American Economic Review 2024, 114(2): 348–384
https://doi.org/10.1257/aer.20220376

The Immigrant Next Door†

By Leonardo Bursztyn, Thomas Chaney, Tarek A. Hassan, and Aakaash Rao*

We study how decades-long exposure to individuals of a given foreign descent shapes natives' attitudes and behavior toward that group. Using individualized donations data, we show that long-term exposure to a given foreign ancestry leads to more generous behavior specifically toward that group's ancestral country. Focusing on exposure to Arab Muslims to examine mechanisms, we show that long-term exposure (i) decreases 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 Muslims, and (v) increases knowledge of Arab Muslims and Islam. (*JEL* D64, D83, D91, J15)

Many countries face growing challenges surrounding backlash against the presence of "nonnatives." As hypothesized by Allport (1954), and as empirically demonstrated in more recent work (e.g., Lowe 2021), the effects of specific forms of contact on attitudes and behavior depend heavily on the nature of interaction. Summing across all of the different forms of interaction that naturally occur between immigrants and natives, what is the aggregate effect on natives' beliefs and behavior?

In this paper, we show that the decades-long presence of immigrant groups induces more positive behavior and attitudes toward those groups. We combine several sources of data to measure the presence of, generosity toward, and prejudice against foreign-origin groups in the United States. In particular, we measure *presence* using variation in the number of residents of a US county who claim ancestry from a given foreign origin, and we measure *generosity* toward specific foreign countries using individualized data from two large charitable organizations, both of which channel donations from American donors to a large number of disaster-struck foreign countries in South America, Africa, Asia, and Oceania. Turning to mechanisms, we

* Bursztyn: University of Chicago and NBER (email: bursztyn@uchicago.edu); Chaney: University of Southern California, Sciences Po, and CEPR (email: thomas.chaney@gmail.com); Hassan: Boston University, NBER, and CEPR (email: thassan@bu.edu); Rao: Harvard University (email: arao@g.harvard.edu). Stefano DellaVigna was the coeditor for this article. We thank the four anonymous referees, Davide Cantoni, Michela Carlana, Ray Fisman, Nathan Nunn, Kevin Lang, Matt Lowe, Ricardo Perez-Truglia, Gautam Rao, Chris Roth, Marco Tabellini, Lisa Tarquinio, Eduardo Teso, Romain Wacziarg, Noam Yuchtman, and numerous seminar participants for helpful comments and suggestions. We thank Beyza Gulmezoglu, Andrew Kao, Andrei Kim, Ewan Rawcliffe, Thomas Yu, and Christoph Ziegler 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 N337272 and N884847).
† Go to https://doi.org/10.1257/aer.20220376 to visit the article page for additional materials and author disclosure statements.

measure *attitudes* toward a specific foreign-origin group of particular relevance to the policy debate, Arab Muslims, using the Implicit Association Test, survey data on explicitly stated warmth, voting for presidential candidate Donald Trump, and support for Trump's proposed "Muslim Ban" in 2016. Finally, we measure actual *contact* with and *knowledge* about Arab Muslims through a large-scale custom survey. In sum, we find that exposure to descendants of a given group increases natives' generosity toward that group, lowers prejudice against that group, and increases personal contact with and knowledge about that group.

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

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

We find that a larger local population with ancestry from a given foreign country substantially increases donations from European-ancestry residents to that foreign country. This estimated effect of exposure operates on both the extensive and intensive margins of donations and is economically significant: a 1 percent increase in foreign ancestry increases the number of donations by approximately 0.1 percent and the dollar value of donations by approximately 0.3 percent. We show evidence this effect operates not just at the county level but also at the aggregate (commuting zone and state) level. Horse racing the effect of exposure to first-generation immigrants against the effects of exposure to foreign ancestry, which includes second- and higher-generation immigrants, we find evidence that on the margin, exposure to people of a given foreign ancestry, but who were born in the United States, has a positive and significant effect on donations to their ancestral country, whereas additional exposure to foreign-born immigrants has a null effect on donations.

Even though these results condition on county fixed effects and quasi-random variation in the ancestral composition of US counties, different types of "natives" might still selectively move within the United States to avoid living near descendants of migrants from specific origins. If such "selective White flight" were large enough in magnitude, it could bias our estimated effects of contact. Using 30 years of detailed census data on internal migration, we show that none of our results are attributable to such endogenous sorting of the native population. On average, White Americans do not react to the presence of descendants of foreign migrants from a given country by moving to counties with smaller populations of that ancestral group, nor does this null effect mask significant heterogeneity by subgroup. We conclude that the effect of ancestry on donations is indeed causal.

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

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

Taking the evidence together, we conclude that natives' greater charitable donations toward a foreign-origin group's ancestral country, their more positive explicit and implicit attitudes toward that group, their lower support for policies and

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

candidates hostile toward that group, and their greater contact with and knowledge of that group are driven by that group's long-term presence. The long-term presence of minority foreign groups, summing up over all types of day-to-day interactions with natives, induces more favorable behavior and attitudes toward them.

Related Literature.—Our paper contributes to a large literature studying the effect of intergroup contact on attitudes and discrimination, building on the seminal work by Allport (1954). Given the selection issues inherent to most observational designs studying contact, much of this literature relies on randomized experiments.[2] Other papers exploit natural experiments, such as the random assignment of roommates or classmates (Boisjoly et al. 2006; Rao 2019; Carrell, Hoekstra, and West 2019; Corno, La Ferrara, and Burns 2022; Scacco and Warren 2018; Billings, Chyn, and Haggag 2021), the random composition of military boot camp cohorts (Dahl, Kotsadam, and Rooth 2021; Finseraas and Kotsadam 2017), or the random assignment of location for military or missionary deployment (Bagues and Roth 2023; Crawfurd 2021).

One important theme in this literature is persistence. Some studies (Schindler and Westcott 2021; Bazzi et al. 2019; Bagues and Roth 2023) find that the effects of contact persist over long periods, while others (Dahl, Kotsadam, and Rooth 2021; Enos 2014) find that effects fade out quickly. Recent work (Lowe 2021; Mousa 2020; Bazzi et al. 2019) has also documented considerable heterogeneity: contact may lead to more positive social preferences in some contexts, while having no effects or even negative effects in others.[3] Given these disparate findings, a crucial question concerns the *aggregate effect* of ancestral presence: summing up over all types of naturally occurring interactions over the course of decades, how does intergroup exposure shape beliefs and prejudices and translate into real-world behavior? Our data and identification strategy allow us to identify such causal effects on a comprehensive range of outcomes in the most natural possible setting—day-to-day interaction over decades.

Our paper also complements a growing body of work on the relationship between immigration, political attitudes, and voting behavior. Some work finds that higher immigration leads to greater support for right-wing parties,[4] while other work has found evidence in the opposite direction:[5] for instance, Calderon, Fouka, and Tabellini (2023) find that the Second Great Migration of African Americans to the Northern United States increased White people's support for the civil rights

[2] See Pettigrew and Tropp (2006) and Paluck, Green, and Green (2019) for meta-analyses of this literature. Experiments studying the effects of long-run contact on adults, rather than children, are especially scarce: Paluck, Green, and Green (2019) find that, at the time of writing, there were no randomized studies that show the effects of interracial and interethnic contact on adults over the age of 25, and there were only 3 such studies that quantify the effects more than a single day after treatment.

[3] For example, while Lowe (2021) and Mousa (2020) find that cooperative contact leads to more positive social behavior, Lowe (2021) finds that adversarial contact has the opposite effect, and Mousa (2020) finds that this more positive behavior is limited to specific contexts. Bazzi et al. (2019) exploit a population resettlement program to identify the long-run effects of intergroup contact on national integration in Indonesia and find that the program leads to greater integration in fractionalized communities with many small groups but has the opposite effect in polarized areas with a few large groups.

[4] See, for example, Barone et al. 2016; Halla, Wagner, and Zweimuller 2017; Dustmann, Vasiljeva, and Piil Damm 2019; Brunner and Kuhn 2018; Becker and Fetzer 2016; Colussi, Isphording, and Pestel 2021.

[5] See, for example, Dill 2013; Vertier, Viskanic, and Gamalerio 2023; Achard et al. 2022.

movement, while Tabellini (2020) shows increased immigration to US counties caused higher support for anti-immigration legislation, the election of more conservative legislators, and lower redistribution, despite the economic benefits generated for nonimmigrants. Steinmayr (2021) finds evidence of both positive and negative effects: the Far Right vote share is increased by short-term exposure to refugees but decreased by sustained contact.

Our work complements these results in multiple respects. We isolate the direct effect of exposure to out-groups on implicit and explicit attitudes and altruistic behavior toward these groups, thus shedding light on underlying mechanisms;[6] we examine the effects of the presence of dozens of different foreign ancestral groups over the period of decades, allowing us to control flexibly for county- and country-level confounders; and we offer evidence of mechanisms through which the long-term presence of individuals of foreign descent affects behavior and generosity.[7] Thus, we contribute to the extensive literature on cultural persistence and change by showing that the local presence of foreign groups changes long-term attitudes toward them (Alesina, Giuliano, and Nunn 2013; Giuliano and Nunn 2021).

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

I. Data

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

A. *Presence: Historical Migrations and Ancestry*

To quantify the presence of members of a given ethnicity, we collect data on the ancestral composition of US counties. We conjecture that a person living in county d with a larger community with ancestry from country f has a stronger exposure to that community (a conjecture we corroborate empirically in Section III). We

[6] Recent contributions have used Implicit Association Test (IAT) scores as a *predictor* of biased behaviors (Glover, Pallais, and Pariente 2017; Carlana 2019); we instead use these scores as an *outcome* and provide evidence that implicit bias can be shaped by exposure to out-groups, complementing recent work in other contexts (Lowes et al. 2015, 2017; Schindler and Westcott 2021).

[7] Fouka, Mazumder, and Tabellini (2022) finds that the Great Migration, which led millions of African Americans to migrate out of the rural South, improved White residents' views of immigrants and facilitated social integration of European immigrant groups. Similarly, Fouka and Tabellini (2022) find that Mexican immigration improves White residents' attitudes and behavior toward Black Americans.

use data from Burchardi, Chaney, and Hassan (2019a), which compiles immigration and ancestry data from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. 2015) samples of the 1880, 1900, 1910, 1920, 1930, 1970, 1980, and 1990 waves of the US census and from the 2006–2010 five-year sample of the American Community Survey (ACS).[8]

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 United States between $t-1$ and t (the interval between two consecutive census waves). Our stock ancestry variable, $Ancestry^t_{f,d}$, corresponds to the number of respondents in d at t who report ancestry from f; that is, this stock includes both US-born individuals with ancestry from country f and first-generation immigrants from country f. Our empirical strategy isolates quasi-random variation in this variable. Online Appendix Table A2 displays the number of individuals with ancestry from a given country, the peak arrival time of immigrants from that country, and the number of counties with nonzero ancestral presence from that country for the top ten countries in our dataset by national ancestral population. In total, 11.2 percent of individuals report ancestry from one of the 44 countries in our donations dataset. Online Appendix Figure A1 plots the fraction of individuals in each county who claim ancestry from a foreign country in our dataset (top panel) and the fraction of individuals in each county who claim ancestry from an Arab Muslim country in our dataset (bottom panel).

