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ABSTRACT

Employment Discrimination against Indigenous Peoples in the United States: Evidence from a Field Experiment*

We conducted a resume correspondence experiment to measure discrimination in hiring faced by Indigenous Peoples in the United States (Native Americans, Alaska Natives, and Native Hawaiians). We sent employers realistic resumes for common jobs (retail sales, kitchen staff, server, janitor, and security) in 11 cities and compared interview offer rates. We signaled Indigenous status in one of four different ways. Based on 13,516 applications, we do not find hiring discrimination in any context. These findings hold after numerous robustness checks, although our checks and discussions raise multiple concerns that are relevant to audit studies generally.

JEL Classification: J15, J7, C93

Keywords: indigenous peoples, employment discrimination, Native American, Alaska Native, Native Hawaiian, Indian reservations, correspondence experiment, resume study, Oaxaca-Blinder Decomposition

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Introduction

Indigenous Peoples[1] in North America faced perpetual injustices throughout history. A summary[2] includes, but is not limited to, the colonization, annexation, and military occupation of Hawaii (Silva 2004; Sai 2008), genocide (Thornton 1987), massacres (e.g., Wounded Knee, Brown 2007), forced relocation (e.g., the "Trail of Tears") and isolation in Indian reservations (Foreman 1972), disenfranchisement (Wolfley 1991), the slaughter of the bison (Feir, Gillezeau, and Jones 2017), and the forcible assimilation of Indigenous children through Indian boarding schools (Feir 2016b, 2016a; Adams 1995).

These injustices extend to contemporary racial disparities, which are some of the largest. Among racial and ethnic minorities, American Indians and Alaska Natives (AIANs) have the lowest employment-to-population ratio (54.6%, with 59.9% for whites), the highest unemployment rate (9.9%, with 4.6% for whites) (U.S. Bureau of Labor Statistics 2016), and they earn significantly less income (median income of $35,060 in 2010, compared to $50,046 for the nation as a whole) (U.S. Census Bureau, 2015).[3] Poverty rates among those who identify as AIAN alone are nearly double the rates of those in the general population (26.6% versus 14.7%) (U.S. Census Bureau, 2015). These disparities are even more substantial for the 22% of AIANs who

[1] The term Indigenous Peoples refers to those who lived in North America before colonization. Indigenous Peoples in the United States encompass a broad group including Native Americans (of which there are at least 566 identified tribal groups in the United States), Alaska Natives, and Native Hawaiians and other Pacific Islanders. In this paper, we use Indigenous Peoples to refer to the broad group of those who are Native American or Alaska Native (titled American Indian or Alaska Native, AIAN, in the U.S. Census), or Native Hawaiian (titled Native Hawaiian and other Pacific Islanders, NHPI, in the U.S. Census.)

[2] See Nabokov (1999) for a more detailed historical summary.

[3] These disparities are less stark for NHPIs as they have the highest employment-to-population ratio (62.8%); though, this reflects a stronger economy in Hawaii. Even absent this, unemployment rates are still higher for NHPIs relative to whites (5.7%, versus 4.6%) (U.S. Bureau of Labor Statistics 2016).

reside or used to reside on one of the 326 federal or state Indian reservations (U.S. Census Bureau 2015) or on Alaska Native Statistical Areas (U.S. Census Bureau 2011). These disparities are only becoming more relevant as Indigenous populations grow.[4]

Several factors could contribute to these disparities, such as differences in education, geography (especially Indian reservations), and the intergenerational legacy of colonialism.[5] Another possible explanation is employment discrimination. Survey evidence suggests that Indigenous Peoples face employment discrimination[6] and there are negative stereotypes against Indigenous Peoples that may lead to employment discrimination.[7] However, we are only aware of one peer-reviewed study that attempted to quantify employment discrimination against Indigenous Peoples in the United States (Hurst 1997).[8] Hurst (1997) decomposed the AIAN-white earnings

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[4] According to the 2010 Census, 5.2 million people identified as AIAN, alone or in combination (Norris, Vines, and Hoeffel, 2012) and 1.2 million people identified as Native Hawaiian or Other Pacific Islander (NHPI), alone or in combination (Hixson, Hepler, and Kim, 2012). The AIAN population is projected to grow to 8.6 million by 2050 (U.S. Census Bureau, 2015) and the NHPI population is also experiencing relatively rapid growth (Hixson, Hepler, and Kim 2012).

