries and European-Ethnicity Donors Only

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV
Panel A: Charity 1	Donations (dummy) *(mean = 0.005, sd = 0.068)*					
IHS(Ancestry)	0.004***	0.009***	0.010***	0.010***	0.008***	0.013***
	(0.0001)	(0.0004)	(0.001)	(0.001)	(0.001)	(0.001)
AP F-statistic	—	587.3	343.8	246.6	243.7	115.1
Observations	719,289	719,289	718,602	714,022	714,022	714,022
Panel B: Charity 1	IHS(# donations) *(mean = 0.006, sd = 0.091)*					
IHS(Ancestry)	0.006***	0.016***	0.018***	0.017***	0.015***	0.024***
	(0.0002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Panel C: Charity 2	Donations (dummy) *(mean = 0.006, sd = 0.076)*					
IHS(Ancestry)	0.007***	0.015***	0.012***	0.009***	0.008***	0.023***
	(0.0001)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.001)
AP F-statistic	—	1164.9	822.1	662.7	656.9	227.7
Observations	1,030,248	1,030,248	1,029,264	1,022,704	1,022,704	1,022,704
Panel D: Charity 2	IHS(# donations) *(mean = 0.006, sd = 0.095)*					
IHS(Ancestry)	0.008***	0.019***	0.016***	0.011***	0.010***	0.031***
	(0.0001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Panel E: Charity 2	IHS($ donations) *(mean = 0.028, sd = 0.387)*					
IHS(Ancestry)	0.034***	0.074***	0.061***	0.043***	0.038***	0.118***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)
Distance controls	No	No	Yes	Yes	Yes	Yes
Demographic controls	No	No	No	Yes	Yes	Yes
Origin state FE	No	No	No	No	Yes	Yes
Destination country FE	No	No	No	No	No	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donations to Arab League countries from donors with European-ethnicity names are included. The dependent variable in Panels A and C is a dummy for the presence of at least one donation from destination to origin in a quarter. The dependent variable in Panels B and D is the IHS-transformed number of donations from destination to origin in a quarter. The dependent variable in Panel E is the IHS-transformed total value of donations from destination to origin in a quarter. The main variable of interest is the IHS-transformed population with ancestry from country d: year 2000 for Charity 1 and year 2010 for Charity 2. In all columns, we include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\dots,2000}$ 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. All specifications control for quarter fixed effects. All specifications control for log 2010 population. Columns 3–6 include logged county-country distance and latitude difference. Columns 4–6 includes the 2010 population density, the share of 1970 prime-age population with high school education, and the share of 1970 prime-age population with college education. Columns 5–6 include origin state fixed effects. Column 6 includes destination country fixed effects. We suppress the first-stage F-statistic and the number of observations in Panel B because they are identical to those in Panel A; we likewise suppress these statistics in Panels D and E because they are identical to those in Panel C. Standard errors are given in parentheses. Standard errors are robust. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Effect of Ancestry on Donations, All Countries, Different Populations of Donors

	(1) Europeans	(2) Other continents	(3) All
Panel A: Charity 1		Donations (dummy)	
IHS(Ancestry)	0.023***	0.023***	0.024***
	(0.007)	(0.008)	(0.008)
Dep. var. mean	0.007	0.007	0.008
Dep. var. s.d.	0.082	0.086	0.089
First-stage F-statistic	119.0	118.9	118.8
Observations	2,195,559	2,195,559	2,195,559
Panel B: Charity 1		IHS(# donations)	
IHS(Ancestry)	0.058***	0.059***	0.062***
	(0.015)	(0.016)	(0.016)
Dep. var. mean	0.009	0.010	0.011
Dep. var. s.d.	0.128	0.137	0.144
Panel C: Charity 2		Donations (dummy)	
IHS(Ancestry)	0.025**	0.026**	0.026**
	(0.011)	(0.012)	(0.012)
Dep. var. mean	0.010	0.011	0.012
Dep. var. s.d.	0.101	0.105	0.107
First-stage F-statistic	1202.4	1169.3	1175.4
Observations	9,482,679	9,482,679	9,482,679
Panel D: Charity 2		IHS(# donations)	
IHS(Ancestry)	0.052**	0.057**	0.058**
	(0.022)	(0.024)	(0.025)
Dep. var. mean	0.013	0.014	0.015
Dep. var. s.d.	0.145	0.153	0.158
Panel E: Charity 2		IHS($ donations)	
IHS(Ancestry)	0.153**	0.163**	0.163**
	(0.069)	(0.073)	(0.075)
Dep. var. mean	0.051	0.055	0.058
Dep. var. s.d.	0.526	0.548	0.564
Origin county FE	Yes	Yes	Yes
Destination country FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Donations are dropped when the first-best or second-best classification of their name's ethnicity matches the receiving country. Column 1 additionally limits the sample to European donors, while Column 2 additionally limits the sample to donors whose name is matched to a country on a different continent than the receiving country. The dependent variable in Panels A and C is a dummy for the presence of at least one donation from destination to origin in a quarter. The dependent variable in Panels B and D is the IHS-transformed number of donations from destination to origin in a quarter. The dependent variable in Panel E is the IHS-transformed total value of donations from destination to origin in a quarter. The main variable of interest is the IHS-transformed population with ancestry from country d: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. All specifications control for origin county, destination country, and quarter fixed effects. We suppress the first-stage F-statistic and the number of observations in Panel B because they are identical to those in Panel A; we likewise suppress these statistics in Panels D and E because they are identical to those in Panel C. Standard errors are given in parentheses. Standard errors are clustered at the destination country level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Heterogeneity by Physical and Cultural Distance

	(1)	(2)	(3)	(4)	(5)
Panel A: Charity 1			*IHS(# donations)*		
IHS(Ancestry)	0.058***	0.032**	0.029***	0.022***	0.024***
	(0.015)	(0.013)	(0.005)	(0.008)	(0.007)
IHS(Ancestry) × physical distance		−0.001			
		(0.002)			
IHS(Ancestry) × genetic distance			0.010**		
			(0.004)		
IHS(Ancestry) × linguistic distance				−0.001	
				(0.003)	
IHS(Ancestry) × religious distance					−0.005
					(0.004)
Observations	2,195,559	2,195,559	2,038,509	2,126,457	2,126,457
Panel B: Charity 2			*IHS(# donations)*		
IHS(Ancestry)	0.052**	0.071**	0.065**	0.051***	0.046*
	(0.022)	(0.031)	(0.026)	(0.018)	(0.025)
IHS(Ancestry) × physical distance		0.006			
		(0.004)			
IHS(Ancestry) × genetic distance			0.015**		
			(0.006)		
IHS(Ancestry) × linguistic distance				0.010	
				(0.022)	
IHS(Ancestry) × religious distance					0.005
					(0.012)
Observations	9,482,679	9,482,679	8,647,173	9,102,618	8,706,852
Origin county FE	Yes	Yes	Yes	Yes	Yes
Destination country FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donors with a European-ethnicity name are kept. The dependent variable in both panels is the IHS-transformed number of donations. The main variable of interest is the IHS-transformed population with ancestry from country d — year 2000 for Charity 1 and year 2010 for Charity 2 — and the interaction of this variable with a measure of distance: log physical distance in Column 2, genetic distance in Column 3, linguistic distance in Column 4, and religious distance in Column 5 (all distance measures are standardized). In all specifications, 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; we also include as excluded instruments the interaction of the corresponding distance measure with all of the excluded instruments listed above. All specifications control for origin, destination, and quarter fixed effects. Standard errors are given in parentheses. Standard errors are clustered at the origin county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure 1: Charity 2 donations by origin (top) and destination (bottom)

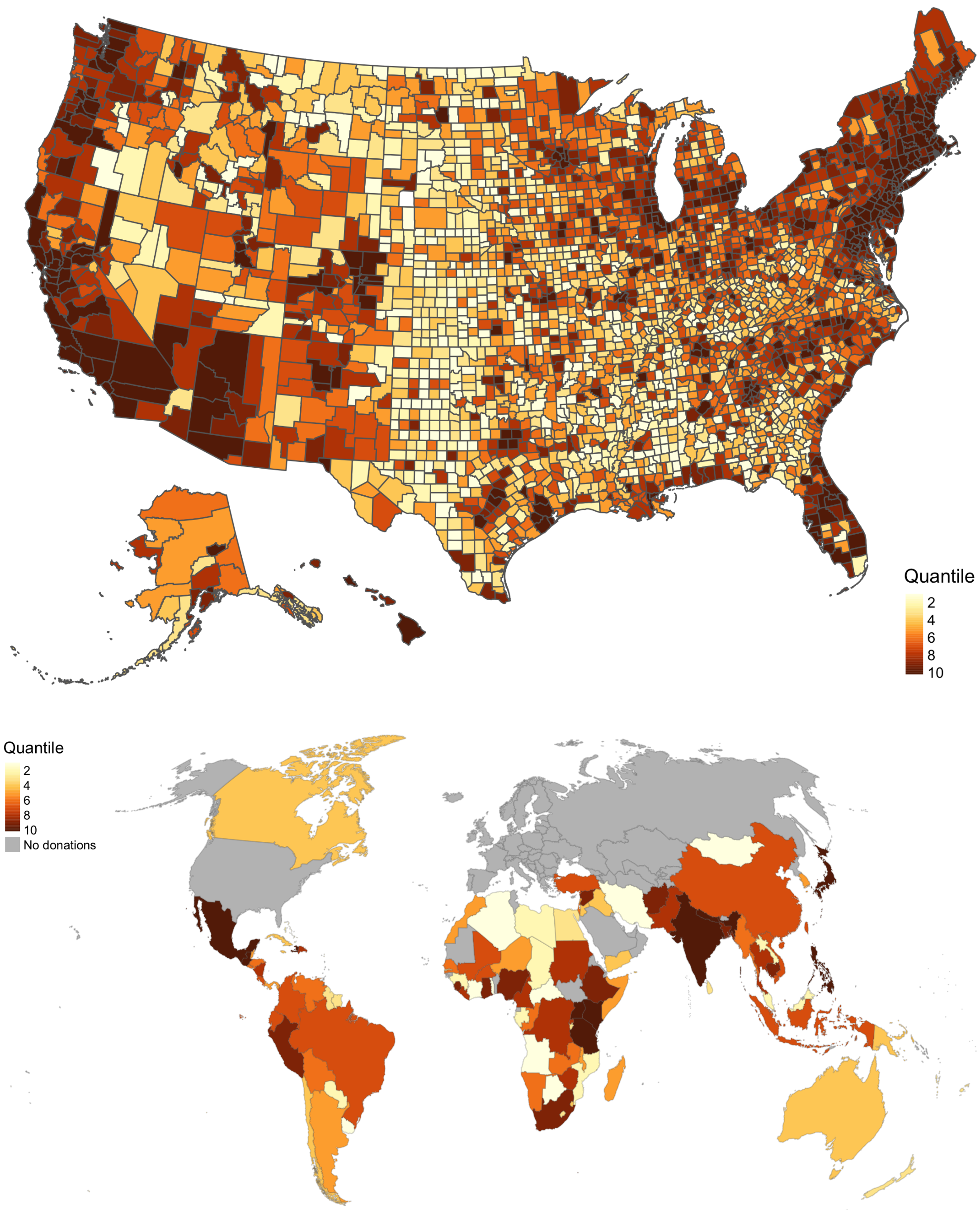
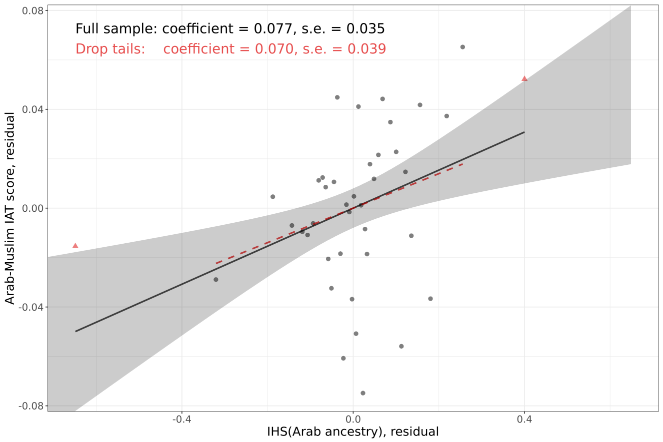
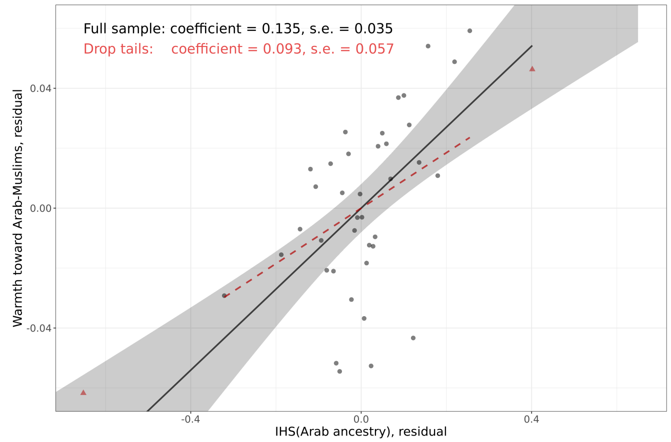