B. *Generosity: Charitable Donations*

To measure generosity toward foreign countries, we collect data on charitable donations toward foreign causes from two major charitable organizations, to which we refer as Charity 1 and Charity 2.[9] While both organizations occasionally donate to US-based causes, they primarily channel donations from US donors toward foreign nongovernmental organizations. We focus solely on donations to specific foreign countries, the vast majority of which occur immediately after a natural or man-made disaster in that country. After removing donors whom we are unable to match to a unique county of residence, we are left with 80,556 individual donations spanning 2004 to 2017 for Charity 1 and 715,663 individual donations spanning 2010 to 2017 for Charity 2. For each donation, the organizations know the name of the donor, the date of the donation, the foreign destination of the donation, and, for Charity 2 only, the dollar amount of the donation. Online Appendix Figure A2 maps the distribution of donors across US counties and the worldwide distribution of the receiving countries. Donations come from all parts of the United States; recipient countries are primarily in Africa, South Asia, and Latin America.

We pool donations across Charity 1 and Charity 2 and restrict our sample to the 44 recipient countries in both datasets, for all of which we have ancestry data from the census. To identify the likely ancestral country of origin of donors, we

[8] We discuss the procedure in detail in online Appendix II.A.

[9] Charity 1 requested anonymity. Charity 2 is GlobalGiving (https://www.globalgiving.org), "a nonprofit that has served disaster-impacted communities around the world since 2004, mainly by raising money from US donors to drive locally led responses to natural or man-made disasters." We publish the data in Bursztyn et al. (2024).

contract with NamSor, an organization that uses machine learning techniques on historical census data to classify names by ethnicity, gender, and religion. In our main specification, we restrict the sample to donors matched to European countries to approximate a population of White "natives."[10] Given that no recipient country in our dataset is in Europe, this restriction also ensures that our results are not driven by the natural tendency of individuals to donate to their own ancestral country. We then aggregate donations at the domestic county $d \times$ foreign country $f \times$ quarter t level.

C. *Implicit and Explicit Prejudice: IATs and Stated Warmth*

We draw data on implicit and explicit prejudice against Arab Muslims from two sources. The first source is Project Implicit, a platform through which respondents can complete Implicit Association Tests (IATs) quantifying subconscious prejudice against different groups. IAT scores are generally regarded as difficult to manipulate (Egloff and Schmukle 2002), and a number of studies have correlated these scores with real-world psychological responses and economic decision-making (Bertrand, Chugh, and Müllainathan 2005; Carlana 2019; Glover, Pallais, and Pariente 2017). We use data from all Arab Muslim, Asian, and race IATs taken before January 1, 2021 (Xu et al. 2014, 2013a,b). Subjects taking the IAT answer additional questions, including a measure of explicitly stated attitudes ("warmth") toward the group in question. Subjects also report their demographic characteristics and indicate their reason for taking the test. In order to assuage concerns about respondents endogenously selecting into taking the IAT, we classify respondents taking the test due to "Assignment for work" or "Assignment for school" as "forced respondents" and conduct our primary analyses with the 108,235 White, non-Muslim forced respondents to the Arab Muslim IAT. To ensure that our estimates generalize to a representative sample, we turn to Nationscape, a large-scale survey representative of the US population, administered by the Democracy Fund Voter Study Group and fielded between 2019 and 2020 (Tausanovitch and Vavreck 2021). In this survey, respondents explicitly state their favorability toward Muslims. We again restrict the sample to White, non-Muslim respondents. For comparability, we normalize all measures—implicit prejudice against Arab Muslims (Project Implicit), warmth toward Arab Muslims (Project Implicit), favorability toward Muslims (Nationscape)—to mean zero and standard deviation one, with higher values representing more positive attitudes.

D. *Political Choice: Muslim Ban Support and Trump Voting*

We assess how exposure to Arab Muslims shapes political choice by analyzing two distinct outcomes from the Cooperative Congressional Election Study (CCES) (Ansolabehere and Schaffner 2017, 2019; Ansolabehere, Schaffner, and Luks 2019, 2020; Kuriwaki 2023), a widely used representative and stratified survey tracking public opinion and political attitudes. First, we examine the effect of exposure to

[10] In particular, we restrict to donors matched to countries classified as European by the International Organization for Standardization. We validate the accuracy of this classification in online Appendix Section III.A. 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 nations are matched to the United States, while most Americans are matched to European countries.

individuals of Arab Muslim ancestry on support for the "Muslim Ban," proposed by Donald Trump during his 2016 presidential campaign and first implemented in January 2017.[11] As our second measure of political choice, we study voting behavior in the 2016 US presidential elections (MIT Election Data and Science Lab 2018). 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.[12] We thus in part attribute increases in Republican support between 2012 and 2016 to hostility toward Arab Muslims. Both CCES and Nationscape include questions eliciting respondents' support for the Muslim Ban and 2016 voting behavior. As before, we limit to White, non-Muslim respondents.

E. *Contact and Mechanisms: Reported Contact and Knowledge*

To further understand the mechanisms through which exposure to Arab Muslims shapes beliefs, we fielded a large-scale survey between December 30, 2020 and January 2, 2021 in cooperation with Luc.id, a consumer research company widely used in the social sciences (e.g., Burzstyn et al. 2023; Fetzer et al. 2021). We restrict our sample to White, non-Muslim respondents who were born in the United States and who report that they are not of Arab descent. Our resulting sample ($n = 5{,}063$) is broadly representative of the targeted population in terms of age, gender, income, Hispanic ethnicity, and education (online Appendix Table A4). We include the survey questionnaire in online Appendix IV and publish the data in Bursztyn et al. (2024).

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

II. Effect of the Presence of Foreign Ancestries on Natives' Donations

We begin by examining the effects of the presence of foreign-descent groups on natives' propensity to donate to those groups' ancestral countries. This analysis

[11] Executive Order 13769, "Protecting the Nation From Foreign Terrorist Entry Into the United States," severely restricted travel from Iran, Iraq, Libya, Somalia, Sudan, Syria, and Yemen. The order did not target all Arab countries (e.g., the United Arab Emirates and Saudi Arabia were exempted). Although it was not officially a ban on Muslims, Trump's repeated comments on the campaign trail—and the fact that all countries on the list were Muslim majority—caused it to be widely interpreted as such.

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

allows us to exploit the dyadic structure of our donations dataset—that is, the fact that we observe donation flows originating from many different counties and going to many different countries—by including a rich set of fixed effects.

A. *Econometric Specification*

In our primary analyses, we measure county d's exposure to foreign ancestral group f as the inverse hyperbolic sine of the number of residents in domestic county d who claim ancestry from a foreign country f, $IHS(Ancestry_{d,f})$.[13] This functional form places an emphasis on the *absolute size* of the community with ancestry from f. For example, a large enough population with ancestry from a given origin country may support grocery stores, restaurants, cultural events and centers, etc. As we discuss in Section IIF, our conclusions remain unchanged if we instead consider the *share* of the population in county d with ancestry from f.

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

$$IHS\left(\#Donations_{d,f}^{t}\right) = \beta IHS\left(Ancestry_{d,f}^{t}\right) + \delta_d \times \delta_t + \delta_f \times \delta_t + Controls_{d,f}^{t} + \epsilon_{d,f}^{t}, \tag{1}$$

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

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

Nevertheless, there remain two main challenges to identifying β. First, unobserved factors may affect both the existing stock of ancestry from a given foreign country and the propensity of local residents to donate specifically to that country, creating a spurious correlation between ancestry and donations. For instance, it is possible that Arab Muslims endogenously prefer settlement in US counties that are and always have been more (or less) tolerant toward Arab migrants than toward other origins. Second, even after isolating exogenous variation in foreign ancestry, it is still possible that different types of natives sort across counties to live near to their preferred foreign minority—selective White flight. We address each of these concerns in turn.

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

B. *Isolating Exogenous Variations in Foreign Ancestry*

To address the first concern, we construct instruments for the present-day distribution of foreign ancestry across US counties by combining data from the long history of foreign migrations to the United States with a simple model of international migration, following closely the approach first developed by Burchardi, Chaney, and Hassan (2019b).[14] Our instruments purposefully exclude any determinant of migration that could correlate with the endogenous response of foreign migrants to natives' attitudes toward specific foreign groups, such as prejudice, hostility, or generosity toward specific groups.

In this model, the historical allocation of foreign migrants across domestic counties is governed by three forces. First, during times when more migrants arrive from a given foreign origin f, more migrants from f will settle in *all* domestic counties, all else equal. We label this first source of variation a "push factor," which varies across foreign origins f and over time t. Second, we assume that upon their arrival in the United States, a migrant from f is more likely to settle in d if they can find better economic opportunities there. We proxy for the attractiveness of county d at time t for migrants arriving from *any* foreign origin using the fraction of foreign migrants, irrespective of their origin, who settle in d at time t. We label this second source of variation an "economic pull factor," which varies across domestic counties d and over time t. Third, we assume that upon their arrival in the United States, a migrant from f is also more likely to settle in d if it hosts a large preexisting community from f. We label this third source of variation a "social pull factor."

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

To exclude the possibility that our push and pull factors are contaminated by any remaining county-country-specific factors, when predicting ancestry from f in d, we leave out from the push factor migrants from f settling in the census region (Northeast, South, West, or Midwest) where county d is located, and from the economic pull factor migrants from the same continent as f.[15]

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

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

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

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

where $Controls_{d,f}^{t}$ includes the full set of controls and fixed effects in (1). $I_{f,-r(d)}^{s}$ is our push factor, the total number of migrants arriving from country f in period s, excluding those who settle in d's region $(-r(d))$; $I_{-c(f),d}^{s}/I_{-c(f)}^{s}$ is our economic pull factor, the fraction of all migrants arriving in the United States in period s who settle in county d, excluding migrants from f's continent $(-c(f))$. The vector $\mathbf{PCs}_{d,f}^{t}$ are principal components summarizing the information contained in higher-order interactions of push and pull factors.[16]

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 United States following the Mexican Revolution: a large "push" from Mexico. At the same time, due to the newly booming automobile industry, Detroit was attracting large numbers of migrants from all origins: a large "economic pull" for Detroit. The push-pull interaction thus induced a large stock of Mexican ancestry in Detroit starting in 1920 (Mexico push 1920 × Detroit pull 1920). As immigration from Mexico again increased in the 1980s, the "social pull" factor led to large inflows of Mexican migrants, even though Detroit was no longer an attractive place for migrants in general (Mexico push 1980 × Mexico push 1920 × Detroit pull 1920). And the next wave of Mexican migrants in the 1990s was again in part attracted to Detroit due to the large Mexican ancestry inherited from both 1920 and 1980 (Mexico push 1990 × Mexico push 1980 × Mexico push 1920 × Detroit pull 1920). As a result, Detroit had a large Mexican community in 2010 inherited from at least three waves. In equation (2), the first wave corresponds to the push-pull term $\gamma_{1920} I_{Mexico,notMidwest}^{1920}\left(I_{notLatinAmerica,Detroit}^{1920}/I_{notLatinAmerica}^{1920}\right)$; the next two waves are summarized in the principal components.