[5] Research on how historical mistreatment of Indigenous Peoples has led to current disparities includes Feir (2016a, 2016b), Adams (1995), and Feir, Gillezeau, and Jones (2017).

[6] In a survey of 342 Native American adults in the United States, 31% of respondents believed that they were discriminated against because they were Native American when applying for jobs (NPR, Harvard T.H. Chan School of Public Health, and Robert Wood Johnson Foundation 2017). See also https://www.justice.gov/opa/pr/justice-department-sues-south-dakota-state-agency-discrimination-against-native-american-job (accessed May. 1, 2016)

[7] Stereotypes, especially in the media, are that Native Americans are "savages" or "noble savages" (they are spiritual, wise, and have traditional beliefs and cultural traditions) (McLaurin 2012; Riverwind 2007). The stereotypes most closely connected with employment are that Native Americans are lazy, less interested in work, less educated or skilled, and rely on government handouts (Riverwind 2007; Schmidt 2007; Tan, Fujioka, and Lucht 1997; James et al. 1994). There is also the perception that Native Americans are more likely to suffer from alcoholism (Riverwind 2007; Tan, Fujioka, and Lucht 1997). For a broader discussion, see James et al. (1994).

[8] Research on discrimination against Indigenous people is somewhat more common for Canada (e.g., Feir, 2013) and Australia (e.g., Booth, Leigh, and Varganova, 2012). Many discrimination studies use focus on the United States, but they are all on other disadvantaged groups. See Neumark (2018) for a review of the experimental studies. Austin (2013) suggests that a resume-

gap using the Oaxaca-Blinder decomposition method. Hurst (1997) found that, while observable factors such as education and geography explain a large part of the gap (e.g., 87% of the earning gap between those who identify as AIAN alone versus white alone), there is "still a substantial unexplained differential in earnings between the various categories of Indians and non-Indians." (p. 805).

Quantifying employment discrimination against Indigenous Peoples is essential to inform policies to reduce these large economic disparities. If there is little discrimination, then disparities are primarily caused by factors other than employment discrimination like differences in education, which policy-makers could then target directly. However, if there is significant discrimination, then this suggests that supply-side policy measures like education or skills training[9] may be less effective at closing this gap. In this case, stronger discrimination laws, or stronger enforcement of them, could be more helpful, as could efforts that seek to reduce discriminatory attitudes or behaviors or our abilities to act upon them.

To quantify whether discrimination is behind these economic disparities, we conducted a field experiment of hiring discrimination—more specifically, a resume correspondence study—sending job applications to job openings. Field experiments such as ours are the preferred method of estimating employment discrimination because they can hold all factors other than minority

correspondence study of our nature for discrimination against Native Americans would be useful (pp. 25).

[9] For example, the Bureau of Indian Affairs' (BIA) Financial Assistance and Social Services (FASS) program (https://www.benefits.gov/benefits/benefit-details/801), the Native American Vocational and Technical Education Program (NAVTEP) (…/756), the U.S. Department of Labor's Division of Indian and Native American Program (DINAP) (…/81), the Indian Higher Education Grant Program (…/796), and the U.S. Department of the Interior's Job Placement and Training Program (…/797) (all accessed June 30, 2018).

status constant (Neumark 2018; Bertrand and Duflo 2016; Gaddis 2018) which is not the case for studies that use survey data (e.g., Hurst, 1997).

In our field experiment, job applications are identical on average but are either signaled to be white or Indigenous (Native American, Alaska Native, or Native Hawaiian). Our general approach follows previous studies of this nature (e.g., Pager, 2003; Bertrand and Mullainathan, 2004; Lahey, 2008; Neumark, Burn, and Button, forthcoming) by estimating hiring discrimination by comparing interview offer rates ("callbacks") by race. Since signaling Indigenous status is not straightforward, we use four different methods: first names for some Native Hawaiian applications, last names for some Native American applicants of Navajo ancestry, listing an Indigenous language along with English as mother tongues in a language section on the resume, or by mentioning Indigenous status in the description of a volunteer experience, mirroring Tilcsik (2011), Ameri et al. (2018), and Namingit, Blankenau, and Schwab (2017).