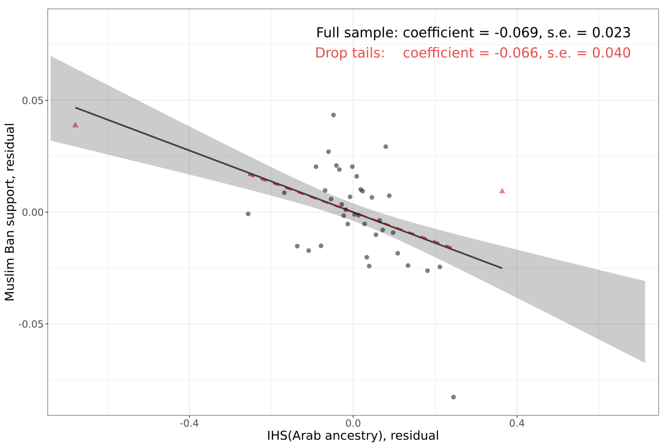
Figure 2: Binned scatter plots: effects on attitudes and political preferences



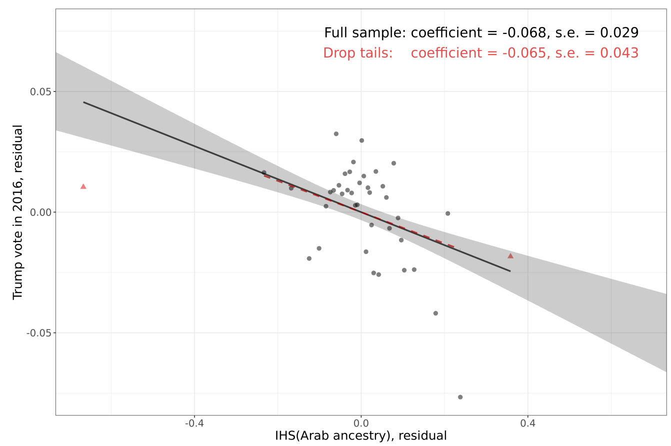
(a) Score on Arab-Muslim IAT (Project Implicit)



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



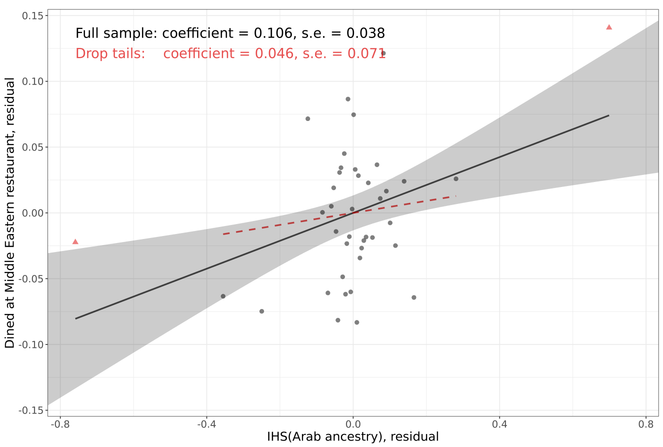
(c) Muslim Ban support (CCES)



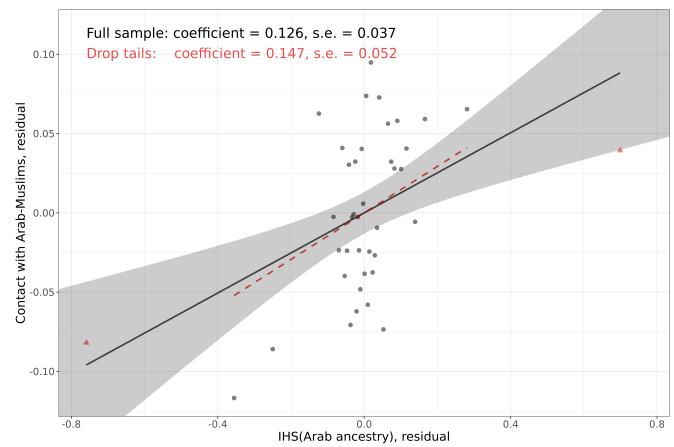
(d) Trump vote in 2016 (CCES)

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 1. 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.

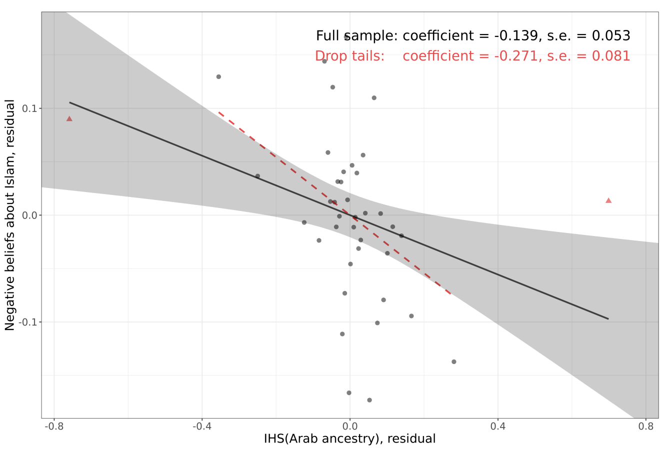
Figure 3: Binned scatter plots: contact and knowledge



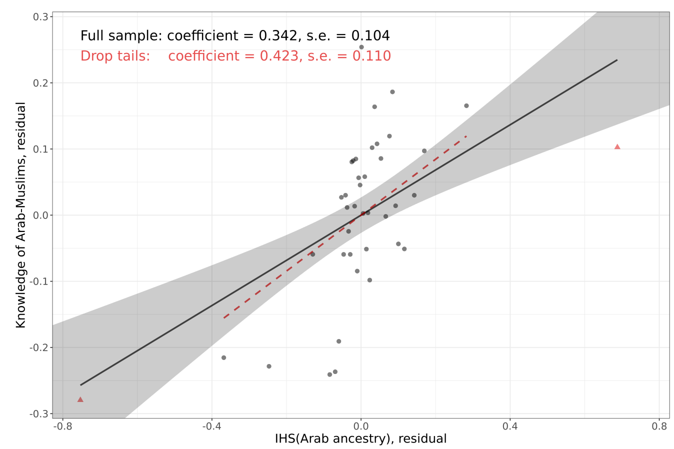
(a) Visited Middle Eastern restaurant



(b) Contact with Arab-Muslims



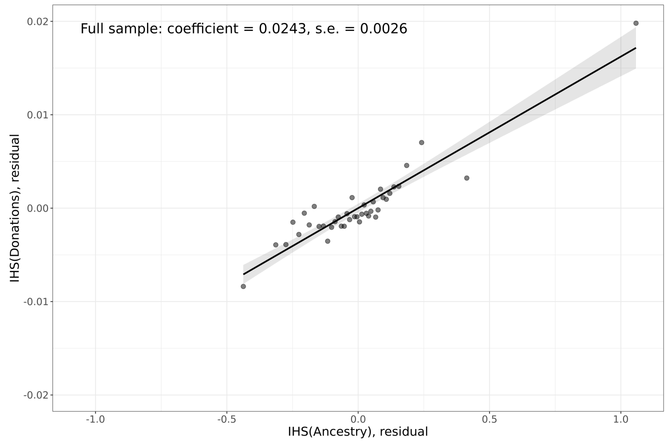
(c) Negative beliefs about Islam



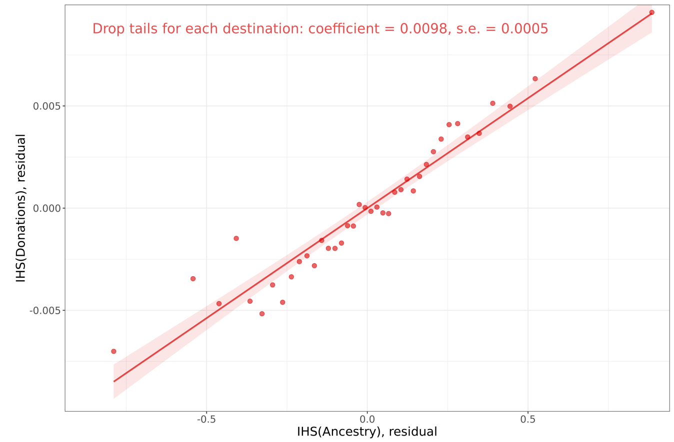
(d) Knowledge about Islam

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 4. 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.

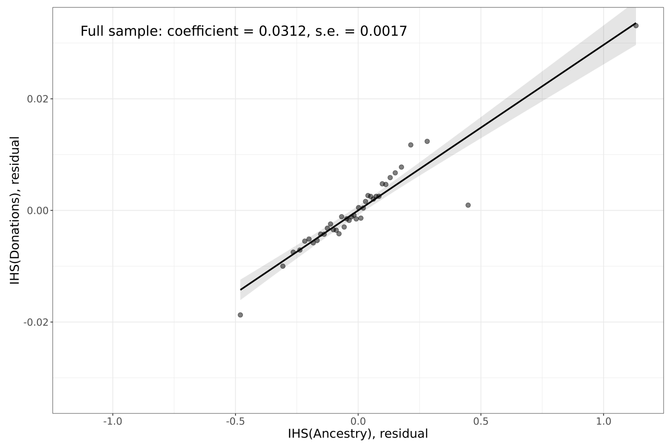
Figure 4: Binned scatter plots: IHS(# donations), Arab countries only



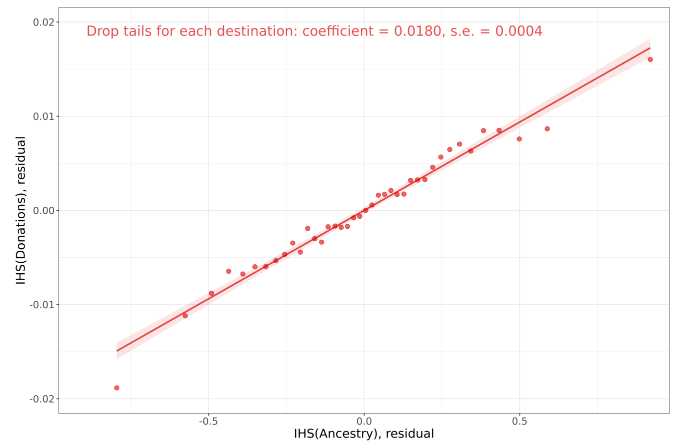
(a) Charity 1, full sample



(b) Charity 1, dropping tails of fitted values



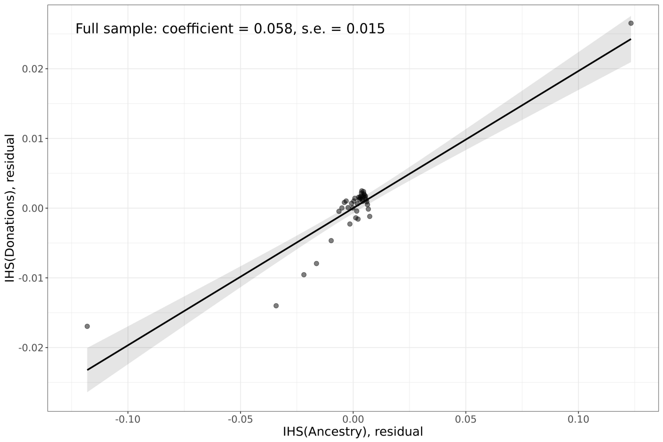
(c) Charity 2, full sample



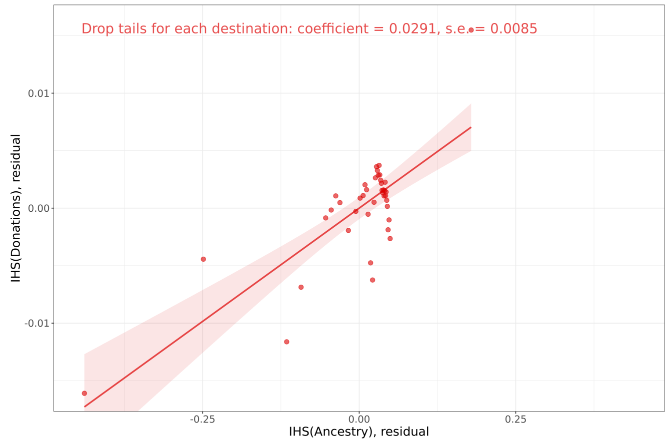
(d) Charity 2, dropping tails of fitted values