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

$$(3) \qquad \operatorname{cov}\left(I_{f,-r(d)}^{s}\frac{I_{-c(f),d}^{s}}{I_{-c(f)}^{s}}, \epsilon_{d,f}^{t} \middle| \, controls\right) = 0, \quad \forall s \leq t,$$

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

To return to our stylized example, we observe in 2010 many charitable donations from Detroit residents who are not of Mexican descent to Mexico, even controlling for the fact that Detroit residents may be more generous toward *all* foreign countries—the Detroit × quarter fixed effect $\delta_d \times \delta_t$ in (1)—and that Mexico may be a preferred destination for donations from *all* US donors—the Mexico fixed effect $\delta_f \times \delta_t$ in (1). Our first stage predicts a large population of Mexican ancestry in 2010 in Detroit because many Mexicans happened to migrate to the United States

[16] Formally, for all $\{d,f\}$ pairs, there are 758 higher-order terms: $I_{f,-r(d)}^{s}\left(I_{-c(f),d}^{s}/I_{-c(f)}^{s}\right)\prod_{u=s+1}^{t_0} I_{f,-r(d)}^{u}, \forall(s,t_0)$ subject to $1880 \leq s < t_0 \leq t$. The vector $\mathbf{PrincipalComponents}_{d,f}^{t}$ corresponds to the 5 largest principal components, which jointly capture over 99 percent of the total variation among higher-order terms.

in 1920 (excluding the Midwest)—precisely at the time when Detroit was attracting a large share of foreign migrants in 1920 (excluding Latin Americans). Our identifying assumption requires that this interaction of the timing of large Mexican out-migrations and large Detroit in-migrations in 1920 affects disproportionate generosity toward Mexico (relative to causes in other countries) among White (non-Mexican) Detroiters in 2010 only through its effect on Mexican settlement in Detroit and not through any other channel.

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

C. *Main Results*

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

Our preferred estimate in column 4 (0.107, SE $=$ 0.043) implies that a one unit increase in the IHS of ancestry from country f (approximately one-half a standard deviation) increases the IHS of the number of donations to f by 0.107 (approximately two-thirds of a standard deviation).[17] We present this result graphically in Figure 1, in which we plot (binned) predicted ancestry against (binned) IHS-transformed donations, where both are residualized by the set of controls included in column 4 of Table 1. Interpreting the IHS transformation as an approximation of the natural logarithm, the estimated elasticity of the number of donations to f with respect to the size of the ancestral group from f is 0.1: a 1 percent increase in the local population with ancestry from a given country increases the number of donations from donors with European names toward that country by 0.1 percent. The remaining columns show this effect on donations operates at both the extensive and intensive margins: a 1 unit increase in the IHS of ancestry from country f increases the (linear) probability that any residents with European names in the county donate to country f by 4.7 percent and increases the dollar amount of donations by 0.329 percent (Charity 2 only). The first-stage F-statistics tend to be large, but we nonetheless provide

[17] Consistent with Burchardi, Chaney, and Hassan (2019b), the F-statistics on the excluded instruments are well above critical levels throughout (330.6 in column 4), showing that the first stage has sufficient power across all of these variations.

Table 1—Effect of Ancestral Presence on Donations

	IHS(# *donations*)				*Donations* (dummy)	*IHS* ($ *donations*)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. IV						
IHS(*Ancestry*)	0.139	0.132	0.132	0.107	0.047	0.329
	(0.028)	(0.032)	(0.033)	(0.043)	(0.021)	(0.137)
First-stage *F*-statistic	417.1	404.2	393.6	330.6	330.6	337.8
Weak IV-robust *p*-value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Panel B. OLS						
IHS(*Ancestry*)	0.015	0.010	0.009	0.004	0.002	0.016
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.014)
Dep. var. mean	0.019	0.019	0.019	0.019	0.015	0.078
Dep. var. SD	0.182	0.182	0.182	0.182	0.121	0.651
Observations	4,703,862	4,700,864	4,700,864	4,703,862	4,703,862	3,972,708
Foreign country × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	No	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	—	—	—
US state × quarter FE	No	No	Yes	—	—	—
US county × quarter FE	No	No	No	Yes	Yes	Yes

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

p-values from weak IV-robust inference (based on Conditional Likelihood Ratio tests, following Andrews 2016 and Sun 2018).

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

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

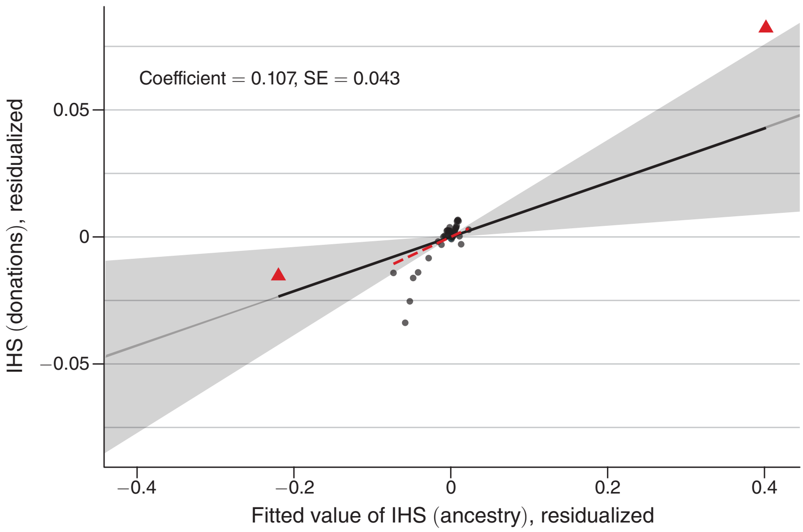

FIGURE 1. BINNED SCATTERPLOT OF DONATIONS

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

OLS versus IV.—To probe the robustness of our instrumental variable strategy, it is useful to first examine the OLS estimates in panel B of Table 1. As we move from column 1 to column 4 (adding more and more controls), the OLS estimate drops by more than two-thirds and becomes statistically indistinguishable from zero in the most stringent specification with quarter × county fixed effects (column 4). These large changes in the OLS coefficient suggest that some of the positive correlation between donations and ancestry in column 1 is likely explained by the fact that counties with more residents of foreign ancestry are wealthier or more generous toward all foreign causes or by the fact that some foreign causes are more popular with donors throughout the United States than others. As we control for more and more of these factors, the OLS coefficient drops dramatically.

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

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

D. *Ruling Out "Selective White Flight"*

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

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

$$(4) \qquad WhiteFlightIndex_{d,f}^{t} = \sum_{d'} \frac{Out_{d,d'}^{t}}{Out_{d,\cdot}^{t}} \frac{Ancestry_{d',f}^{t}/Ancestry_{d'}^{t}}{E\left[Ancestry_{d'',f}^{t}/Ancestry_{d''}^{t} \mid f\right]},$$

where $Out_{d,d'}^{t}/Out_{d,\cdot}^{t}$ is the share of White natives from d who move to d' in period t, $Ancestry_{d',f}^{t}/Ancestry_{d'}^{t}$ is the population share in d' with ancestry from f, and $E\left[Ancestry_{d'',f}^{t}/Ancestry_{d''}^{t} \mid f\right]$ is the average population share with ancestry from f across all US counties. The index thus takes a low value if White residents leaving d move to counties with a disproportionately small ethnic enclave from f. For instance, for $d = Detroit$ and $f = Mexico$, this index takes a low value if a large share of White movers from Detroit choose domestic locations where Mexican ancestry is small relative to its national average. We construct this index for moves by White Americans between 1970 and 2000, using all available data from the 1980, 1990, and 2000 censuses (Ruggles et al. 2022).

TABLE 2—EFFECT OF ANCESTRAL PRESENCE ON WHITE FLIGHT

	Selective White flight index	
	(1)	(2)
Panel A		
IHS(Ancestry)	0.035	−0.010
	(0.010)	(0.007)
First-stage F-statistic	71.03	45.86
Weak IV-robust p-value	< 0.01	< 0.01
Dep. var. mean	0.036	0.036
Dep. var. SD	0.062	0.062
Observations	366,888	366,888
	Selective White flight index, by subgroup	
Panel B		
IHS(Ancestry) × *Married*	−0.002	−0.002
	(0.002)	(0.002)
IHS(Ancestry) × *Female*	−0.00004	−0.00004
	(0.0004)	(0.0004)
IHS(Ancestry) × *College*	0.002	0.002
	(0.001)	(0.001)
IHS(Ancestry) × *Age*	0.001	0.001
	(0.001)	(0.001)
IHS(Ancestry) × *Income*	0.001	0.001
	(0.001)	(0.001)
Year FE	Yes	Yes
Foreign country FE	Yes	Yes
US county FE	No	Yes

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

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

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

where we again instrument for ancestry using equation (2). The table shows no evidence of selective White flight, which would manifest as an economically significant negative coefficient on ancestry: if d hosts a large community from f, movers from d would move to places with a *small* population from f. If anything, the estimate in column 1 (conditional on time and country fixed effects) is marginally positive—the opposite of selective White flight. Once we add county fixed effects in column 2, the estimated coefficient becomes a precisely estimated zero ($\beta = -0.010$, SE $= 0.007$). To investigate whether this null average effect masks heterogeneity, we construct our index separately for married and unmarried individuals, male and female individuals, individuals with and without a four-year college degree, individuals above- and below-median age, and individuals with above- and below-median income. As shown in panel B, we find no evidence of significant heterogeneity across any of the five subgroups. In other words, we find no evidence for the kind of selective White flight that could bias our results.[18]

E. *Local versus Aggregate Effects*

The county $\times$ quarter and country $\times$ quarter fixed effects in our baseline specification rule out a wide range of possible confounding factors that would make residents in some counties more generous than in others or more generous toward some foreign countries than others. As a result of including them, however, our estimates speak only to the *relative* effect of ancestry: White residents of treated counties donate more, relative to their overall level of generosity toward foreign countries. This leaves open the possibility of crowding out: more donations from d may come at the expense of fewer donations from elsewhere. Table 3 suggests there is no such crowding out. Column 1 replicates our baseline county-level regression ($\beta = 0.107$, SE $= 0.043$). To measure the absolute effect, column 2 omits both county $\times$ quarter and country $\times$ quarter fixed effects ($\beta = 0.065$, SE $= 0.015$), and column 3 omits the county $\times$ quarter fixed effect ($\beta = 0.132$, SE $= 0.033$). In both cases, the effect remains positive and highly significant, suggesting that the presence of foreign ancestry in county d positively affects the *absolute* number of donations from d rather than solely reducing the number of donations from d to other destinations.[19]

Consistent with this positive absolute effect, columns 4 and 5 further show positive estimates at higher levels of spatial aggregation. When we aggregate our data at the commuting zone level in column 4 and at the state level in column 5, we find that, if anything, coefficients increase in magnitude as we increase the level of geographical aggregation ($\beta = 0.219$, SE $= 0.104$ for commuting zones, and $\beta = 0.534$, SE $= 0.225$ for states). In column 6, we aggregate *destinations* rather than *origins* to calculate the effect of a greater ancestral presence from all countries in a given continent on donations to all countries in that continent, and we again estimate a positive and highly significant effect ($\beta = 0.342$, SE $= 0.114$). We conclude that the presence of descendants of foreign migrants has a positive *aggregate* impact on the natives' generosity.