We also quantify whether there is additional bias against Native Americans from Indian reservations. Employers may have negative perceptions of these reservations, as poverty rates there are higher[10] and educational quality can be lower (DeVoe, Darling-Churchill, and Snyder, 2008). Estimating this potential bias has important implications given increased migration over time from Indian Reservations to urban centers (e.g., Snipp 1997, Pickering, 2000). Bias may be additional friction in the ability of Native Americans to successfully migrate to urban centers.

Our large-scale field experiment, based on 13,516 job applications in 11 cities and five occupations, shows no evidence of hiring discrimination against Indigenous Peoples. This holds even when we analyze the data separately by occupation, occupation-and-gender, and by city. Our

[10] Native Americans living on tribal lands were 10.1% more likely to live in poverty compared to those in rural areas (Collett, Limb, and Shafer, 2016).

estimates of no discrimination differ from the majority of similar field experiments of hiring discrimination that find discrimination against the minority group (Neumark 2018; Baert 2018). We similarly find no differences based on how we signal Indigenous status and no additional bias against Native Americans who lived on an Indian Reservation.

We conduct an extensive battery of robustness checks, including adjusting for the variance of unobservables as recommended by Neumark (2012) and Neumark and Rich (2018). We also carefully put our results in context and compare them to the previous literature. We conduct a complementary Oaxaca-Blinder decomposition of gaps in earnings, unemployment rates, and unemployment duration, to explore how our results compare to non-experimental estimates of discrimination and to determine what observable factors may be behind these disparities in economic outcomes. These checks and discussions suggest that our results are not due to choices in our experimental design, but they do shed light on concerns such as economic cycles and the saliency of audit study signals, which affect the interpretation of previous audit studies more broadly and should be considered by future researchers.

Field Experiment Design

In this section, we summarize how we designed our field experiment. We discuss issues such as our pre-analysis plan, how we signaled race, how we constructed the resumes, which jobs we targeted, and which cities we picked. Our goal was to design the field experiment to be as externally valid as possible, and we aim in this section to be transparent in our design, especially as our choices and discussion may be helpful to others designing these experiments. Additional details on the design of our field experiment are in Online Appendix A.

To briefly summarize the general experimental design, we sent two applications in a random order to each job in retail sales, server, kitchen staff, janitor, and security. One application was from an Indigenous applicant (Native American, Native Hawaiian, or Alaska Native), with the Indigenous status signaled in four possible ways (volunteer experience, language, first name, last name). The other application was from a non-Indigenous (white) applicant that had no minority signals. All applicants had a high school diploma and relevant work experience in the occupation, with resumes constructed partly from publicly-posted resumes on Indeed.com. We applied to jobs in 11 cities: Albuquerque, Anchorage, Billings, Chicago, Honolulu, Houston, Los Angeles, New York, Oklahoma City, Phoenix, and Sioux Falls. We measured discrimination by comparing callback rates – interview offers or other positive responses – by race. Figure 1 provides a diagram that summarizes our resumes and approach.

Pre-Analysis Plan

Before putting this experiment into the field, we filed a pre-analysis plan and registered it with the American Economic Association's Randomized Control Trial Registry.[11] The goal was to pre-specify any variables, models, sample sizes, or decisions to prevent data mining or p-hacking while simultaneously avoiding tying our hands too much in ways that would negatively affect our ability to conduct this research later (see Olken 2015 and Lahey and Beasley 2018). We discuss this pre-analysis plan in greater detail in Online Appendix B.[12]

Signaling Indigenous Status

[11] Few audit studies of discrimination are registered yet registering randomized control trials in other fields is standard. For our registered trial, see https://www.socialscienceregistry.org/trials/2299 (accessed December 26, 2017).

[12] We also explain a few minor deviations that we made to our analysis relative to the pre-analysis plan, although these minor deviations do not affect the results.