Notes: Figure 4 presents binned scatter plots displaying the relationship between the fitted values of county's population from a given Arab country and the county's IHS-transformed number of donations to that country in a given quarter. Panels A and B consider Charity 1 (C1) dataset; Panels C and D consider the Charity 2 (C2) dataset. The main variable of interest is the IHS-transformed population with ancestry from country d: year 2000 for Charity 1 (C1) and year 2010 for Charity 2 (C2). 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 6 of Table 6. Panels B and D present regression lines dropping the top and bottom 2.5% of fitted values for each country. Standard errors are clustered at the foreign country level. 95% confidence intervals are reported.

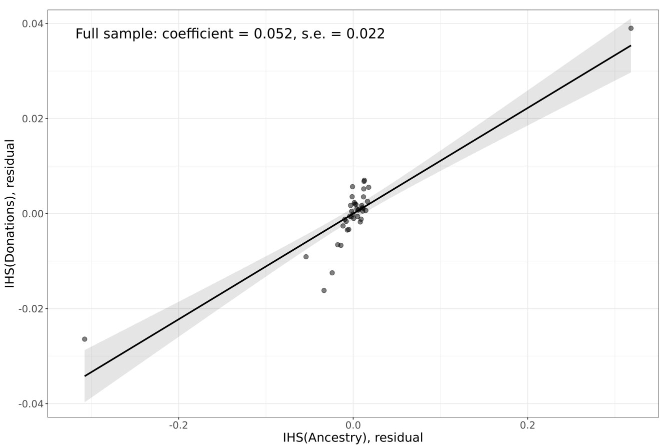
Figure 5: Binned scatter plots: IHS(# donations), all countries



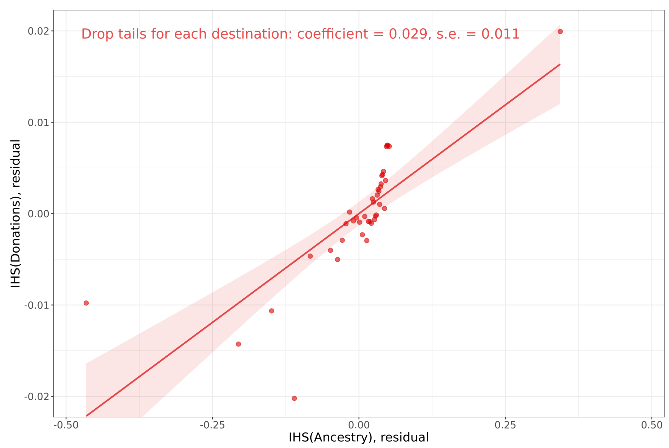
(a) Charity 1, full sample



(b) Charity 1, dropping tails of fitted values



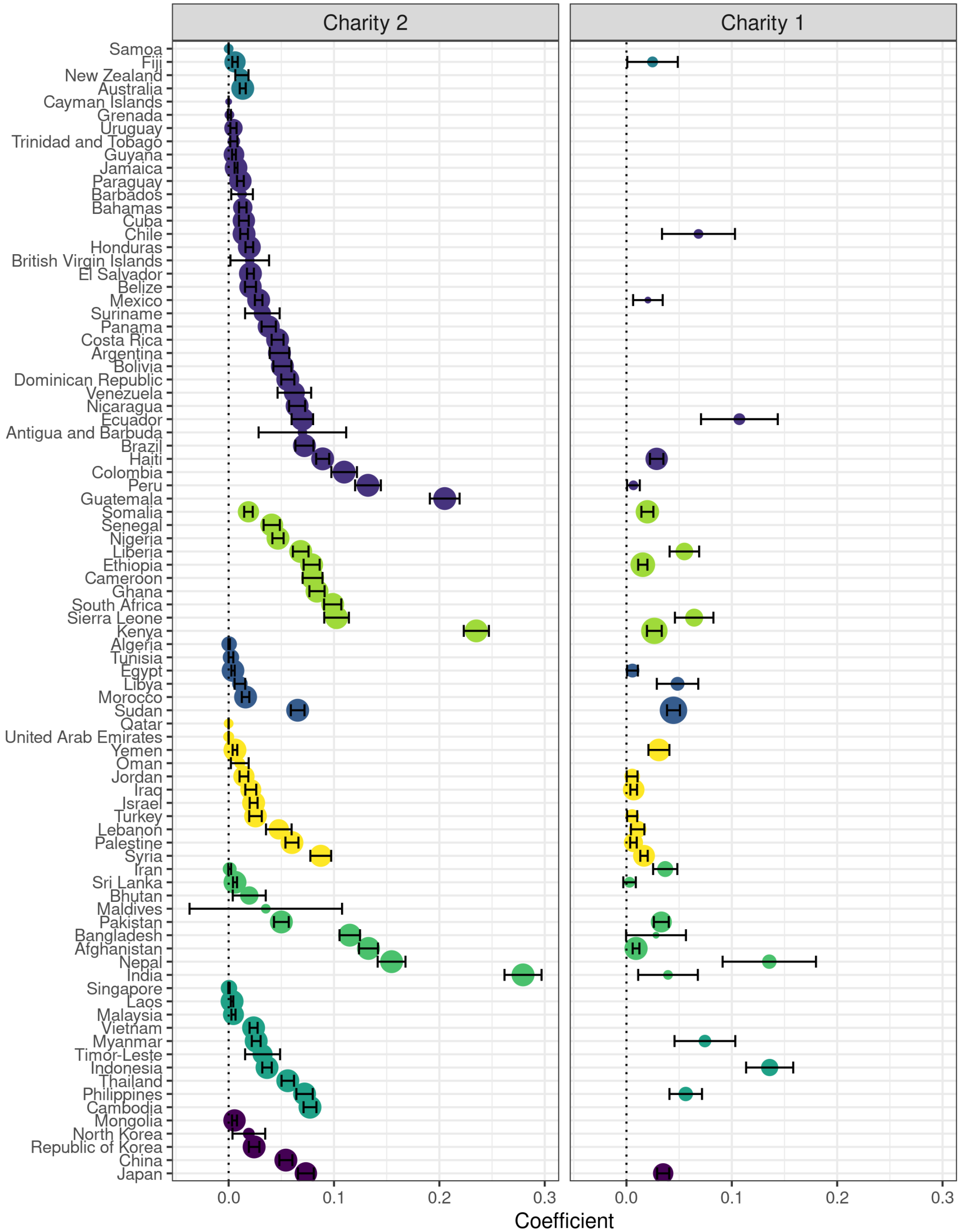
(c) Charity 2, full sample



(d) Charity 2, dropping tails of fitted values

Notes: Figure 5 presents binned scatter plots displaying the relationship between the fitted values of county's population from a given country and the county's IHS-transformed number of donations to that country in a given quarter. Panels A and B consider Charity 1 (C1) dataset; Panels C and D consider the Charity 2 (C2) dataset. The main variable of interest is the IHS-transformed population with ancestry from country d: year 2000 for C1 and year 2010 for C2. 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. We residualize outcomes and instruments by the controls used in Table 7. Panels B and D present regression lines dropping the top and bottom 2.5% of fitted values for each country. Standard errors are clustered at the foreign country level. 95% confidence intervals are reported.

Figure 6: Effect of ancestry on donations, by destination country



Notes: Figure 6 presents estimates from split-sample regressions of the IHS-transformed number of donations flowing from each county to the country in each row, separately by receiving country. The main variable of interest in each row is the county's IHS-transformed 2010 ancestry from that country. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for IHS-transformed 2010 population and time fixed effects. Standard errors are robust. We report 95% confidence intervals.

Online Appendix

"The Immigrant Next Door"

Leonardo Bursztyn

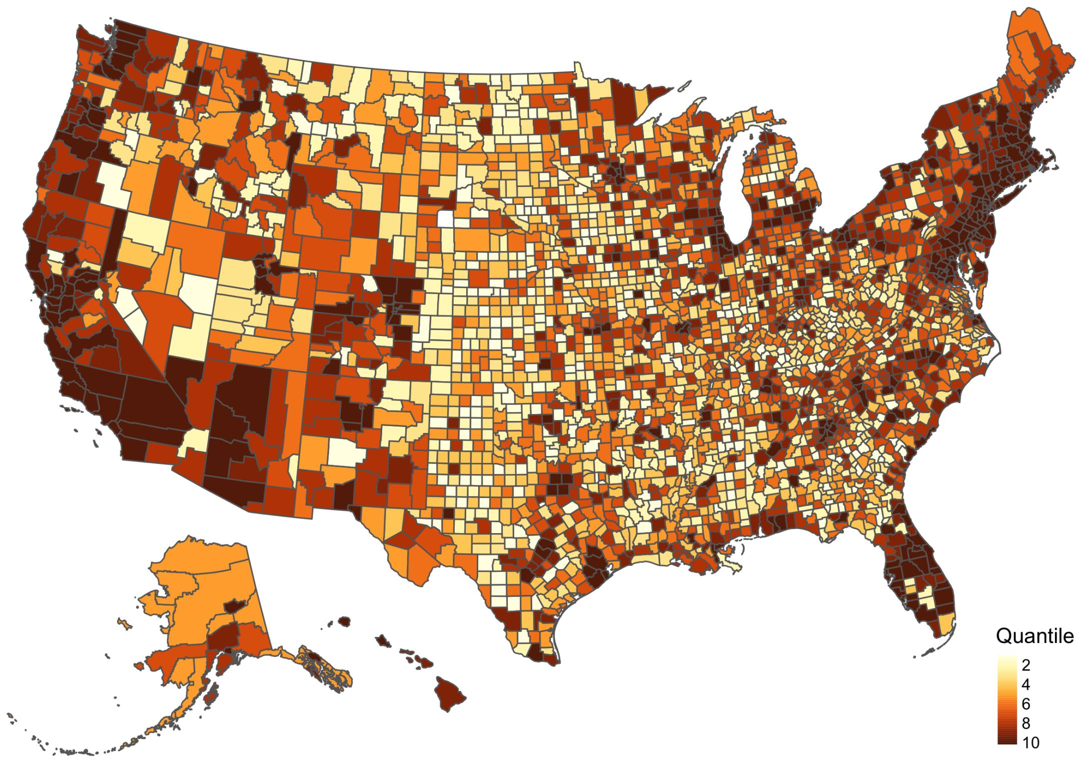
Thomas Chaney

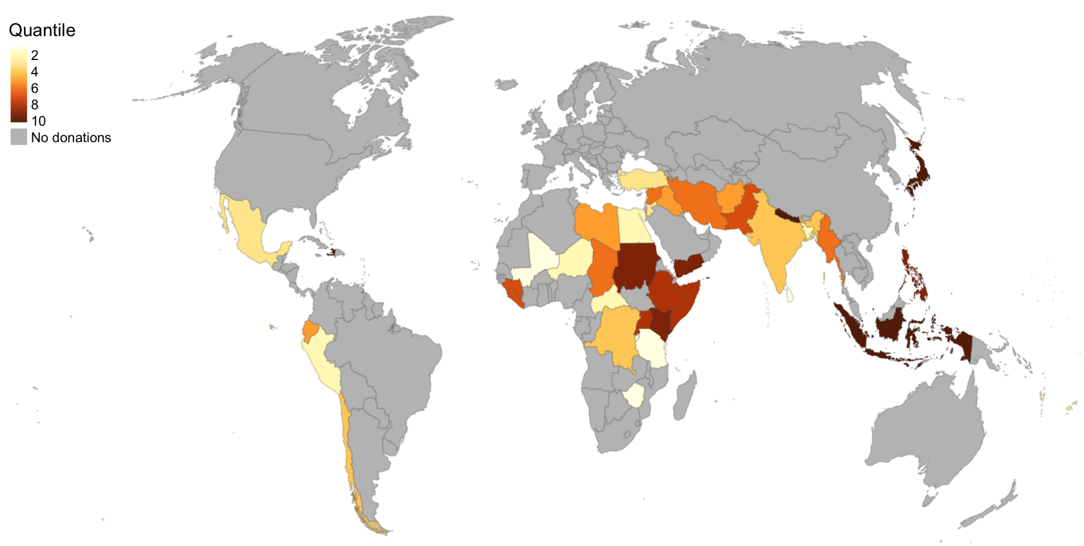
Tarek A. Hassan

Aakaash Rao

A Additional Tables and Figures

Appendix Figure A1: Charity 1 donations by origin (top) and destination (bottom)