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

[19] Of course, it is possible that greater donations toward a given country crowd out other forms of capital flows toward the same country or different countries: we are unable to speak to this possibility.

TABLE 3—EFFECT OF ANCESTRAL PRESENCE ON DONATIONS: INVESTIGATING LOCAL VERSUS AGGREGATE EFFECTS

	IHS(# *donations*) to:					
	Country					Continent
	(1)	(2)	(3)	(4)	(5)	(6)
IHS(*Ancestry*) *from country in county*	0.107	0.065	0.132			
	(0.043)	(0.015)	(0.033)			
IHS(*Ancestry*) *from country in CZ*				0.219		
				(0.104)		
IHS(*Ancestry*) *from country in state*					0.534	
					(0.225)	
IHS(*Ancestry*) *from continent in county*						0.342
						(0.114)
First-stage F-statistic	330.6	492.1	393.6	177.5	90.36	84.16
Weak IV-robust p-value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Dep. var. mean	0.019	0.019	0.019	0.061	0.551	0.121
Dep. var. SD	0.182	0.182	0.182	0.348	1.045	0.511
Observations	4,703,862	4,700,864	4,700,864	1,062,791	76,449	489,528
Foreign country × quarter FE	Yes	No	Yes	Yes	Yes	No
Foreign continent × quarter FE	No	No	No	No	No	Yes
Distance controls	Yes	Yes	Yes	Yes	Yes	No
Demographic controls	—	Yes	Yes	—	—	—
US state × quarter FE	—	No	Yes	No	Yes	—
US commuting zone × quarter FE	—	No	No	Yes	No	—
US county × quarter FE	Yes	No	No	No	No	Yes

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

F. *Additional Robustness Checks*

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

Alternative Instruments.—Online Appendix Table A5 shows our results remain virtually unchanged if we alter the construction of our instruments to allow for a range of potential challenges to our identifying assumptions. In our standard specification, we measure the "pull" factor (the county's attractiveness to the average migrant) using the number of migrants arriving in the county from other continents than f. Leaving out migrants arriving from the same continent insulates our instruments from any d-f specific confounding factors that may also affect migrants from (similar) neighboring countries. In column 1, we measure the pull factor using only *European* migrants, that is, using only the choices made by migrants arriving from countries that are not in our donations sample. In column 2, instead of leaving out migrants from any country f' in the same continent as f, we remove instead migrants from any country f' that historically has tended to send migrants to the United States at the same time.[20] Finally, in column 3, we repeat the same robustness exercise for the calculation of our push factor, where instead of leaving out migrants from f arriving in any d' in the same census region as d, we leave out any d' that historically tended to receive foreign migrants at the same time as d. The fact that all of these specifications yield almost identical results bolsters our confidence that they indeed isolate quasi-random variation in the ancestral composition of US counties.

In online Appendix Table A6, we show that our results are virtually identical when we include as excluded instruments the principal components summarizing the information contained in the higher-order interactions of push and pull factors. Additionally, online Appendix Table A7 shows our results remain stable even when we use only variation in migrations dating back more than 50 years for identification. Successively dropping the instruments corresponding to the interactions from most recent decades, the coefficient remains stable; only when dropping all decades after 1930 does the coefficient of interest lose statistical significance, but it nevertheless retains two-thirds of its original size.

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

One potential concern is that our primary, and most restrictive, sample choice—that is, limiting the sample to donors with European-origin names—fails to exclude some donors with ancestry from the country to which they are donating. For example, our procedure might fail to detect women from a non-European country who took the name of a spouse of European ancestry, or it may fail to detect a second-generation immigrant whose mother was Arab but took the name of a

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

European spouse. To estimate the extent of this bias, we draw from data on intermarriage from the American Community Survey to remove donations from county *d* to country *f* that may originate from donors with ancestry from *f* misclassified as having European ancestry (see details in online Appendix II). Reassuringly, we find in online Appendix Table A9 that our estimates barely change between panel A (no correction) and panel B (with correction). As further evidence against the quantitative significance of this potential confound, our estimates remain similar and significant if we limit our sample to men (see Section IIIC).

Sample Restrictions.—Online Appendix Table A3 shows all of our main results hold if we use data from each charity individually (considering the full set of countries in both charities' datasets rather than restricting to those countries in both). Online Appendix Table A10 instead explores the robustness of our main finding to removing specific groups of foreign countries (panel A) or domestic census regions (panel B), confirming that no specific group of countries or US census region drives the overall effect.

Inference.—In online Appendix Table A11, we present the standard errors associated with five alternate clustering choices—robust standard errors, clustering at the domestic county level, clustering at the domestic state level, clustering at the foreign country level, and two-way clustering by foreign country and domestic state—and show that our baseline two-way clustering at the country-county levels is conservative. As an alternative and more demanding approach to inference, we conduct a series of permutation tests, randomly matching each country in our dataset to another "placebo" country and swapping the endogenous variables (IHS-transformed ancestry) and the excluded instruments to those associated with the placebo country. We then estimate equation (1) to recover, for example, an average of the effect of Peruvian ancestry on donations to Ethiopia, of Ethiopian ancestry on donations to Nepal, etc. Under the null hypothesis that cross-country spillovers are *on average* zero, the resulting regression coefficients will have mean zero. Consistent with this null, online Appendix Figure A5 shows an approximately normal distribution of 1,000 placebo coefficients centered on 0. The implied *p*-value for the effect of ancestry on donations in our main specification is 0.01.

Functional Form.—Finally, online Appendix Table A12 replicates our main specifications using the share of the population with ancestry from foreign country *f* as the endogenous variable, rather than IHS-transformed ancestry from *f*. Again, we find similar quantitative and qualitative results.

III. Mechanisms

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

A. *Cultural Bridge: First- versus Higher-Generation Immigrants*

A small literature, primarily in sociology, has argued that natives' attitudes toward second-generation immigrants (those born in the United States but whose parents, grandparents, etc. were of foreign birth) are more positive than attitudes toward first-generation immigrants (Barrera, Bensidoun, and Edo 2022; Kuziemko and Ferrie 2014; Kunst and Sam 2014; Hernandez, Denton, and Macartney 2008). Is the presence of second- (and higher-) generation immigrants also more effective in increasing natives' generosity toward these immigrants' ancestral country? To test this hypothesis, we estimate the marginal effect of first-generation versus higher-generation immigrants by adding the IHS of the number of immigrants born in f who reside in d in 2010 as a second endogenous variable to equation (1).

Naturally, the number of US-born residents in d with ancestry from f is correlated with the number of immigrants from f in d. Thus, we verify that our instruments have sufficient statistical power to separately isolate variation in the number of descendants versus first-generation immigrants, reporting the Sanderson and Windmeijer (2016) conditional first-stage F-statistics of both variables.[21] Our instruments pass this test for both endogenous variables, indicating that they isolate independent exogenous variation in both variables and that we can interpret our coefficients as marginal effects. In other words, we can separately estimate the effect of exogenously changing the size of the ancestral population (holding fixed the number of first-generation immigrants) and the effect of exogenously changing the number of immigrants (holding fixed the size of the ancestral population).

Table 4 presents the results of this horse race. An exogenously larger foreign-born population from foreign country f increases the number of charitable donations to f (column 1). But this effect entirely disappears when we control for the size of the population with foreign ancestry from f (columns 2–4), instrumenting both endogenous variables with our standard set of excluded instruments. The effect of exposure to foreign ancestry is stable as we measure the stock of foreign ancestry at different points, 1990 (column 2), 2000 (column 3), or 2010 (column 4), while the marginal effect of exposure to foreign-born migrants remains insignificant in all specifications. This difference suggests that descendants of migrants from recipient countries have a larger impact on donations made by White natives than foreign-born migrants themselves. This larger impact could reflect the fact US counties with large populations of foreign ancestry, but not foreign born, from f have been exposed to immigrants from f for a longer period of time, with the effect of exposure building

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

TABLE 4—ANCESTRAL PRESENCE VERSUS PRESENCE OF FIRST-GENERATION IMMIGRANTS

	IHS(# *donations*)			
	(1)	(2)	(3)	(4)
IHS(*Foreign-born 2010*)	0.192	0.015	−0.0003	−0.027
	(0.078)	(0.085)	(0.091)	(0.111)
IHS(*Ancestry 1990*)		0.089		
		(0.032)		
IHS(*Ancestry 2000*)			0.100	
			(0.043)	
IHS(*Ancestry 2010*)				0.120
				(0.063)
F-stat *IHS*(*Foreign-born 2010*)	39.73	20.06	27.77	23.32
F-stat *IHS*(*Ancestry*)	—	20.32	22.61	19.72
Dep. var. mean	0.019	0.019	0.019	0.019
Dep. var. SD	0.182	0.182	0.182	0.182
Observations	4,703,862	4,703,862	4,703,862	4,703,862
Foreign country × quarter FE	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes
US county × quarter FE	Yes	Yes	Yes	Yes

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

up over time. Alternatively, it may be that second- and higher-generation immigrants are better able to act as a *cultural bridge* between White natives and foreign countries, inducing greater generosity toward their ancestral countries.

B. *Attitudes*, *Political Choices*, *Contact*, *and Knowledge*

We now turn to more direct measures of altruism and prejudice by focusing our analysis on Arab Muslims, a group that not only has experienced widespread discrimination in recent years but for which several large-scale cross-county datasets are available. We pool the migration data across all countries in the Arab League[22] and construct a single set of instruments for the distribution of residents with Arab Muslim ancestry across US counties. We begin by replicating our estimates on donations for the pooled group of Arab Muslims, then turn to a number of outcomes measuring attitudes, political choices, contact, and knowledge of Islam.

[22] We include countries in the Arab League (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) and add Syria, which was suspended from the League in 2011.