Indigenous people in the United States belong to numerous different tribal groups. Consequently, racial signals must be carefully chosen to be appropriate for each tribal group. Further, there is no obvious way to signal Indigenous status, and different possibilities have strengths and weaknesses. Names are most externally valid way to signal race, since names always need to be included, but names could be a weak signal or could signal socioeconomic status in addition to race (e.g., Fryer and Levitt 2004; Barlow and Lahey 2018). On the other hand, disclosing minority status through work or volunteer experience (e.g., Tilcsik 2011; Ameri et al. 2018; Namingit, Blankenau, and Schwab 2017) may be a strong signal but may be less externally valid since minority groups may prefer not to signal group affiliation to avoid potential discrimination.

We used four possible ways to signal that the job applicant is Indigenous: volunteer experience, languages spoken, first names for Native Hawaiians, and last names for Native Americans of Navajo ancestry. We present our matching of possible signals to Indigenous groups in Table 1 and explain these assignment decisions below (sample resumes are in Online Appendix H). We also test the robustness of our results to signal type in our robustness section and in Online Appendix D. We the test the saliency of our signals through surveys, discussed later and presented in greater detail in Online Appendix E and Online Appendix F.

Volunteer experience as an Indigenous signal.

Volunteer and work experience have been used before to signal minority status. Tilcsik (2011) and others signal sexual orientation through volunteer experience with a lesbian, gay, bisexual, and transgender (LGBT) or gay or lesbian group. Ameri et al. (2018) signal disability partly through a relevant volunteer experience as an accountant at a fictional disability group. Namingit, Blankenau, and Schwab (2017) disclose an illness-related gap in employment history partly through a volunteer experience (Cancer survivor's group) on a resume.

We follow a similar approach by using volunteer experience as one way to signal race. We used volunteer experience as a youth mentor with the Big Brothers and Big Sisters (BBBS) of America to signal race. In this volunteer experience, it is typical for "Bigs" to be matched with "Littles" based on race or other socioeconomic factors to improve mentorship. We list this in a volunteer experience section with a title such as "Youth Mentor," and a description such as: "I mentored youth in my [Native American/Native Hawaiian/Alaska Native] community. I worked with youth on social skills, academics, and understanding our [Native American/Native Hawaiian/Alaska Native] culture." For an example, see the example resumes presented in Online Appendix H.

A concern with using a volunteer experience to signal race is that this experience could be valuable to employers, independent of the racial signal.[13] To control for this, all resumes, regardless of race or signals used listed a volunteer experience. For the white resume in a pair where the Indigenous resume has the volunteer signal, the white resume has a volunteer experience either at a local Boys & Girls Club or at a local food bank. For any resume pair where the Indigenous applicant does not signal through volunteer experience, then one resume chosen at random gets the BBBS volunteer experience without a mention of race, and the other resume gets either Boys & Girls Club or food bank.[14] Thus, we can directly identify the effect of the BBBS experience, relative to the control volunteer experiences, separately from its use as a racial signal.

[13] However, similarly-constructed resume experiments did not find that the addition of similar volunteer experiences improved callback rates (Neumark, Burn, and Button, forthcoming).
[14] When this volunteer experience was listed on the resume and was not used to signal race, it was listed in a volunteer experience section with a title such as "Youth Mentor" and the description such as "I mentor youth in my community. I work with youth on social skills, academics, and community engagement."

However, we find no differences in callback rates by type of volunteer experience.

Language as an Indigenous signal.

We found few audit-correspondence studies of discrimination that used language as a signal of minority status.[15] The American Community Survey codes 169 AIAN languages, plus Hawaiian and Hawaiian Pidgin. While most Indigenous people primarily speak English, Indigenous languages are somewhat common: 26.8% of AIANs spoke a language other than English at home in 2014, compared to 21.2% nationally (U.S. Census Bureau 2015). Among those who identified as NHPI alone and were born in the United States, 30.3% spoke a language other than English at home (U.S. Census Bureau 2014). Since it is rare for non-Indigenous people to speak an Indigenous language, especially as a native speaker, this makes for a robust racial signal. We thus used Indigenous languages to signal Indigenous status in some cases for most (but not all) of the tribal groups since Indigenous language use varies by tribal group.[16] Table 1 presents the languages that we selected for each Indigenous group, and Online Appendix A presents our analysis of Census data to determine the frequency of each Indigenous language and thus to what extent signaling through language is appropriate.[17]