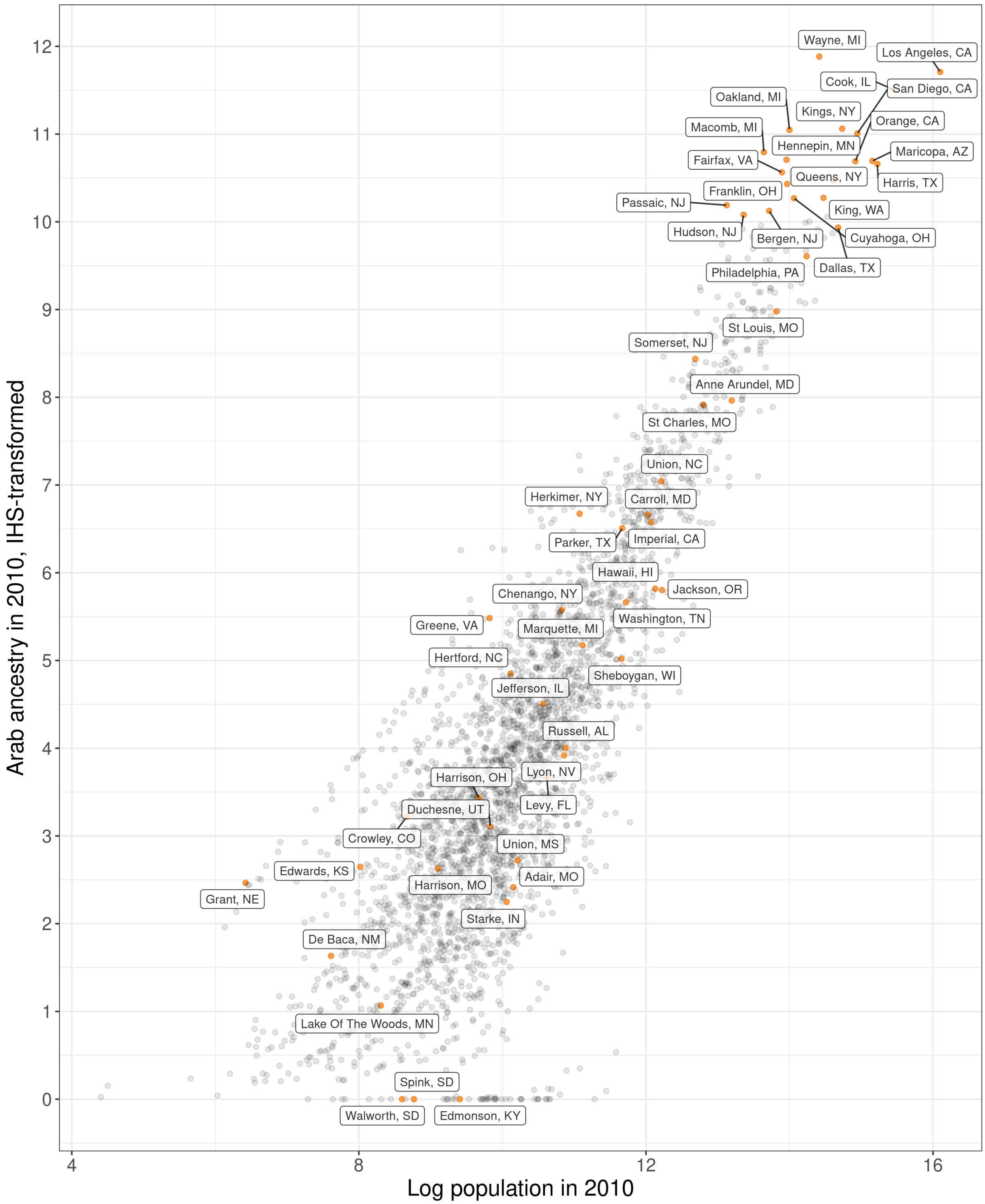


Appendix Table A1: Summary Statistics

	Obs.	Mean	Std. dev.	Median	Min	Max
Panel A: County level						
A.1: Ancestry						
2000 Arab ancestry	3,112	345.67	2165.36	15.84	0.00	60,834.34
2000 IHS-transformed Arab ancestry	3,112	3.67	2.19	3.46	0.00	11.71
2010 Arab ancestry	3,112	446.33	2636.79	21.12	0.00	72,415.13
2010 IHS-transformed Arab ancestry	3,112	4.00	2.13	3.74	0.00	11.88
Panel B: Individual level						
B.1: Project Implicit						
Arab-Muslim IAT score	59,862	0.012	0.991	-0.014	-4.123	4.426
Warmth toward Arab-Muslims	59,862	0.007	1.002	-0.154	-2.413	2.106
B.2: Nationscape						
Support for the Muslim Ban	127,779	0.529	0.499	1.000	0.000	1
Voted for Trump in 2016	127,779	0.582	0.493	1.000	0.000	1
Favorability toward Arab-Muslims	228,629	-0.126	1.006	0.316	-1.667	1.308
B.3: CCES						
Support for the Muslim Ban	228,629	0.306	0.461	0.000	0.000	1
Voted for Trump in 2016	228,629	0.532	0.499	1.000	0.000	1
Panel C: County-quarter level						
C.1: Donations to Arab countries, Charity 1						
Number of donations	168,102	0.058	1.557	0.000	0.000	428
C.2: Donations to Arab countries, Charity 2						
Number of donations	99,616	0.144	1.388	0.000	0.000	85
Dollar value of donations	99,616	17.41	235.83	0.00	0.00	20,000
Panel D: County-country-quarter level						
D.1: Ancestry, Charity 1 countries						
2000 population from country d (thousands)	2,195,594	0.052	2.405	0.000	0.000	2,483.578
2000 IHS-transformed population from country d	2,195,594	0.764	1.629	0.000	0.000	15.418
D.2: Ancestry, Charity 2 countries						
2010 population from country d (thousands)	9,482,714	0.183	7.009	0.000	0.000	2,629.375
2010 IHS-transformed population from country d	9,482,714	1.257	2.073	0.013	0.000	15.475
D.3: Donations, Charity 1						
Number of donations to country d	2,195,594	0.030	1.575	0.000	0.000	947
D.4: Donations, Charity 2						
Number of donations to country d	9,482,714	0.034	1.525	0.000	0.000	1,305
Dollar value of donations to country d	9,482,714	4.02	227.65	0.00	0.00	184,327

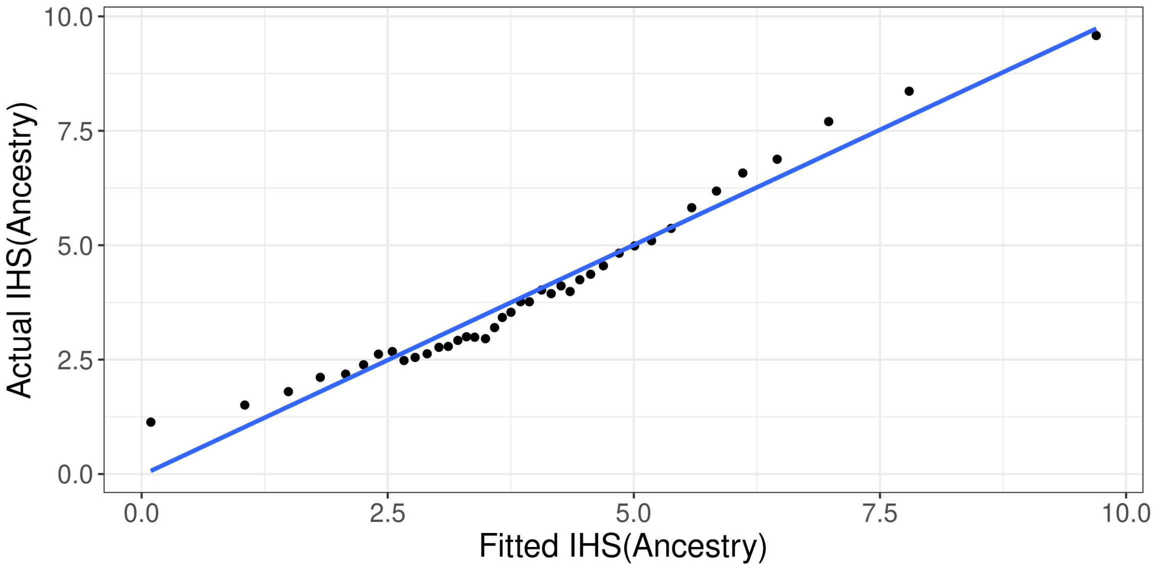
Notes: The table presents summary statistics for all datasets used in the main analyses except the custom survey (summary statistics for which are presented in Appendix Table A4). Donations statistics are calculated from the full set of Charity 1 and Charity 2 donors which we can match to a county (that is, no donors are dropped on the basis of their name's predicted ancestry).

Appendix Figure A2: Arab-ancestry population across counties

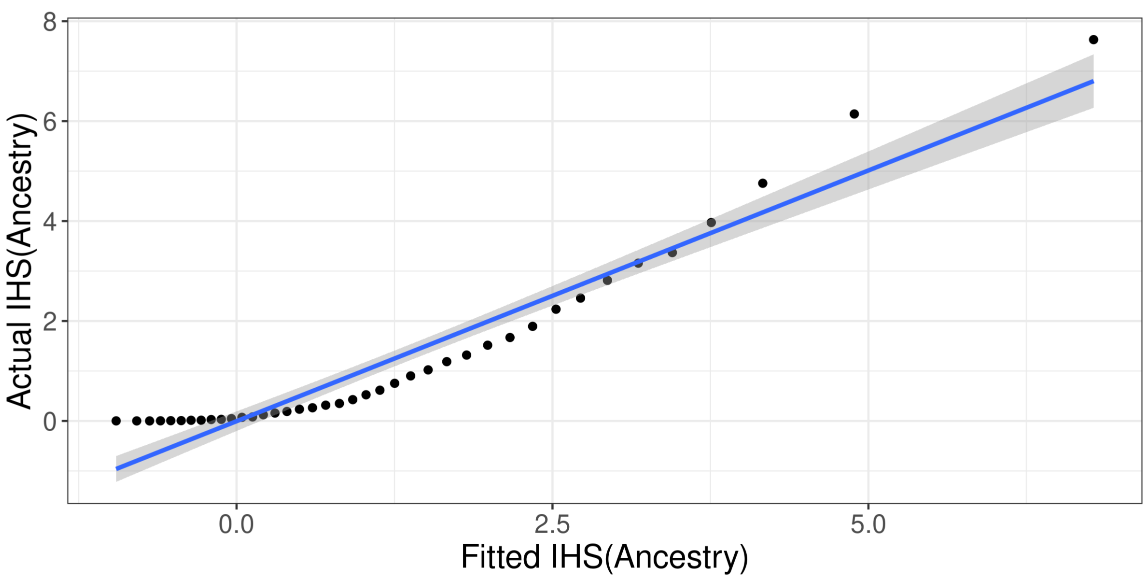


Notes: Figure A2 plots US counties, with logged 2010 population on the x-axis and the IHS-transformed population of Arab ancestry in 2010 on the y-axis.