Charitable Donations toward Arab Muslim Countries.—Eight percent of donations in our dataset are toward Arab Muslim countries. To quantify the effect of exposure to Arab Muslims on donations by local residents, we estimate a simplified version of equation (1):

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

where, again, we instrument the (IHS-transformed) number of residents of Arab ancestry in domestic county d, $IHS\left(Ancestry_{d,Arab}\right)$, using equation (2). As before, we restrict to donors who have European-ethnicity names to ensure that we are not capturing a natural tendency of people of Arab Muslim descent to donate to their home countries.[23]

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

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

Table 5 shows estimates of equation (6). Mirroring our previous findings, an exogenously larger Arab population in county d substantially increases the flow of donations from d to all Arab countries. The estimated effects are substantial: in our preferred specification (column 3), a one-unit increase in the IHS-transformed Arab population causes a 0.397 increase in the IHS-transformed number of dona-

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

TABLE 5—EFFECT OF PRESENCE OF ARAB ANCESTRY ON DONATIONS TOWARD ARAB COUNTRIES

	IHS(# *donations*)		
	(1)	(2)	(3)
Panel A. IV			
IHS(*Arab ancestry*)	0.388	0.371	0.397
	(0.048)	(0.057)	(0.059)
First-stage F-statistic	466.4	361.9	317.5
Weak IV-robust p-value	$<$0.01	$<$0.01	$<$0.01
	IHS(# *donations*)		
Panel B. OLS			
IHS(*Arab ancestry*)	0.027	0.012	0.011
	(0.002)	(0.002)	(0.002)
Dep. var. mean	0.048	0.048	0.048
Dep. var. SD	0.297	0.297	0.297
Observations	150,336	150,336	150,336
Quarter FE	Yes	Yes	—
Distance controls	No	Yes	Yes
Demographic controls	No	Yes	Yes
US state × quarter FE	No	No	Yes

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

tions. The fact that this estimated elasticity of the number of donations with respect to ancestry is larger for Arabs as a group than for individual countries (0.107 in Table 1) suggests there may exist positive spillovers between communities originating from nearby countries, such that (for example) a larger community from Jordan may increase generosity toward Syria.[24]

These results are robust to controlling for a battery of county-level demographic controls (those identified in online Appendix Figure A8 as potentially unbalanced between high– and low–Arab Muslim ancestry counties) and state fixed effects. The OLS coefficient fluctuates substantially with the inclusion of controls, while the IV coefficients remain stable across variations; in particular, when we add controls for

[24] Consistent with such positive spillovers among Arab countries, panel A of online Appendix Table A13 shows spillovers between different ancestral groups: we investigate how donations to a given foreign country are affected by a larger local population of residents with ancestry from the continent containing that country (excluding residents with ancestry from that country). Panel B additionally controls for the IHS-transformed population of residents with ancestry from the country in question. While coefficient estimates are less precise, the evidence suggests weak positive spillovers between geographically proximate countries.

all of the unbalanced county characteristics, the coefficient of interest changes from 0.388 (SE = 0.048) to 0.371 (SE = 0.057). Adding the interaction of state and time fixed effects raises it slightly to 0.397 (SE = 0.059). Any other county-level omitted variables would have to have dramatically larger effects than these observables to impact our results. Our instruments appear effective at isolating exogenous variation in ancestry uncorrelated with other drivers of differential generosity.

Attitudes toward Arab Muslims.—We now turn to measures of attitudes toward Arab Muslims. Because our data come from individual-level surveys, we are now also able to include individual-level controls. We limit the sample to White, non-Muslim respondents who took the IAT for work or school. Our baseline specification is

$$Attitude_{i,d,Arab} = \beta IHS\big(Ancestry_{d,Arab}\big) + Controls_{i,d} + \epsilon_{i,d}, \tag{7}$$

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

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

We find that our estimated coefficients are statistically significant and economically meaningful: in our preferred specification with individual controls (age, male, age squared, age $\times$ male) and state fixed effects (column 3), a one-unit increase in the IHS-transformed population of Arab ancestry in a county (approximately one-half a standard deviation) causes a 0.075 (SE = 0.027) standard deviation increase in average Arab Muslim IAT scores and a 0.136 (SE = 0.033) standard deviation increase in explicitly stated warmth (panel B). We show these results graphically in Figure 2.[25] Our estimates remain stable with and without state fixed effects and as we introduce a series of "bad controls" (Angrist and Pischke 2009): column 5 of Table 6 shows that our estimate remains stable as we introduce a control for the non-European population, evidence that our effects are not simply capturing exposure to non-White residents in general. Column 6 instead controls for the average race IAT score within county *d*, which measures the implicit attitudes of White respondents toward African Americans, while column 7 controls for the 2012 Republican vote share. The coefficient of interest remains stable across these

[25] To put this effect into perspective, a one-IHS increase in the size of the Arab-ancestry population roughly corresponds to going from the Arab-ancestry population of Kings County, New York to that of Wayne County, Michigan, or going from the Arab-ancestry population of St. Louis County, Missouri to San Mateo County, California (see online Appendix Figure A7).

TABLE 6—EFFECT OF PRESENCE OF ARAB ANCESTRY ON ATTITUDES TOWARD ARAB MUSLIMS

	OLS	IV					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Score on Arab Muslim IAT (std., higher score = less prejudiced)							
IHS(Arab ancestry)	0.013	0.071	0.075	0.067	0.068	0.053	0.053
	(0.006)	(0.018)	(0.027)	(0.026)	(0.027)	(0.023)	(0.025)
IHS(non-Euro ancestry)					−0.008		
					(0.018)		
Avg. race IAT score						0.352	
						(0.064)	
2012 Rep. vote share							−0.125
							(0.053)
AP F-statistic	—	12.20	9.907	6.600	6.263	6.625	6.279
Weak IV-robust p-value	—	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Observations	108,235	108,235	107,110	107,110	107,110	107,110	107,110
Panel B. Warmth toward Arab Muslims (std., higher score = more favorable)							
IHS(Arab ancestry)	0.043	0.156	0.136	0.107	0.117	0.085	0.087
	(0.008)	(0.029)	(0.033)	(0.031)	(0.030)	(0.027)	(0.031)
IHS(non-Euro ancestry)					−0.046		
					(0.020)		
Avg. race IAT score						0.585	
						(0.084)	
2012 Rep. vote share							−0.269
							(0.073)
AP F-statistic	—	12.30	9.950	6.547	6.220	6.581	6.223
Weak IV-robust p-value	—	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Observations	108,109	108,109	106,999	106,999	106,999	106,998	106,999
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Individual-level demographics	No	No	Yes	Yes	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes	Yes	Yes

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

variations, suggesting that our measures do not simply proxy for general prejudice against minorities or for political or social conservatism.

It is possible that "supply-side" mechanisms—such as companies matching donations to certain causes or individuals of a particular ancestral group raising donations for their ancestral country (DellaVigna, List, and Malmendier 2012)—partially explain the effects of ancestry on donations documented above.[26] The effects of

[26]For example, one alternative interpretation of our results could be that charities might strategically target fundraising campaigns for causes in disaster-struck countries toward areas with larger communities with ancestry

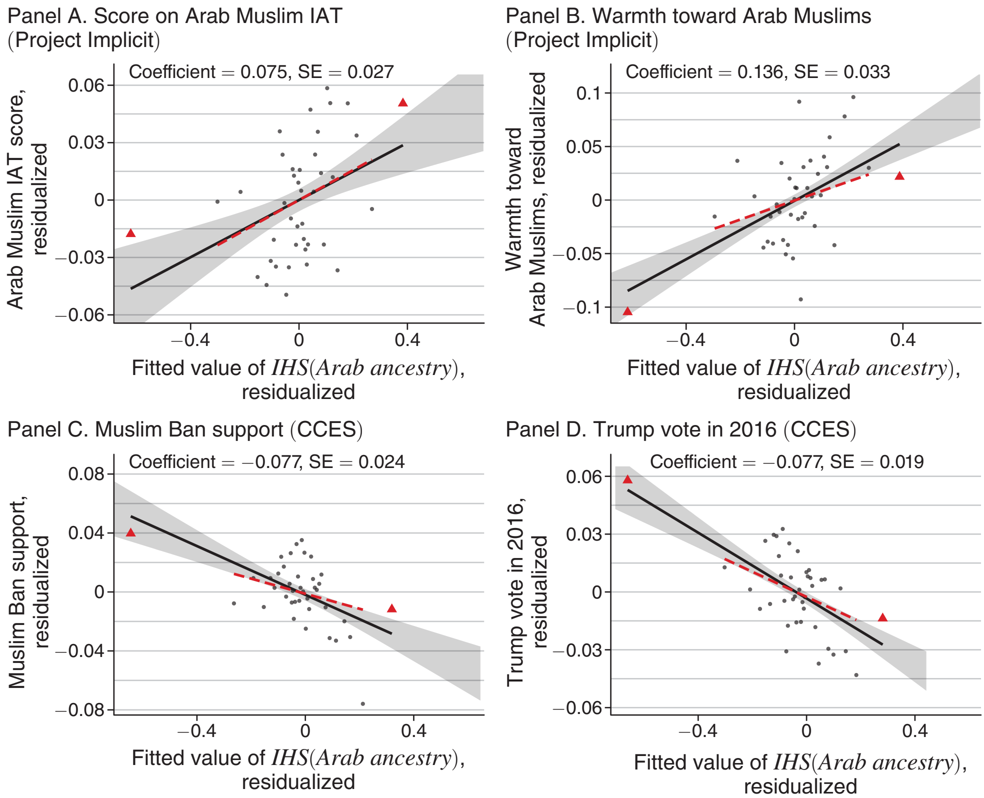

Figure 2. Binned Scatterplots of Attitudes and Political Preferences

Notes: Figure 2 presents binned scatterplots 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 $\left\{I^t_{f,-r(d)}\left(I^t_{-c(f),d}/I^t_{-c(f)}\right)\right\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. We residualize by the controls used in column 3 of Table 6. Red triangles are used to indicate the top and bottom 2.5 percent of the data by fitted values; the red dotted line indicates the regression fit after dropping these observations. Standard errors are clustered at the county level. Ninety-five percent confidence intervals are reported.

ancestry on implicit and explicit attitudes, however, indicate that "demand-side" mechanisms are present as well: greater contact with a given ancestral group changes natives' private views and, plausibly through this channel, induces them to donate.[27]

In the online Appendix, we replicate our results using the full sample of Project Implicit respondents rather than restricting to respondents who were forced to take

from that country. To evaluate this concern, we asked our contacts at Charities 1 and 2 for information about their fundraising strategies. Reassuringly, neither charity strategically targets counties based on ancestry, region, or demographics.

[27] Online Appendix Table A14 shows coefficient estimates on the four other measures of explicit attitudes toward Arab Muslims from Project Implicit. We find strong and robust positive treatment effects on measures of *personal* beliefs (columns 3 and 4), in line with our earlier estimates on warmth and implicit bias. However, we find weaker and less robust treatment effects on measures of social norms against Islamophobia (columns 1 and 2). We view these results as suggestive evidence that exposure causally improves *private* attitudes toward Arab Muslims and that these changes in private attitudes are more important in explaining changes in behavior than changes in social norms.

the Implicit Association Test for work or school. All of our results remain statistically significant, and coefficient estimates change little, suggesting a limited role of endogenous selection of more tolerant residents taking the IAT to confirm their lack of prejudice (see online Appendix Table A15). To further ensure that our results are not driven by selection into Project Implicit tests, we replicate our analysis using outcomes from Nationscape (online Appendix Table A16), again with virtually identical results.