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[15] One example may be Oreopolous (2011) to some extent. Also, another study suggests that it would be a possibility. Behaghel, Crépon, and Barbanchon (2015) study the effect of randomly anonymizing resumes received by employers on outcomes for minority workers. While they do not construct "tester" resumes as in a typical audit-correspondence study, they note that language often signals race, ethnicity, or nationality on actual resumes.
[16] We did not use language to signal Indigenous status for individuals from the Osage or Blackfeet tribes since Indigenous language use by these tribes is very low.
[17] We used two approaches to determine which languages are spoken by which tribal groups. The first was to ascertain the languages historically spoken by the tribe. The second was to determine which Indigenous languages were spoken by individuals who live on the Indian reservations associated with the tribe. While not all individuals from a tribe live on a reservation, this was the only data-driven approach for us to investigate language use by the tribal group.

It is unclear how employers would view this signal. The ability to speak an Indigenous language may be seen positively by employers, either because the language could be used on the job (though this is rare) or because it is a signal of general ability.[18] On the other hand, speaking an Indigenous language may signal that the applicant is "more" Indigenous, either culturally or by ancestry, which may be disliked by discriminatory employers. It may also signal that the applicant has worse English skills even if it is made clear, as we do on the resumes, that the applicant speaks both languages natively.

To investigate this, we added the Irish Gaelic language as a control language to 10% of the white resumes. We added the Irish Gaelic language which, like Indigenous languages, is uncommonly-used in the United States. It is also one that is unlikely to signal that the applicant might have worse English skills since English is nearly universal in Ireland. While this control is imperfect, we find no difference in callback rates between resumes with an Indigenous language or Irish Gaelic or between resumes with Irish Gaelic and resumes with no other languages listed.

First name as an Indigenous signal (Native Hawaiian only).

We signaled race through first names for some Native Hawaiian applicants. We considered names within the top 100 baby names from Social Security records for the state of Hawaii.[19] We settled on three male names: Kekoa, Ikaika, and Keoni, and one female name: Maile. We

[18] For example, employers may see people that speak a second language (Indigenous or not) as of greater ability because it is difficult to learn a second language. Alternatively, employers may see individuals who learn a second language at home as more productive for other reasons (e.g., they were raised by more active parents).

[19] We first queried the United States Social Security Administration's "Popular Names by State" database for the state of Hawaii (https://www.ssa.gov/cgi-bin/namesbystate.cgi, accessed November 8, 2016). We considered names in the top 100 names for boys or girls born in 1985-1987 (corresponding to around age 30, the approximate age of our applicants).

confirmed that these were Native Hawaiian names through various sources.[20] When using the first name as a racial signal, we randomly assigned one of these names, conditional on gender. We did not use first names to signal race for Alaska Natives or Native Americans because there was little information on first names for these populations.[21]

Last name as an Indigenous signal (Native American, Navajo, only).

To find Indigenous-specific last names, we use tabulations from the 2000 Census of the racial composition of each last name.[22] Unfortunately, these data also do not include information on NHPI individuals, so we can only use this data to determine names for AIAN individuals. We used this data and other sources on the ancestry of names to select four names of Navajo origin: Begay, Yazzie, Benally, and Tsosie. These are among the most common last names that are almost exclusively held by individuals who identify as AIAN alone. Online Appendix A provides more details of our process for selecting these names.

We also considered the possibility of assigning some Native American last names that were perhaps stronger signals (e.g., Sittingbull, Whitebear). However, these names are rare.[23] These

[20] These sources were "allbabynames.net" (see, e.g., http://www.allbabynames.net/index.php?query=Kekoa), http://babynames.allparenting.com/US/States/Hawaii_A_Baby_Name_Paradise/, https://en.wiktionary.org/wiki/Appendix:Hawaiian_given_names, http://www.behindthename.com/names/usage/hawaiian, and http://www.alohafriends.com/names_traditional.html (all accessed November 13, 2016). All names appear in each source, except Maile does not appear for the last source.

[21] For example, there is no Census or Social Security Administration tabulation of first names by race as there is for last names (Tzioumis 2018) and there is little information that suggests that Native American or Alaska Native first names are sufficiently common. Furthermore, no Alaska Native-specific names appear in the Social Security database in Alaska for the years 1985-1987.