Appendix Figure A3: Fitted vs. actual values of population



(a) Total population from Arab countries



(b) Population across all countries

Notes: Appendix Figure A3 presents binned scatter plots of the fitted against the actual values of ancestry. Panel A considers total Arab ancestry; Panel B considers all countries in our donations data. We instrument ancestry using $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments.

Appendix Table A2: Effect of Arab Ancestry on Attitudes toward Arab-Muslims (forced and unforced respondents)

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Panel A: Project Implicit	*Score on Arab-Muslim IAT (std., higher score = less prejudiced)*						
IHS(Arab ancestry)	0.014***	0.061***	0.055***	0.083***	0.087***	0.061***	0.067***
	(0.005)	(0.016)	(0.019)	(0.018)	(0.019)	(0.016)	(0.017)
Age			−0.012***	−0.012***	−0.012***	−0.013***	−0.013***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age squared			0.00004***	0.00005***	0.00005***	0.00005***	0.00005***
			(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Male			−0.165***	−0.166***	−0.165***	−0.165***	−0.165***
			(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
IHS(non-Euro ancestry)					−0.030**		
					(0.013)		
Avg. race IAT score						0.027***	
						(0.005)	
2012 Rep. vote share							−0.159***
							(0.041)
AP F-statistic	—	12.91	11.54	11.47	13.09	10.92	10.55
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	139,166	139,166	137,277	137,182	137,182	137,182	137,182
Panel B: Project Implicit	*Warmth toward Arab-Muslims (std., higher score = more favorable)*						
IHS(Arab ancestry)	0.030***	0.109***	0.101***	0.128***	0.119***	0.091***	0.091***
	(0.008)	(0.018)	(0.030)	(0.025)	(0.027)	(0.022)	(0.024)
AP F-statistic	—	12.91	11.58	11.53	13.14	10.96	10.58
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	139,600	139,600	137,738	137,635	137,635	137,635	137,635
State FE	No	No	Yes	Yes	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes	Yes	Yes

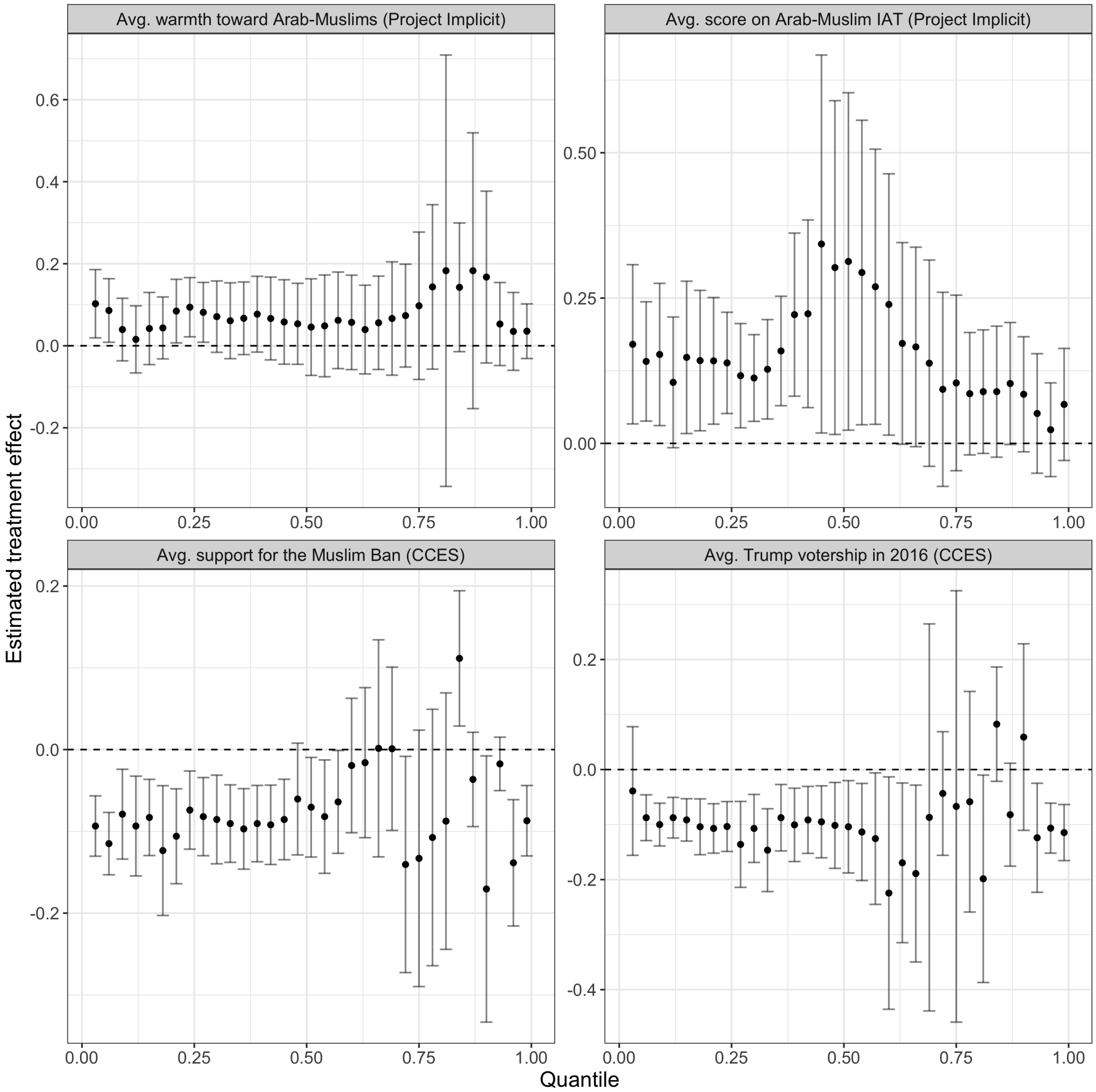
Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variable in Panel A is the score on the Arab-Muslim IAT (from Project Implicit); the dependent variable in Panel B is the stated warmth toward Arab-Muslims (also from Project Implicit). Both measures are scaled to take mean zero and standard deviation one. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. County-level demographic controls include the 2010 population density, the share of the 1970 prime-age population with a high school education, and the share of the 1970 prime-age population with a college education. Standard errors are clustered at the county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A3: Effect of Arab Ancestry on Auxiliary Measures of Prejudice and Social Norms

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Panel A: Project Implicit	*Avoid prejudice due to social standards (std.)*						
IHS(Arab ancestry)	0.024^{***}	0.080^{**}	0.033	0.027	0.022	0.022	0.022
	(0.006)	(0.032)	(0.030)	(0.032)	(0.030)	(0.031)	(0.030)
AP F-statistic	—	13.01	10.19	10.55	12.04	10.05	10.07
Weak IV-robust p-value	—	< 0.05	> 0.10	> 0.10	> 0.10	> 0.10	> 0.10
Observations	59,570	59,570	58,826	58,798	58,798	58,798	58,798
Panel B: Project Implicit	*Avoid prejudice to avoid social disapproval (std.)*						
IHS(Arab ancestry)	0.031^{***}	0.096^{**}	0.019	−0.005	−0.005	−0.004	−0.005
	(0.008)	(0.041)	(0.032)	(0.029)	(0.030)	(0.029)	(0.030)
Weak IV-robust p-value	—	< 0.05	> 0.10	> 0.10	> 0.10	> 0.10	> 0.10
Panel C: Project Implicit	*Personally motivated to avoid prejudice (std.)*						
IHS(Arab ancestry)	0.025^{***}	0.093^{***}	0.070^{**}	0.087^{**}	0.072^{**}	0.065^{**}	0.055^{*}
	(0.009)	(0.031)	(0.036)	(0.035)	(0.033)	(0.032)	(0.033)
Weak IV-robust p-value	—	< 0.01	< 0.05	< 0.05	< 0.05	< 0.05	< 0.10
Panel D: Project Implicit	*Personally motivated to avoid stereotyping (std.)*						
IHS(Arab ancestry)	0.022^{***}	0.093^{***}	0.081^{**}	0.097^{**}	0.092^{**}	0.074^{*}	0.064
	(0.009)	(0.034)	(0.041)	(0.042)	(0.043)	(0.041)	(0.042)
Weak IV-robust p-value	—	< 0.01	< 0.05	< 0.05	< 0.05	< 0.10	< 0.10
State FE	No	No	Yes	Yes	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes	Yes	Yes
Non-European population	No	No	No	No	Yes	No	No
Avg. race IAT score	No	No	No	No	No	Yes	No
2012 Rep. vote share	No	No	No	No	No	No	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. The dependent variables represent agreement with the statements "Because of today's standards I try to appear nonprejudiced toward Arab Muslims" (Panel A), "I attempt to appear nonprejudiced toward Arab Muslims in order to avoid disapproval from others" (Panel B), "I am personally motivated by my beliefs to be nonprejudiced toward Arab Muslims" (Panel C), "Because of my personal values, I believe that using stereotypes about Arab Muslims is wrong" (Panel D). All outcomes are scaled to mean zero and standard deviation one such that higher values indicate greater agreement with the statement. 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. County-level demographic controls include 2010 population density, share of 1970 prime-age population with high school education, and share of 1970 prime-age population with college education as controls. Standard errors are given in parentheses. Standard errors are robust in Panels A and B and are clustered at the congressional district level in Panel C. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Figure A4: Quantile regressions: effects on attitudes and political preferences



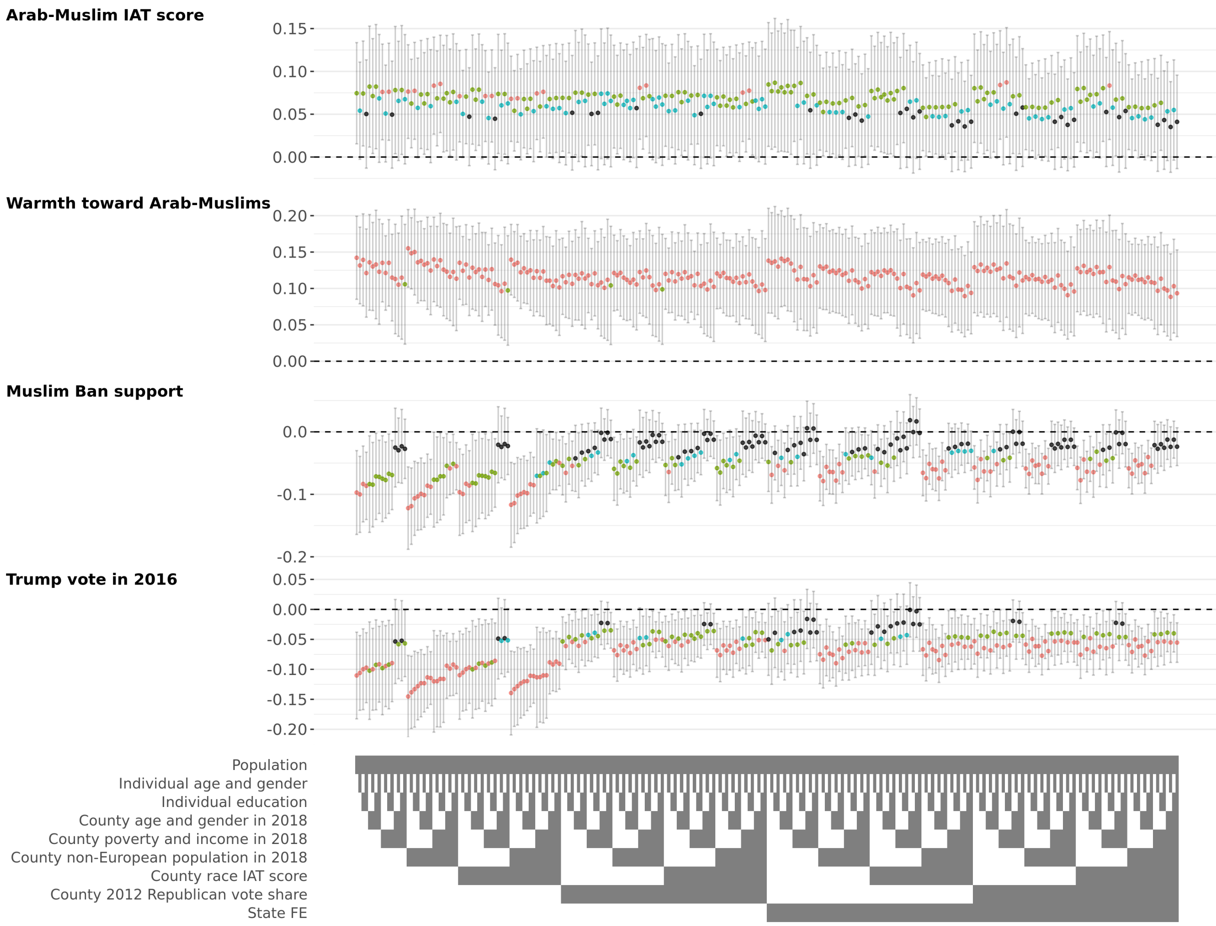
Notes: Figure A4 presents estimates from quantile regressions at the county level. The main variable of interest is the county's IHS-transformed 2010 ancestry from Arab League countries. We include $\{I^t_{f,-r(d)}(I^t_{-c(f),d}/I^t_{-c(f)})\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population and state fixed effects. Standard errors are robust. We report 95% confidence intervals.