As further evidence that our regressions are capturing effects on natives' attitudes specifically toward Arab Muslims, rather than toward immigrants or minorities more broadly, online Appendix Table A17 investigates the effect of the presence of an Arab Muslim ancestral population on White respondents' attitudes toward other groups. In panel A, we find no statistically detectable effect of Arab ancestry on implicit attitudes toward Asians and Black people nor on respondents' explicit attitudes toward Asians. Interestingly, we do find a small positive effect of Arab ancestry on explicitly stated attitudes toward Black people, which is about a quarter of the size of the direct effect on explicit attitudes toward Arab Muslims. Such a spillover is consistent with the findings of Fouka and Tabellini (2022), who show that greater inflows of Hispanic immigrants improved natives' attitudes toward Black people.[28] Because the sample of test takers differs on observables between groups, we conduct a number of exercises to facilitate more direct comparison of the point estimates. Panel B reweights the sample to match the sample of Arab Muslim test takers on observables, panel C limits the sample to counties represented in the Arab Muslim IAT data, and panel D both limits the sample to these counties and reweights. In all cases, the estimated effects on attitudes toward Arab Muslims remain significantly larger than the effects on Asians or Black people.

Political Choices.—To what extent do these effects on attitudes translate into political choices? We consider two outcomes: support for the Muslim Ban and voting for presidential candidate Donald Trump in 2016. Table 7 shows coefficient estimates using our individual-level specification, equation (7), again limiting to White, non-Muslim respondents. The results suggest that an exogenous increase in the presence of residents of Arab ancestry significantly reduces both support for the Muslim Ban (panel A) and voting for Donald Trump in 2016 (panel B): in our preferred specification (column 3), a one-unit increase in the IHS of Arab ancestry decreases the probability that a respondent supports the Muslim Ban by 7.7 percentage points ($\text{SE} = 0.024$) and the probability that a respondent voted for Trump in 2016, controlling for the respondent's county-level vote share for Romney in 2012, by 7.7 percentage points ($\text{SE} = 0.020$). To put these magnitudes in perspective, the impact of one-half a standard deviation increase in the population of Arab ancestry on votes for candidate Trump is equivalent to a 15 percentage point decrease in the 2012 Republican vote share. Online Appendix Table A18 replicates Table 7, controlling for respondents' *own* 2012 vote rather than the vote share of their county.

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

Table 7—Effect of Presence of Arab Ancestry on Political Preferences

	OLS	IV			
	(1)	(2)	(3)	(4)	(5)
Panel A. Support for the Muslim Ban					
IHS(*Arab ancestry*)	−0.033	−0.099	−0.077	−0.039	−0.044
	(0.005)	(0.036)	(0.024)	(0.022)	(0.021)
IHS(*non-Euro ancestry*)					0.00004
					(0.012)
AP F-statistic	—	16.63	9.650	5.143	4.947
Weak IV-robust p-value	—	<0.01	<0.01	<0.01	<0.01
Dep. var. mean	0.529	0.529	0.529	0.529	0.529
Dep. var. SD	0.499	0.499	0.499	0.499	0.499
Observations	57,195	57,195	57,195	57,195	57,195
Panel B Voted for Trump in 2016					
IHS(*Arab ancestry*)	−0.015	−0.057	−0.077	−0.045	−0.052
	(0.004)	(0.019)	(0.020)	(0.021)	(0.021)
IHS(*non-Euro ancestry*)					0.007
					(0.012)
2012 Rep. vote share	0.636	0.579	0.527	0.513	0.515
	(0.033)	(0.043)	(0.032)	(0.035)	(0.033)
AP F-statistic	—	19.22	10.74	5.281	5.129
Weak IV-robust p-value	—	<0.01	<0.01	<0.01	<0.01
Dep. var. mean	0.463	0.463	0.463	0.463	0.463
Dep. var. SD	0.499	0.499	0.499	0.499	0.499
Observations	98,032	98,032	98,032	98,032	98,032
State FE	No	No	Yes	Yes	Yes
Individual-level demographics	No	No	Yes	Yes	Yes
County-level demographics	No	No	No	Yes	Yes

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

Our sample size drops substantially because this question was only asked in the 2016 wave; we continue to find statistically significant effects of Arab presence on Trump voting, suggesting that the most saliently anti-Muslim presidential candidate in recent memory activated political preferences in a way that Romney did not.

Contact and Personal Knowledge.—To gain further insight into the mechanisms by which greater exposure to Arab Muslims might affect implicit and explicit attitudes, political choices, and charitable donations, we turn to our custom survey. We evaluate two possible mechanisms: personal contact and knowledge. First, if a greater population of Arab Muslims leads to more personal interaction with Arab

Muslims, it may improve attitudes and increase altruism, in line with the contact hypothesis (Allport 1954). Second, even in the absence of direct personal contact, a larger Arab Muslim community may increase knowledge of Arab Muslims and Islam in general—due to, for example, greater and more accurate coverage on local media and social media or contact with social acquaintances who themselves have greater personal contact with Arab Muslims. Such increased knowledge may translate into greater altruism if it leads residents to update negative priors (Grigorieff, Roth, and Ubfal 2020).

We first examine whether living in a county with an exogenously greater population of Arab Muslims translates into greater personal contact with Arab Muslims. In Panel A of Table 8, we estimate the effects of the IHS-transformed population with Arab ancestry in a respondent's county on several binary outcomes: whether the respondent has an Arab Muslim friend, workplace acquaintance, or neighbor (columns 1–3) and has eaten in a Middle Eastern restaurant (column 4). Column 5 reports effects on a binary variable taking value one if any of the variables in columns 1–3 take value one.[29] We find statistically significant effects on all outcomes except for the "friends" indicator (though the point estimate is positive). The effects are large—a 1-unit increase in the IHS of the Arab population (approximately one-half a standard deviation) translates into an approximately 13 percent increase in the probability that the respondent has an Arab Muslim friend, neighbor, or workplace acquaintance—and are robust to weak IV-robust inference.[30]

In panel B of Table 8, we examine whether greater exposure to Arab Muslims also translates into greater *knowledge* of Arab Muslims and Islam in general. We examine effects on knowledge of the Pillars of Islam (column 2), knowledge of the definition of Ramadan (column 3), knowledge of the share of Muslims in the United States (column 4), and an index of these three outcomes (column 5) constructed by scaling each of the three knowledge questions to mean zero and standard deviation one and summing the scaled values. In column 1, we examine a specific outcome (derived from the question on the Pillars of Islam) specifically measuring beliefs about *negative* traits of Islam: whether "holy war against non-believers" and/or the "subservience of women and children to men" are among the five Pillars.[31] A one-unit increase in the IHS-transformed Arab ancestry translates into a 0.37 standard deviation increase in the knowledge index.

Additional Robustness.—We conduct four additional exercises to examine the robustness of our results. First, because it is not straightforward to map the outcomes studied in this section to a specific group of countries (particularly because many Muslim-majority countries are not Arab), we consider two alternative definitions in online Appendix Table A19, constructing new instruments specifically for

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

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

[31] 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.

Table 8—Effect of Presence of Arab Ancestry on Contact and Knowledge

	(1)	(2)	(3)	(4)	(5)
	Friends	Workplace	Neighbors	Restaurant	Any (1–3)
Panel A. Contact with Arab Muslims					
IHS(*Arab ancestry*)	0.036	0.103	0.093	0.117	0.130
	(0.025)	(0.037)	(0.026)	(0.043)	(0.038)
AP *F*-statistic	9.025	9.025	9.025	8.300	8.300
Weak IV-robust *p*-value	$<$0.01	$<$0.01	0.63	$<$0.01	$<$0.01
Dep. var. mean	0.098	0.286	0.198	0.441	0.397
Dep. var. SD	0.297	0.452	0.398	0.496	0.489
Observations	5,189	5,189	5,189	5,189	5,189
	Subservience/war	Pillars	Ramadan	Pop. accuracy	Index (2–4)
Panel B. Knowledge of Arab Muslims					
IHS(*Arab ancestry*)	−0.130	0.429	0.106	2.894	0.372
	(0.053)	(0.149)	(0.040)	(1.061)	(0.103)
AP *F*-statistic	8.300	8.300	8.300	7.903	7.903
Weak IV-robust *p*-value	0.25	$<$0.01	$<$0.01	$<$0.01	$<$0.01
Dep. var. mean	0.589	4.492	0.764	−15.075	0.000
Dep. var. SD	0.758	1.558	0.425	13.646	1.000
Observations	5,051	5,051	5,051	4,757	4,757
Demographics	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates from regressions at the individual level. In panel A, the dependent variables in columns 1–3 are indicators for whether the respondent has an Arab Muslim friend, workplace acquaintance, or neighbor, respectively; the dependent variable in column 4 is an indicator for whether the respondent reports having ever eaten at a Middle Eastern restaurant; and the dependent variable in column 5 is an indicator taking value 1 if any of the indicators in columns 1–3 take value 1. 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 United States and the actual size of the Muslim population in the United States. Respondents with invalid guesses ($<$ 0 percent or $>$ 100 percent) were dropped. The dependent variable in column 5 is constructed by scaling the dependent variables in columns 2–4 to mean zero and standard deviation one, summing these three scaled values, and renormalizing. The main variable of interest is the 2010 IHS-transformed population with ancestry from Arab League countries. We include $\left\{I^t_{f,-r(d)}\left(I^t_{-c(f),d}/I^t_{-c(f)}\right)\right\}_{t=1880,\ldots,2010}$ and the first five principal components of the higher-order interactions of push and pull factors as excluded instruments. All specifications control for log 2010 population. Individual demographics include age, male, age squared, and age $\times$ male. Standard errors are given in parentheses. Standard errors are clustered at the county level.

each: all countries targeted by the Muslim Ban (panel B) and all Muslim-majority countries (panel C). The results remain stable and significant. Second, in online Appendix Table A20, we again replicate all of our specifications using the share of the population of Arab Muslim ancestry, rather than the IHS-transformed population, as our endogenous variable. All coefficient estimates are strong and statistically significant. Third, online Appendix Table A21 shows that, as with our main results, there is no evidence of selective White flight that could result in White residents who dislike Arabs moving toward counties with relatively few Arabs. If anything, White residents leaving counties with large Arab presence tend to relocate to areas with even larger Arab populations, conditional on moving at all. Finally, in columns 5–7 of online Appendix Table A9, we repeat the exercise of Section IIF to subtract the estimated number of donations that may have been made by Arab ancestry donors misclassified as having European ancestry. The estimated causal impact of the presence of Arab ancestry on donations to Arab countries is almost identical without (panel A) and with (panel B) this correction.

C. (*Lack of*) *Heterogeneity*

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

With one exception (donations to Arab countries), the other columns of Figure 3 show no evidence of heterogeneity along several other important dimensions. The effect of the presence of foreign ancestry on attitudes is similar in small- and large-population counties, despite the fact that residents in larger counties may have more freedom to strategically move away from specific foreign ancestry groups within their county. The effect is also similar in sparsely and densely populated counties, despite the fact that residents in denser counties may have more frequent interactions with all residents in their county. Finally, the effect is also similar for male and female respondents, the only characteristic we can observe (or infer) across our different data at the individual level. This absence of heterogeneity by gender alleviates the potential concern that our effects are driven by women from non-European ancestry who took European last names.