Appendix Table A4: Survey Representativeness

	Survey mean	CCES mean
Age	52.389	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	5026	115930

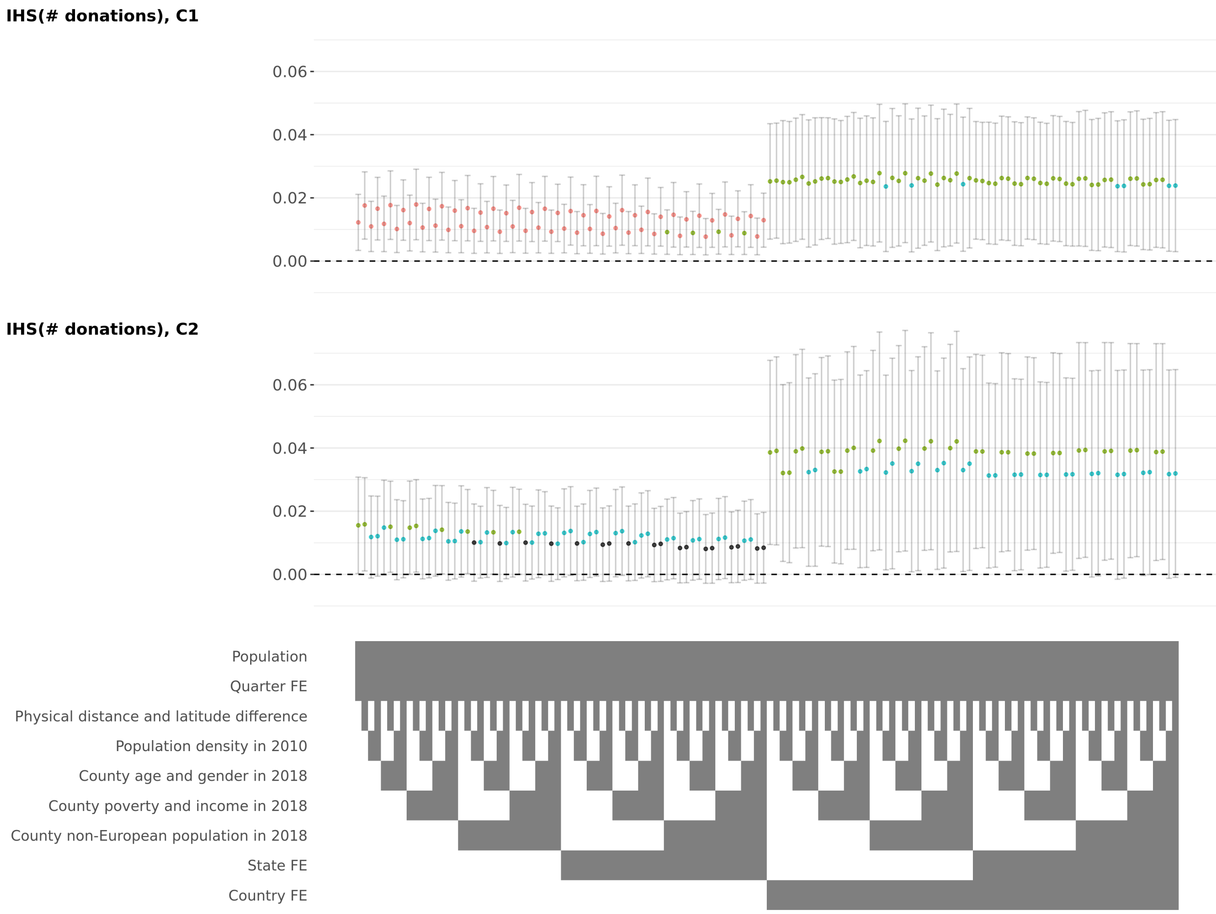
Notes: Column 1 presents means of respondent characteristics from our survey. Column 2 presents means of respondent characteristics from the 2016-2019 waves of the CCES.

Appendix Figure A5: Coefficient stability: effects on attitudes and political preferences



Notes: Figure A5 presents coefficient estimates from regressions at the test-taker level. Only white test-takers are included. The dependent variables in Panel 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); the dependent variable in Panel C is support for the Muslim Ban (from CCES); the dependent variable in Panel D is self-reported Trump votership in 2016 (also from CCES). The main variable of interest is the county's IHS-transformed 2010 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 demographic covariates include age, age squared, age × male, and a male indicator. Individual education include high school and college diploma indicators. County age and gender in 2018 include the share of the population below 18, the share of the population below 65, the median age, and the sex ratio. County education and economy in 2018 include the share of prime-age men and women with a high school education, the share of prime-age men and women with a college education, the share of the population below the poverty line, and the log median income. County non-European population in 2018 includes the IHS-transformed population of non-European ancestry. Standard errors are clustered at the county level. We report 95% confidence intervals. Red points, green points, and blue points are statistically significant at the 1%, 5%, and 10% levels, respectively; black points are insignificant at the 10% level.

Appendix Figure A6: Coefficient stability: effects on IHS(# donations to Arab countries)



Notes: Figure A6 presents coefficient estimates from regressions at the origin county-destination country-quarter level. Only donations to Arab League countries from donors with European-ethnicity names are included. The dependent variables in Panels A and B are the IHS-transformed number of donations from destination to origin in a quarter. The main variable of interest is county d's IHS-transformed 2010 ancestry from country f. 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. Standard errors are clustered at the foreign country level. We report 95% confidence intervals. Red points, green points, and blue points are statistically significant at the 1%, 5%, and 10% levels, respectively; black points are insignificant at the 10% level.

Appendix Table A5: Effect of Ancestry on Donations, Alternative Instruments

	(1) Standard	(2) Excl. corr. origins	(3) Eur. only pull
Panel A: Charity 1		Donations (dummy)	
IHS(Ancestry)	0.023***	0.021***	0.018***
	(0.007)	(0.006)	(0.005)
AP F-statistic	119.0	141.3	135.0
Observations	2,195,559	2,195,559	2,195,559
Panel B: Charity 1		IHS(# donations)	
IHS(Ancestry)	0.058***	0.059***	0.047***
	(0.015)	(0.016)	(0.012)
Panel C: Charity 2		Donations (dummy)	
IHS(Ancestry)	0.025**	0.034**	0.022**
	(0.011)	(0.014)	(0.009)
AP F-statistic	1202.4	176.5	803.2
Observations	9,482,679	9,482,679	9,482,679
Panel D: Charity 2		IHS(# donations)	
IHS(Ancestry)	0.052**	0.073**	0.048***
	(0.022)	(0.029)	(0.018)
Panel E: Charity 2		IHS($ donations)	
IHS(Ancestry)	0.153**	0.209**	0.136**
	(0.069)	(0.086)	(0.055)
Quarter FE	Yes	Yes	Yes
Origin county FE	Yes	Yes	Yes
Destination country FE	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donations from donors for whom the first-best or second-best classification of their name's ethnicity is not European and does not match the receiving country are included. The dependent variable in Panels A and C is a dummy for the presence of at least one donation from destination to origin in a quarter. The dependent variable in Panels B and D is the IHS-transformed number of donations from destination to origin in a quarter. The dependent variable in Panel E is the IHS-transformed total value of donations from destination to origin in a quarter. Column 2 uses an alternative construction of the instrument that excludes countries with correlated migrant flows. Column 3 uses an alternative construction of the instrument that calculates the pull factor based only on European emmigrants. The main variable of interest is the IHS-transformed population with ancestry from country d: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. All specifications control for origin county, destination country, and quarter fixed effects. We suppress the first-stage F-statistic and the number of observations in Panel B because they are identical to those in Panel A; we likewise suppress these statistics in Panels D and E because they are identical to those in Panel C. Standard errors are given in parentheses. Standard errors are robust. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A6: Effect of Ancestry on Donations, Excluding Different Countries

Countries excluded:	(1) None	(2) Arab	(3) Latino	(4) Arab & Latino	(5) African
Panel A: Charity 1			Donations (dummy)		
IHS(Ancestry)	0.023***	0.017***	0.011***	0.010***	0.038***
	(0.007)	(0.006)	(0.002)	(0.002)	(0.009)
Dep. var. mean	0.007	0.008	0.007	0.008	0.009
Dep. var. s.d.	0.082	0.088	0.082	0.088	0.093
First-stage F-statistic	807.7	588.9	109.7	463.7	1403.9
Observations	2,195,559	1,476,270	2,167,290	1,448,001	986,274
Panel B: Charity 1			IHS(# donations)		
IHS(Ancestry)	0.058***	0.042***	0.025***	0.024***	0.079***
	(0.015)	(0.012)	(0.006)	(0.006)	(0.027)
Dep. var. mean	0.009	0.011	0.009	0.011	0.012
Dep. var. s.d.	0.128	0.142	0.127	0.142	0.153
Panel C: Charity 2			Donations (dummy)		
IHS(Ancestry)	0.025**	0.023**	0.030*	0.027*	0.018
	(0.011)	(0.011)	(0.017)	(0.016)	(0.011)
Dep. var. mean	0.010	0.011	0.010	0.011	0.011
Dep. var. s.d.	0.101	0.103	0.102	0.105	0.102
First-stage F-statistic	1027.3	1019.1	385.4	348.6	502.8
Observations	9,482,679	8,452,431	7,645,194	6,614,946	5,707,197
Panel D: Charity 2			IHS(# donations)		
IHS(Ancestry)	0.052**	0.047**	0.062*	0.056*	0.035
	(0.022)	(0.021)	(0.034)	(0.032)	(0.021)
Dep. var. mean	0.013	0.013	0.013	0.014	0.013
Dep. var. s.d.	0.145	0.150	0.147	0.154	0.151
Panel E: Charity 2			IHS($ donations)		
IHS(Ancestry)	0.153**	0.138**	0.183*	0.164*	0.109
	(0.069)	(0.066)	(0.103)	(0.095)	(0.066)
Dep. var. mean	0.051	0.054	0.052	0.055	0.052
Dep. var. s.d.	0.526	0.541	0.532	0.551	0.536
Quarter FE	Yes	Yes	Yes	Yes	Yes
Origin county FE	Yes	Yes	Yes	Yes	Yes
Destination country FE	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donations from donors whose first-best or second-best classification of their name's ethnicity is not European and does not match the receiving country are included. The main variable of interest is the IHS-transformed population with ancestry from country d: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. Column 1 includes all countries; Column 2 excludes Arab League countries; Column 3 excludes Latino countries; Column 4 excludes both Arab League and Latino countries; Column 5 excludes African countries. All specifications control for origin county, destination country, and quarter fixed effects. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A7: Effect of Ancestry on Donations, Different Choices of Clustering

	(1)	(2)	(3)	(4)	(5)
Sample:	C1	C1	C2	C2	C2
Outcome:	1(donations)	# donations	1(donations)	# donations	$ donations
IHS(Ancestry)	0.023	0.058	0.025	0.052	0.153
Robust SE	(0.003)	(0.008)	(0.001)	(0.002)	(0.006)
Clustering: Destination country	(0.007)	(0.015)	(0.011)	(0.022)	(0.069)
Clustering: Origin county	(0.002)	(0.006)	(0.003)	(0.008)	(0.020)
Clustering: Origin state	(0.002)	(0.006)	(0.004)	(0.011)	(0.028)
2-way clustering: Country/county	(0.006)	(0.014)	(0.011)	(0.023)	(0.069)
2-way clustering: Country/state	(0.006)	(0.013)	(0.011)	(0.022)	(0.068)
Observations	2,195,559	2,195,559	9,482,679	9,482,679	9,482,679
Quarter FE	Yes	Yes	Yes	Yes	Yes
Origin county FE	Yes	Yes	Yes	Yes	Yes
Destination country FE	Yes	Yes	Yes	Yes	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. Only donations from donors whose first-best or second-best classification of their name's ethnicity is not European and does not match the receiving country are included. The dependent variable in Columns 1 and 3 are dummies for the presence of at least one donation from destination to origin in a quarter in Charity 1 (C1) data and in the Charity 2 (C2) data, respectively. The dependent variable in Columns 2 and 4 are the IHS-transformed number of donations from destination to origin in a quarter in the Charity 1 and Charity 2 data, respectively. The dependent variable in Column 5 is the IHS-transformed total value of donations from destination to origin in a quarter in the Charity 2 data. The main variable of interest is the IHS-transformed population with ancestry from country d: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. All specifications control for origin county, destination country, and quarter fixed effects. *, **, 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

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.

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.

C Percent Functional Form

Appendix Table C1: White Flight

	(1) Pooled Arab	(2) C1 Arab	(3) C2 Arab	(4) C1	(5) C2
Panel A: 1980 cross-section	*Selective white flight index*				
Percent country ancestry	13.765***	44.831***	43.696***	0.239***	0.087
	(0.850)	(8.781)	(8.194)	(0.013)	(0.059)
Dep. var. mean	8.665	8.662	8.717	8.148	7.980
Dep. var. s.d.	1.452	1.575	1.549	2.282	2.433
Observations	3,084	30,840	49,344	144,948	431,760
Panel B: 1980-2000 panel	*Selective white flight index*				
Percent country ancestry	9.688***	33.646***	33.945***	0.173***	0.088**
	(0.347)	(5.700)	(5.532)	(0.009)	(0.042)
Dep. var. mean	9.334	9.278	9.295	8.841	8.760
Dep. var. s.d.	1.529	1.676	1.646	2.198	2.241
Observations	9,333	93,340	149,344	401,138	1,225,360
Domestic state FE	Yes	Yes	Yes	No	No
Domestic county FE	No	No	No	Yes	Yes
Foreign country FE	No	Yes	Yes	Yes	Yes

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 3.3. 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 Column 1 is the percentage of the population with ancestry from Arab League countries; the endogenous variable in Columns 2–5 is the percentage of the population with ancestry from country d. 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 2–3 limit the sample to domestic county–foreign country pairs in which the foreign country is in the Arab League, separately for Charity 1 (C1) and Charity 2 (C2). Columns 4–5 include the full samples of county-country pairs from Charity 1 (C1) and Charity 2 (C2). Standard errors are given in parentheses. Standard errors are robust in Columns 1–4 and are clustered at the destination country level in Columns 5–8. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table C2: Effect of Arab Ancestry on Attitudes toward Arab-Muslims

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV	(7) IV
Panel A: Project Implicit	*Score on Arab-Muslim IAT (std., higher score = less prejudiced)*						
Percent Arab ancestry	0.051^{***}	0.076^{***}	0.089^{***}	0.099^{***}	0.107^{***}	0.079^{***}	0.081^{***}
	(0.015)	(0.016)	(0.017)	(0.018)	(0.021)	(0.019)	(0.018)
Age			-0.015^{***}	-0.015^{***}	-0.015^{***}	-0.015^{***}	-0.015^{***}
			(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age squared			0.0001^{***}	0.0001^{***}	0.0001^{***}	0.0001^{***}	0.0001^{***}
			(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Male			-0.129^{***}	-0.130^{***}	-0.130^{***}	-0.129^{***}	-0.129^{***}
			(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Percent non-Euro ancestry					-0.001		
					(0.001)		
Avg. race IAT score						0.017^{***}	
						(0.006)	
2012 Rep. vote share							-0.120^{**}
							(0.048)
AP F-statistic	—	13.50	14.06	13.80	8.638	10.79	9.706
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	58,987	58,987	58,247	58,220	58,220	58,220	58,220
Panel B: Project Implicit	*Warmth toward Arab-Muslims (std., higher score = more favorable)*						
Percent Arab ancestry	0.128^{***}	0.168^{***}	0.175^{***}	0.156^{***}	0.149^{***}	0.096^{***}	0.086^{***}
	(0.016)	(0.031)	(0.032)	(0.024)	(0.024)	(0.017)	(0.017)
AP F-statistic	—	13.69	14.14	13.93	8.678	10.86	9.787
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	58,796	58,796	58,068	58,040	58,040	58,040	58,040
Panel C: Nationscape	*Favorability toward Muslims (std., higher score = more favorable)*						
Percent Arab ancestry	0.147^{***}	0.248^{***}	0.190^{***}	0.153^{***}	0.108^{***}	0.115^{***}	0.080^{***}
	(0.024)	(0.058)	(0.038)	(0.033)	(0.028)	(0.031)	(0.026)
AP F-statistic	—	56.94	32.12	32.81	21.49	26.26	23.24
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Observations	187,435	187,435	187,435	187,435	187,435	187,435	187,435
State FE	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); and the dependent variable in Panel C is the stated favorability toward Muslims (from Nationscape). All three 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 percentage of the 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. County-level demographic controls include the 2010 population density, the share of the 1970 prime-age population with a high school education, and the share of the 1970 prime-age population with a college education. Standard errors are given in parentheses. Standard errors are clustered at the county level in Panels A and B and are clustered at the congressional district level in Panel C. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table C3: Effect of Arab Ancestry on Political Preferences

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) OLS	(7) IV	(8) IV	(9) IV	(10) IV
Panel A:	*Support for the Muslim Ban*									
	CCES					Nationscape				
Percent Arab ancestry	-0.106^{***}	-0.160^{**}	-0.131^{***}	-0.078^{**}	0.002	-0.034^{***}	-0.036^{**}	-0.010	-0.020	0.006
	(0.027)	(0.069)	(0.048)	(0.031)	(0.012)	(0.008)	(0.015)	(0.014)	(0.013)	(0.014)
Age			0.016^{***}	0.016^{***}	0.016^{***}			0.017^{***}	0.017^{***}	0.017^{***}
			(0.001)	(0.001)	(0.001)			(0.001)	(0.001)	(0.001)
Age squared			-0.0001^{***}	-0.0001^{***}	-0.0001^{***}			-0.0001^{***}	-0.0001^{***}	-0.0001^{***}
			(0.00001)	(0.00001)	(0.00001)			(0.00001)	(0.00001)	(0.00001)
Male			0.022	0.029^{*}	0.031^{**}			0.137^{***}	0.135^{***}	0.135^{***}
			(0.016)	(0.015)	(0.015)			(0.013)	(0.013)	(0.013)
2012 Rep. vote share					0.437^{***}					0.139^{***}
					(0.030)					(0.024)
AP F-statistic	—	14.12	12.42	10.19	7.812	—	56.28	31.09	32.29	22.68
Weak IV-robust p-value	—	< 0.05	< 0.01	< 0.01	> 0.10	—	> 0.10	> 0.10	> 0.10	> 0.10
Observations	56,814	56,814	56,814	56,814	56,729	58,183	58,183	58,183	58,183	58,183
Panel B:	*Voted for Trump in 2016*									
	CCES					Nationscape				
Percent Arab ancestry	-0.116^{***}	-0.188^{***}	-0.153^{***}	-0.105^{***}	-0.004	-0.084^{***}	-0.124^{***}	-0.092^{***}	-0.091^{***}	-0.034^{***}
	(0.026)	(0.072)	(0.048)	(0.036)	(0.011)	(0.011)	(0.024)	(0.016)	(0.016)	(0.011)
AP F-statistic	—	12.98	11.79	9.865	7.413	—	55.83	31.87	32.82	23.02
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01	> 0.10	—	< 0.01	< 0.01	< 0.01	< 0.05
Observations	77,800	77,800	77,800	77,800	77,679	170,190	170,190	170,190	170,190	170,190
State FE	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes	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, with Columns 1–5 using data from the CCES and Columns 6–10 using data from Nationscape. The dependent variable in Panel B is self-reported Trump votership, with Columns 1–5 again using data from the CCES and Columns 6–10 data from Nationscape. The main variable of interest is the 2010 percentage of the 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. County-level demographic controls include 2010 population density, share of 1970 prime-age population with high school education, and share of 1970 prime-age population with college education as controls. Standard errors are given in parentheses. Standard errors are clustered at the county level in Columns 1–5 and at the congressional district level in Columns 6–10. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table C4: Effect of Ancestry on Donations, Pooling Arab Countries (European-Ethnicity Donors Only)

	(1) OLS	(2) IV	(3) IV	(4) IV
Panel A: Charity 1	Donations (dummy)		*(mean* = 0.011*, sd* = 0.106*)*	
Percent Arab ancestry	0.086***	0.303***	0.310***	0.328***
	(0.011)	(0.045)	(0.072)	(0.098)
AP F-statistic	—	16.24	6.591	6.057
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01
Observations	168,102	168,102	168,102	168,102
Panel B: Charity 1	IHS(# donations)		*(mean* = 0.017*, sd* = 0.180*)*	
Percent Arab ancestry	0.133***	0.488***	0.491***	0.536***
	(0.018)	(0.081)	(0.121)	(0.167)
Panel C: Charity 2	Donations (dummy)		*(mean* = 0.041*, sd* = 0.198*)*	
Percent Arab ancestry	0.232***	0.685***	0.608***	0.596***
	(0.023)	(0.135)	(0.183)	(0.216)
AP F-statistic	—	13.89	6.465	7.357
Weak IV-robust p-value	—	< 0.01	< 0.01	< 0.01
Observations	99,616	99,616	99,616	99,616
Panel D: Charity 2	IHS(# donations)		*(mean* = 0.053*, sd* = 0.296*)*	
Percent Arab ancestry	0.365***	1.182***	1.019***	1.029***
	(0.044)	(0.258)	(0.327)	(0.398)
Panel E: Charity 2	IHS($ donations)		*(mean* = 0.208*, sd* = 1.063*)*	
Percent Arab ancestry	1.297***	3.871***	3.362***	3.315***
	(0.140)	(0.785)	(1.020)	(1.213)
Demographic controls	No	No	Yes	Yes
State FE	No	No	No	Yes
Quarter FE	Yes	Yes	Yes	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 in Panels A and C is a dummy for the presence of at least one donation from the county to any Arab League country in a quarter. The dependent variable in Panels B and D is the IHS-transformed number of donations from the county to Arab League countries in a quarter. The dependent variable in Panel E is the IHS-transformed total value of donations from the county to Arab League countries in a quarter. The main variable of interest is the percentage of the population with ancestry from Arab countries: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. All specifications control for quarter fixed effects. Columns 3–4 include 2010 population density, share of 1970 prime-age population with high school education, and share of 1970 prime-age population with college education. Column 4 includes state fixed effects. We suppress the first-stage F-statistic and the number of observations in Panel B because they are identical to those in Panel A; we likewise suppress these statistics in Panels D and E because they are identical to those in Panel C. 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 C5: Mechanisms: Contact with and Knowledge of Arab-Muslims