IV. Conclusion

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

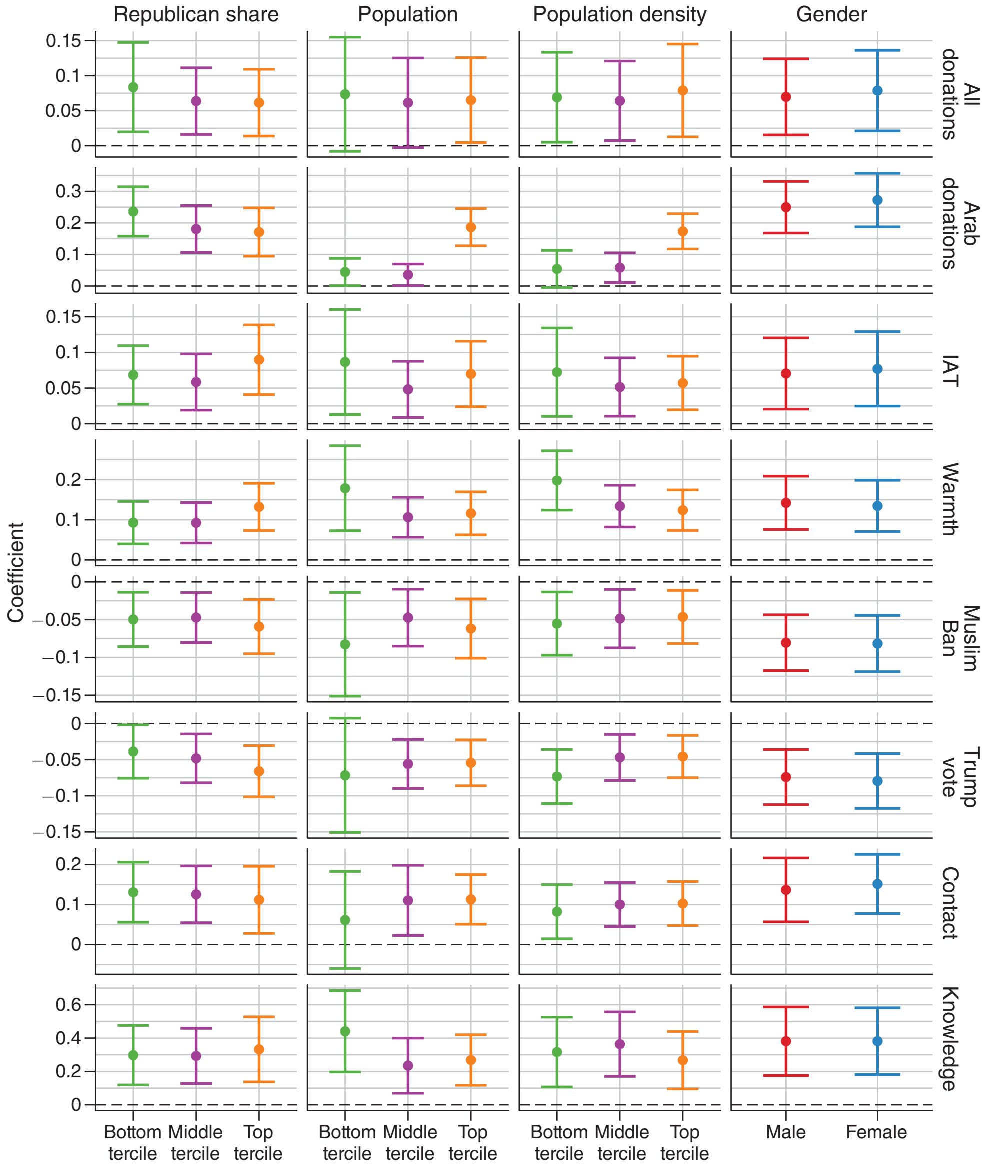

Figure 3. Heterogeneity by Gender, Population, Population Density, and Republican Vote Share

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

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

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

REFERENCES

Achard, Pascal, Sabina Albrecht, Elena Cettolin, Riccardo Ghidoni, and Sigrid Suetens. 2022. "The Effect of Exposure to Ethnic Minorities on Ethnic Preferences." Unpublished.

Alesina, Alberto, Paola Giuliano, and Nathan Nunn. 2013. "On the Origins of Gender Roles: Women and the Plough." *Quarterly Journal of Economics* 128 (2): 469–530.

Allport, Gordon Willard. 1954. *The Nature of Prejudice.* Boston, MA: Addison-Wesley Publishing Company.

Andrews, Isaiah. 2016. "Conditional Linear Combination Tests for Weakly Identified Models." *Econometrica* 84 (6): 2155–82.

Angrist, Joshua D., and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton, NJ: Princeton University Press.

Ansolabehere, Stephen, and Brian F. Schaffner. 2017. "CCES Common Content, 2016." Harvard Dataverse. https://doi.org/10.7910/DVN/GDF6Z0.

Ansolabehere, Stephen, and Brian F. Schaffner. 2019. "CCES Common Content, 2017." Harvard Dataverse. https://doi.org/10.7910/DVN/3STEZY.

Ansolabehere, Stephen, Brian F. Schaffner, and Samantha Luks. 2019. "CCES Common Content, 2018." Harvard Dataverse. https://doi.org/10.7910/DVN/ZSBZ7K.

Ansolabehere, Stephen, Brian F. Schaffner, and Samantha Luks. 2020. "CCES Common Content, 2019." Harvard Dataverse. https://doi.org/10.7910/DVN/WOT7O8.

Arkolakis, Costas, Sun Kyoung Lee, and Michael Peters. 2020. "European Immigrants and the United States' Rise to the Technological Frontier." Unpublished.

Bagues, Manuel, and Christopher Roth. 2023. "Interregional Contact and the Formation of a Shared Identity." *American Economic Journal: Economic Policy* 15 (3): 322–50.

Barone, Guglielmo, Alessio D'Ignazio, Guido de Blasio, and Paolo Naticchioni. 2016. "Mr. Rossi, Mr. Hu And Politics—The Role of Immigration in Shaping Natives' Voting Behavior." *Journal of Public Economics* 136: 1–13.

Barrera, Oscar, Isabelle Bensidoun, and Anthony Edo. 2022. "Second-Generation Immigrants and Native Attitudes Toward Immigrants in Europe." Unpublished.

Bartik, Timothy. 1991. *Who Benefits from State and Local Economic Development Policies*? Kalamazoo, MI: Upjohn Press.

Bazzi, Samuel, Arya Gaduh, Alexander D. Rothenberg, and Maisy Wong. 2019. "Unity in Diversity? How Intergroup Contact Can Foster Nation Building." *American Economic Review* 109 (11): 3978–4025.

Becker, Sascha O., and Thiemo Fetzer. 2016. "Does Migration Cause Extreme Voting?" Unpublished.

Belgiu, Mariana. 2015. "UIA World Countries Boundaries." UNIGIS International Association. https://hub.arcgis.com/maps/252471276c9941729543be8789e06e12.

Bertrand, Marianne, Dolly Chugh, and Sendhil Müllainathan. 2005. "Implicit Discrimination." *American Economic Review* 95 (2): 94–98.

Billings, Stephen B., Eric Chyn, and Kareem Haggag. 2021. "The Long-Run Effects of School Racial Diversity on Political Identity." *American Economic Review: Insights* 3 (3): 267–84.

Boisjoly, Johanne, Greg J. Duncan, Michael Kremer, Dan M. Levy, and Jacque Eccles. 2006. "Empathy Or Antipathy? The Impact of Diversity." *American Economic Review* 96 (5): 1890–1905.

Boustan, Leah Platt. 2010. "Was Postwar Suburbanization "White Flight"? Evidence from the Black Migration." *Quarterly Journal of Economics* 125 (1): 417–43.

Brunner, Beatrice, and Andreas Kuhn. 2018. "Immigration, Cultural Distance and Natives' Attitudes towards Immigrants: Evidence from Swiss Voting Results." *Kyklos* 71 (1): 28–58.

Burchardi, Konrad B., Thomas Chaney, and Tarek A. Hassan. 2019a. "Extended replication files for Migrants, Ancestors, and Foreign Investment." https://doi.org/10.5281/zenodo.8021227.

Burchardi, Konrad B., Thomas Chaney, and Tarek A. Hassan. 2019b. "Migrants, Ancestors, and Foreign Investments." *Review of Economic Studies* 86 (4): 1448–86.

Burchardi, Konrad B., Thomas Chaney, Tarek Alexander Hassan, Lisa Tarquinio, and Stephen J. Terry. 2020. "Immigration, Innovation, and Growth." NBER Working Paper 27075.

Bureau of Labor Statistics. 2018. "Labor Force Data by County, 2018 Annual Averages." United States Department of Labor. https://www.bls.gov/lau/laucnty18.xlsx.

Bursztyn, Leonardo, Thomas Chaney, Tarek A. Hassan, and Aakaash Rao. 2024. "Replication data for: The Immigrant Next Door." American Economic Association [Publisher], Inter-university Consortium for Political and Social Research [Distributor]. https://doi.org/10.3886/E191911V1.

Burzstyn, Leonardo, Georgy Egorov, Ingar Haaland, Aakaash Rao, and Christopher Roth. 2023. "Justifying Dissent." *Quarterly Journal of Economics* 138 (3): 1403–51.

Calderon, Alvaro, Vasiliki Fouka, and Marco Tabellini. 2023. "Racial Diversity and Racial Policy Preferences: The Great Migration and Civil Rights." *Review of Economic Studies* 90 (1): 165–200.

Card, David. 2001. "Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration." *Journal of Labor Economics* 19 (1): 22–64.

Carlana, Michela. 2019. "Implicit Stereotypes: Evidence from Teachers' Gender Bias." *Quarterly Journal of Economics* 134 (3): 1163–1224.

Carrell, Scott E., Mark Hoekstra, and James E. West. 2019. "The Impact of College Diversity on Behavior Toward Minorities." *American Economic Journal: Economic Policy* 11 (4): 159–82.

Colussi, Tommaso, Ingo E. Isphording, and Nico Pestel. 2021. "Minority Salience and Political Extremism." *American Economic Journal: Applied Economics* 13 (3): 237–71.

Corno, Lucia, Eliana La Ferrara, and Justine Burns. 2022. "Interaction, Stereotypes, and Performance: Evidence from South Africa." *American Economic Review* 112 (12): 3848–75.

Crawfurd, Lee. 2021. "Contact and Commitment to Development: Evidence from Quasi-random Missionary Assignments." *Kyklos* 74 (1): 3–18.

Dahl, Gordon B., Andreas Kotsadam, and Dan-Olof Rooth. 2021. "Does Integration Change Gender Attitudes? The Effect of Randomly Assigning Women to Traditionally Male Teams." *Quarterly Journal of Economics* 136 (2): 987–1030.

DellaVigna, Stefano, John A. List, and Ulrike Malmendier. 2012. "Testing for Altruism and Social Pressure in Charitable Giving." *Quarterly Journal of Economics* 127 (1): 1–56.

Dill, Verena. 2013. "Ethnic Concentration and Extreme Right-Wing Voting Behavior in West Germany." Unpublished.

Din, Alexander, and Ron Wilson. 2020. "Crosswalking ZIP Codes to Census Geographies: Geoprocessing the U.S. Department of Housing & Urban Development's ZIP Code Crosswalk Files." *Cityscap* 22 (1): 293–314.