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Survey	*Contact with Arab-Muslims*						
	Restaurant	Friends	Workplace	Neighbors	Any (2–4)		
Percent Arab ancestry	0.320***	0.069**	0.175***	0.242***	0.252***	0.157*	0.142***
	(0.094)	(0.035)	(0.051)	(0.060)	(0.098)	(0.087)	(0.049)
Dep. var. mean	0.439	0.098	0.286	0.198	0.396	0.396	0.396
Dep. var. std. dev	0.496	0.297	0.452	0.398	0.489	0.489	0.489
AP F-statistic	11.59	11.59	11.59	11.24	11.24	5.643	3.942
Weak IV-robust p-value	< 0.05	< 0.05	< 0.05	> 0.10	< 0.05	> 0.10	> 0.10
Observations	5,189	5,189	5,189	5,189	5,189	5,189	5,189
Panel B: Survey	*Knowledge of Arab-Muslims*						
	Subservice/war	Pillars	Ramadan	Pop. accuracy	Index (2–4)		
Percent Arab ancestry	−0.265**	0.824**	0.178**	2.181**	0.557***	0.284*	0.065
	(0.112)	(0.339)	(0.079)	(1.003)	(0.209)	(0.158)	(0.097)
Dep. var. mean	0.590	4.493	0.764	-15.057	0.000	0.000	0.000
Dep. var. std. dev	0.758	1.558	0.425	13.612	1.000	1.000	1.000
AP F-statistic	11.59	11.59	11.59	11.24	11.24	5.643	3.942
Weak IV-robust p-value	< 0.05	< 0.05	< 0.05	> 0.10	< 0.05	> 0.10	> 0.10
Observations	5,014	5,014	5,014	4,724	4,724	4,724	4,724
Individual demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-level demographics	No	No	No	No	No	Yes	Yes
Political controls	No	No	No	No	No	No	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. In Panel A, the dependent variable in Column 1 is an indicator for whether the respondent reports having ever eaten at a Middle Eastern restaurant; the dependent variables in Columns 2–4 are indicators for whether the respondent has an Arab-Muslim friend, workplace acquaintance, or neighbor, respectively; and the dependent variable in Columns 5–7 is an indicator taking value one if any of the indicators in Columns 2–4 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 Columns 5–7 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 percentage of the 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. Individual demographics include age, male, age squared, and age $\times$ male. County-level demographics include the 2010 population density, the share of 1970 prime-age population with high school education, and the share of 1970 prime-age population with college education as controls. Political controls include both controls for individual voting in 2012 and the 2012 county Republican vote share. 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 C6: Effect of Ancestry on Donations, Arab Countries and European-Ethnicity Donors Only

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV
Panel A: Charity 1		Donations (dummy)		*(mean = 0.005, sd = 0.068)*		
Percent country ancestry	0.047***	0.337***	0.472***	0.265***	0.213***	0.295***
	(0.004)	(0.026)	(0.032)	(0.034)	(0.030)	(0.059)
AP F-statistic	—	171.4	141.5	98.84	97.81	48.97
Observations	716,770	716,770	716,770	712,877	712,877	712,877
Panel B: Charity 1		IHS(# donations)		*(mean = 0.006, sd = 0.091)*		
Percent country ancestry	0.060***	0.529***	0.739***	0.420***	0.316***	0.462***
	(0.005)	(0.044)	(0.055)	(0.055)	(0.047)	(0.092)
Panel C: Charity 2		Donations (dummy)		*(mean = 0.006, sd = 0.076)*		
Percent country ancestry	0.078***	0.731***	0.639***	0.343***	0.286***	0.665***
	(0.003)	(0.024)	(0.020)	(0.018)	(0.018)	(0.034)
AP F-statistic	—	262.6	257.3	204.3	193.7	83.26
Observations	1,029,264	1,029,264	1,029,264	1,022,704	1,022,704	1,022,704
Panel D: Charity 2		IHS(# donations)		*(mean = 0.006, sd = 0.095)*		
Percent country ancestry	0.096***	0.958***	0.829***	0.446***	0.383***	0.870***
	(0.004)	(0.039)	(0.033)	(0.027)	(0.027)	(0.051)
Panel E: Charity 2		IHS($ donations)		*(mean = 0.028, sd = 0.387)*		
Percent country ancestry	0.398***	3.717***	3.237***	1.718***	1.448***	3.380***
	(0.015)	(0.134)	(0.113)	(0.095)	(0.095)	(0.180)
Distance controls	No	No	Yes	Yes	Yes	Yes
Demographic controls	No	No	No	Yes	Yes	Yes
Origin state FE	No	No	No	No	Yes	Yes
Destination country FE	No	No	No	No	No	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donations to Arab League countries from donors with European-ethnicity names are included. The dependent variable in Panels A and C is a dummy for the presence of at least one donation from destination to origin in a quarter. The dependent variable in Panels B and D is the IHS-transformed number of donations from destination to origin in a quarter. The dependent variable in Panel E is the IHS-transformed total value of donations from destination to origin in a quarter. The main variable of interest is the percentage of the population with ancestry from country d: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. All specifications control for quarter fixed effects. Columns 3–6 include logged county-country distance and latitude difference. Columns 4–6 includes the 2010 population density, the share of 1970 prime-age population with high school education, and the share of 1970 prime-age population with college education. Columns 5–6 include origin state fixed effects. Column 6 includes destination country fixed effects. We suppress the first-stage F-statistic and the number of observations in Panel B because they are identical to those in Panel A; we likewise suppress these statistics in Panels D and E because they are identical to those in Panel C. Standard errors are given in parentheses. Standard errors are robust. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table C7: Effect of Ancestry on Donations, All Countries, Different Populations of Donors

	(1) Europeans	(2) Other continents	(3) All
Panel A: Charity 1		Donations (dummy)	
Percent country ancestry	0.004**	0.005***	0.005**
	(0.002)	(0.002)	(0.002)
Dep. var. mean	0.007	0.007	0.008
Dep. var. s.d.	0.082	0.086	0.089
First-stage F-statistic	1004540.9	997219.4	1006415.4
Observations	2,187,870	2,187,870	2,187,870
Panel B: Charity 1		IHS(# donations)	
Percent country ancestry	0.008	0.009	0.010
	(0.006)	(0.006)	(0.006)
Dep. var. mean	0.009	0.010	0.011
Dep. var. s.d.	0.128	0.137	0.144
Panel C: Charity 2		Donations (dummy)	
Percent country ancestry	0.003**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)
Dep. var. mean	0.010	0.011	0.012
Dep. var. s.d.	0.101	0.105	0.107
First-stage F-statistic	550587.2	574420.4	582320.6
Observations	9,473,622	9,473,622	9,473,622
Panel D: Charity 2		IHS(# donations)	
Percent country ancestry	0.005**	0.005**	0.005**
	(0.002)	(0.002)	(0.002)
Dep. var. mean	0.013	0.014	0.015
Dep. var. s.d.	0.145	0.153	0.158
Panel E: Charity 2		IHS($ donations)	
Percent country ancestry	0.015**	0.016**	0.016**
	(0.006)	(0.007)	(0.007)
Dep. var. mean	0.051	0.055	0.058
Dep. var. s.d.	0.526	0.548	0.564
Origin county FE	Yes	Yes	Yes
Destination country FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Donations are dropped when the first-best or second-best classification of their name's ethnicity matches the receiving country. Column 1 additionally limits the sample to European donors, while Column 2 additionally limits the sample to donors whose name is matched to a country on a different continent than the receiving country. The dependent variable in Panels A and C is a dummy for the presence of at least one donation from destination to origin in a quarter. The dependent variable in Panels B and D is the IHS-transformed number of donations from destination to origin in a quarter. The dependent variable in Panel E is the IHS-transformed total value of donations from destination to origin in a quarter. The main variable of interest is the percentage of the population with ancestry from country d: year 2000 for Charity 1 and year 2010 for Charity 2. In 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. All specifications control for origin county, destination country, and quarter fixed effects. We suppress the first-stage F-statistic and the number of observations in Panel B because they are identical to those in Panel A; we likewise suppress these statistics in Panels D and E because they are identical to those in Panel C. Standard errors are given in parentheses. Standard errors are clustered at the destination country level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table C8: Heterogeneity by Physical and Cultural Distance

	(1)	(2)	(3)	(4)	(5)
Panel A: Charity 1			*IHS(# donations)*		
Percent country ancestry	0.008	0.112^{**}	0.070^{***}	0.069^{***}	0.052^{***}
	(0.006)	(0.050)	(0.012)	(0.011)	(0.007)
Percent country ancestry × physical distance		0.018^{**}			
		(0.008)			
Percent country ancestry × genetic distance			0.109^{***}		
			(0.021)		
Percent country ancestry × linguistic distance				0.013^{***}	
				(0.002)	
Percent country ancestry × religious distance					0.023^{***}
					(0.004)
Observations	2,187,870	2,187,870	2,031,370	2,119,010	2,119,010
Panel B: Charity 2			*IHS(# donations)*		
Percent country ancestry	0.005^{**}	0.147^{**}	0.013	0.010	0.111
	(0.002)	(0.063)	(0.008)	(0.022)	(0.106)
Percent country ancestry × physical distance		0.051^{**}			
		(0.022)			
Percent country ancestry × genetic distance			0.017		
			(0.014)		
Percent country ancestry × linguistic distance				0.010	
				(0.041)	
Percent country ancestry × religious distance					0.070
					(0.069)
Observations	9,473,622	9,473,622	8,638,914	9,093,924	8,698,536
Origin county FE	Yes	Yes	Yes	Yes	Yes
Destination country FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the county-country-quarter level. Only donors with a European-ethnicity name are kept. The dependent variable in both panels is the IHS-transformed number of donations. The main variable of interest is the percentage of the population with ancestry from country d — year 2000 for Charity 1 and year 2010 for Charity 2 — and the interaction of this variable with a measure of distance: log physical distance in Column 2, genetic distance in Column 3, linguistic distance in Column 4, and religious distance in Column 5 (all distance measures are standardized). In all specifications, 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; we also include as excluded instruments the interaction of the corresponding distance measure with all of the excluded instruments listed above. All specifications control for origin, destination, and quarter fixed effects. Standard errors are given in parentheses. Standard errors are clustered at the origin county level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

D 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

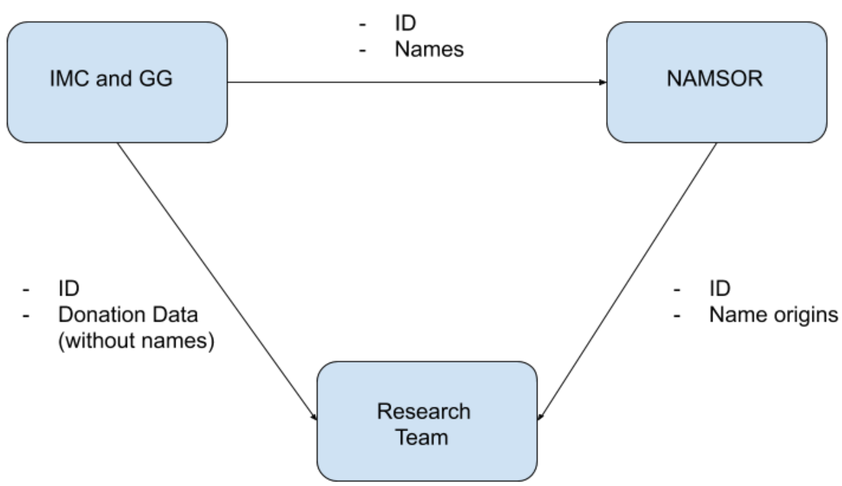
3. Based on these names, NAMSOR determines the most likely origin country of the name

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 D1.

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



Appendix Figure D1: Data Flow for Privacy

E 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!