Duncalfe, Luke. 2020. "ISO-3166 Country and Dependent Territories Lists with UN Regional Codes." GitHub. https://github.com/lukes/ISO-3166-Countries-with-Regional-Codes/ (accessed October 4, 2023).

Duncan, Brian, and Stephen J. Trejo. 2017. "The Complexity of Immigrant Generations: Implications for Assessing the Socioeconomic Integration of Hispanics and Asians." *ILR Review* 70 (5): 1146–75.

Dustmann, Christian, Kristine Vasiljeva, and Anna Piil Damm. 2019. "Refugee Migration and Electoral Outcomes." *Review of Economic Studies* 86 (5): 2035–91.

Egloff, Boris, and Stefan C. Schmukle. 2002. "Predictive Validity of an Implicit Association Test for Assessing Anxiety." *Journal of Personality and Social Psychology* 83 (6): 1441–45.

Enos, Ryan D. 2014. "Causal Effect of Intergroup Contact on Exclusionary Attitudes." *Proceedings of the National Academy of Sciences* 111 (10): 3699–3704.

Fetzer, Thiemo, Lukas Hensel, Johannes Hermle, and Christopher Roth. 2021. "Coronavirus Perceptions and Economic Anxiety." *Review of Economics and Statistics* 103 (5): 968–78.

Finseraas, Henning, and Andreas Kotsadam. 2017. "Does Personal Contact with Ethnic Minorities Affect Anti-Immigrant Sentiments? Evidence from A Field Experiment." *European Journal of Political Research* 56 (3): 703–22.

Fouka, Vasiliki, Soumyajit Mazumder, and Marco Tabellini. 2022. "From Immigrants to Americans: Race and Assimilation during the Great Migration." *Review of Economic Studies* 89 (2): 811–42.

Fouka, Vasiliki, and Marco Tabellini. 2022. "Changing In-Group Boundaries: The Effect of Immigration on Race Relations in the United States." *American Political Science Review* 116 (3): 968–84.

Giuliano, Paola, and Nathan Nunn. 2021. "Understanding Cultural Persistence and Change." *Review of Economic Studies* 88 (4): 1541–81.

Glover, Dylan, Amanda Pallais, and William Pariente. 2017. "Discrimination as a Self-fulfilling Prophecy: Evidence from French Grocery Stores." *Quarterly Journal of Economics* 132 (3): 1219–60.

Goodtables.io. 2020. "Country Codes." GitHub. https://github.com/datasets/country-codes/ (accessed October 4, 2023).

Grigorieff, Alexis, Christopher Roth, and Diego Ubfal. 2020. "Does Information Change Attitudes Toward Immigrants?" *Demography* 57 (3): 1117–43.

Halla, Martin, Alexander F. Wagner, and Josef Zweimuller. 2017. "Immigration and Voting for the Far Right." *Journal of the European Economic Association* 15 (6): 1341–85.

Hernandez, Donald J., Nancy A. Denton, and Suzanne E. Macartney. 2008. "Children in Immigrant Families: Looking to America's Future." *Social Policy Report* 22 (3): 1–24.

Krishnan, Gopal V., Zvi Singer, and Jing Zhang. 2020. "Audit Partner Ethnicity and Its Relation to Client Assignment, Audit Quality, and Discrimination." Unpublished.

Kunst, Jonas R., and David L. Sam. 2014. "It's on Time That They Assimilate—Differential Acculturation Expectations Towards First and Second Generation Immigrants." *International Journal of Intercultural Relations* 39: 188–95.

Kuriwaki, Shiro. 2023. "Cumulative CES Common Content." Harvard Dataverse. https://doi.org/10.7910/DVN/II2DB6.

Kuziemko, Ilyana, and Joseph Ferrie. 2014. "The Role of Immigrant Children in their Parents' Assimilation in the United States, 1850-2010." In *Human Capital in History: The American Record*, edited by Leah Platt Boustan, Carola Frydman, and Robert A. Margo, 97–120. Chicago, IL: University of Chicago Press.

Lowe, Matt. 2021. "Types of Contact: A Field Experiment on Collaborative and Adversarial Caste Integration." *American Economic Review* 111 (6): 1807–44.

Lowes, Sara, Nathan Nunn, James A. Robinson, and Jonathan Weigel. 2015. "Understanding Ethnic Identity in Africa: Evidence from the Implicit Association Test (IAT)." *American Economic Review* 105 (5): 340–45.

Lowes, Sara, Nathan Nunn, James A. Robinson, and Jonathan L. Weigel. 2017. "The Evolution of Culture and Institutions: Evidence from the Kuba Kingdom." *Econometrica* 85 (4): 1065–91.

Manson, Steven, Jonathan Schroeder, David Van Riper, Tracy Kugler, and Steven Ruggles. 2016. "IPUMS National Historical Geographic Information System: Version 17.0 [dataset]." IPUMS. http://doi.org/10.18128/D050.V17.0.

MIT Election Data and Science Lab. 2018. "County Presidential Election Returns 2000-2020." Harvard Dataverse. https://doi.org/10.7910/DVN/VOQCHQ.

Mousa, Salma. 2020. "Building Social Cohesion between Christians and Muslims through Soccer in Post-ISIS Iraq." *Science* 369 (6505): 866–70.

Paluck, Elizabeth Levy, Seth A. Green, and Donald P. Green. 2019. "The Contact Hypothesis Re-evaluated." *Behavioural Public Policy* 3 (2): 129–58.

Pettigrew, Thomas F., and Linda R. Tropp. 2006. "A Meta-analytic Test of Intergroup Contact Theory." *Journal of Personality and Social Psychology* 90 (5): 751–83.

Rao, Gautam. 2019. "Familiarity Does Not Breed Contempt: Generosity, Discrimination, And Diversity in Delhi Schools." *American Economic Review* 109 (3): 774–809.

Ruggles, Steven, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. 2015. "Integrated Public Use Microdata Series: Version 6.0 [dataset]." IPUMS. http://doi.org/10.18128/D010.V6.0.

Ruggles, Steven, Sarah Flood, Ronald Goeken, Megan Schouweiler, and Matthew Sobek. 2022. "IPUMS USA: Version 12.0 [dataset]." IPUMS. https://doi.org/10.18128/D010.V12.0.

Sanderson, Eleanor, and Frank Windmeijer. 2016. "A Weak Instrument F-test in Linear IV Models with Multiple Endogenous Variables." *Journal of Econometrics* 190 (2): 212–21.

Scacco, Alexandra, and Shana S. Warren. 2018. "Can Social Contact Reduce Prejudice and Discrimination? Evidence from A Field Experiment in Nigeria." *American Political Science Review* 112 (3): 654–77.

Schindler, David, and Mark Westcott. 2021. "Shocking Racial Attitudes: Black GIs in Europe." *Review of Economic Studies* 88 (1): 489–520.

Simple Maps. 2017. "United States Cities Database."Version: Basic. Simple Maps. Accessed on October 4, 2023. https://www.simplemaps.com/static/data/us-cities/uscitiesv1.3.csv

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

Steinmayr, Andreas. 2021. "Contact versus Exposure: Refugee Presence and Voting for the Far Right." *Review of Economics and Statistics* 103 (2): 310–27.

Sun, Liyang. 2018. "Implementing Valid Two-Step Identification-Robust Confidence Sets for Linear Instrumental-Variables Models." *Stata Journal* 18 (4): 803–25.

Tabellini, Marco. 2020. "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration." *Review of Economic Studies* 87 (1): 454–86.

Tausanovitch, Chris and Lynn Vavreck. 2021. Democracy Fund + UCLA Nationscape Project, October 10-17, 2019 (version 20211215). Request access to the full data can be submitted at https://www.voterstudygroup.org/data/nationscape.

Tausanovitch, Chris, Lynn Vavreck, Tyler Reny, Alex R. Hayes, and Aaron Rudkin. 2019. "Nationscape Methodology and Representativeness Assessment." Democracy Fund Voter Study Group. https://www.voterstudygroup.org/uploads/reports/Data/NS-Methodology-Representativeness-Assessment.pdf.

US Census Bureau. 2000a. "Census 2000, Profile of General Demographic Characteristics." US Census Bureau. https://www.census.gov/data/developers/data-sets/decennial-census.2000.html. (accessed October 4, 2023).

US Census Bureau. 2000b. "Census 2000, Profile of General Economic Characteristics." US Census Bureau. https://www.census.gov/data/developers/data-sets/decennial-census.2000.html. (accessed October 4, 2023).

US Census Bureau. 2000c. "Census 2000, Profile of General Social Characteristics." US Census Bureau. https://www.census.gov/data/developers/data-sets/decennial-census.2000.html. (accessed October 4, 2023).

US Census Bureau. 2010. "County Classification Lookup Table." US Census Bureau. https://www2.census.gov/geo/docs/reference/ua/County_Rural_Lookup.xlsx (accessed March 28, 2020).

US Census Bureau. 2018a. "American Community Survey 2018 Demographic and Housing Estimates." US Census Bureau. https://www.census.gov/programs-surveys/acs/data.html.

US Census Bureau. 2018b. "American Community Survey 2018 Selected Social Characteristics." US Census Bureau. https://www.census.gov/programs-surveys/acs/data.html.

US Census Bureau. 2018c. "SAIPE State and County Estimates for 2018." US Census Bureau. https://www.census.gov/data/datasets/2018/demo/saipe/2018-state-and-county.html (accessed April 14, 2020).

US Census Bureau. 2021. "Census Regions and Divisions of the United States." US Census Bureau. https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf.

US Department of Agriculture. 2012. "Commuting Zones and Labor Market Areas." United States Department of Agriculture. https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/.

Van Buskirk, Ian, Aaron Clauset, and Daniel B. Larremore. 2023. "An Open-Source Cultural Consensus Approach to Name-Based Gender Classification." *Proceedings of the International AAAI Conference on Web and Social Media* 17: 866–77

Vertier, Paul, Max Viskanic, and Matteo Gamalerio. 2023. "Dismantling the "Jungle": Migrant Relocation and Extreme Voting in France." *Political Science Research and Methods* 11 (1): 129–43.

Walker, Kyle. 2021. "Tidy Census." GitHub. https://github.com/walkerke/tidycensus/(accessed October 4, 2023).

Xu, Frank Kaiyuan, Nicole Lofaro, Brian A. Nosek, Anthony G. Greenwald, Jordan Axt, Lauren Simon, and Nicole Frost. 2013a. "Project Implicit Asian American IAT 2004–2020." OSF. https://osf.io/cpmfk/.

Xu, Frank Kaiyuan, Nicole Lofaro, Brian A. Nosek, Anthony G. Greenwald, Jordan Axt, Lauren Simon, and Nicole Frost. 2013b. "Project Implicit Race IAT 2002–2020." OSF. https://osf.io/52qxl/.

Xu, Frank Kaiyuan, Nicole Lofaro, Brian A. Nosek, Anthony G. Greenwald, Jordan Axt, Lauren Simon, and Nicole Frost. 2014. "Project Implicit Arab IAT 2004–2020." OSF. https://osf.io/t8u7p/.