[22] The tabulations provide a list of 151,671 last names. For each last name, there is an estimate of the number of people per 100,000 people with this last name and the proportion of people with this name that reported each race. See http://www2.census.gov/topics/genealogy/2000surnames/names.zip (accessed June 25, 2016).

[23] For example, "Whiteagle" only occurred for 0.16 people per 100,000 people, and "(Fast/Yellow/White)horse" only occurred for 0.14 people per 100,000 people, each. Even

names are also difficult to assign appropriately to tribal groups. Further, we had concerns that the names, especially the very rare ones that did not appear in the Census data, signaled stereotypical tropes of Native Americans from popular media (McLaurin 2012; Tan, Fujioka, and Lucht 1997).

Assigning racial signals.

Table 1 summarizes which of the signals we used as options for each tribal or Indigenous group. We allocated Indigenous signals as follows. For Navajo and Native Hawaiian applicants, where three signals were possible, we assigned signals with the following probabilities: Name only (30%), Language only (25%), Volunteer only (25%), Name and Language (5%), Name and Volunteer (5%), Language and Volunteer (5%), and all three (5%). For Alaska Native, Apache, Tohono O'odham, and Oglala Lakota applicants, where language and volunteer were possible, we assigned signals with the following probabilities: Language only (40%), Volunteer only (40%), and both (20%). For Osage and Blackfeet applicants, only the volunteer signal was possible. Assigning more than one signal allowed to test whether discrimination increased when saliency, through having multiple signals, was higher.

Indian Reservation Upbringing

We assigned half of the Native American applicants an upbringing on an Indian reservation rather than in the city. We signaled this through having graduated from a high school on an Indian reservation, rather than a local high school. We considered seven Indian reservations, as shown in Table 1. These fall within the top ten most populous reservations (Norris et al., 2012). We used one to three high schools per reservation, depending on availability. We specifically chose high schools with names that were a clear signal that the high school was on an Indian reservation. We

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summing over all these names that were perhaps more salient, they were not sufficiently frequent.

also specified the location of the high school as "City, Reservation Name, State" to ensure the saliency of this signal. For the white, Native Hawaiian, and Alaska Native resumes, and the other half of the Native American resumes without an Indian reservation upbringing, we assigned one of two to four high schools local to the city (from Neumark, Burn, and Button, forthcoming, and Neumark, Burn, Button, and Chehras, 2018).[24]

For half of the Indigenous applicants with an Indian reservation upbringing, we also had their first job out of high school (the least recent job, Job 3, as in Figure 1) listed on the resume as having been on the reservation, while the others had a local job. In addition to strengthening the reservation signal, this on-reservation work experience is realistic for many Indigenous people who grew up on an Indian reservation and later migrated to a city. Since we randomized the addition of this on-reservation work experience, we can identify whether this has any independent effect beyond the location of the high school. A typical entry-level job on a reservation that was also common off a reservation, according to publicly posted resumes on Indeed.com, was a cashier at a grocery store. Thus, for pairs of applicants where we sent Native American applicants, we set Job 3 (see Figure 1) for both resumes to be a cashier at a grocery store, with the store location either being on the reservation or in the local city. All subsequent jobs are in the targeted occupation. Thus, the only change when we included this reservation job was the location of Job 3.

Employers may prefer local or non-rural applicants, which challenges our ability to identify differential treatment by Indian Reservation upbringing. We investigate this by randomly assigning a rural upbringing to white resumes in pairs where we sent a Native American resume.

[24] These schools are ones that have been around for a while and that do not signal any race or ethnicity (e.g., no historically Black schools).

We added a high school in a small town to 25% of these white resumes, and then in half of these we also assigned a Job 3 location in that same rural town, mirroring the reservation job.[25] Adding reservation signals may also increase the likelihood that the employer detects that the applicant is Native American. We attempted to control for this by sometimes assigning Indigenous applicants to have more than one racial signal to see if this affects results (it does not).

Cities

We focused on cities where more Indigenous Peoples live to get estimates of discrimination that better reflect their experiences. We applied for jobs in eight of the ten cities with the most people who identify as AIAN (Norris, Vines, and Hoeffel 2012). These are, in decreasing order of AIAN population: New York, Los Angeles, Phoenix, Oklahoma City, Anchorage, Albuquerque, Chicago, and Houston.[26] We then added two additional smaller cities with a larger proportion who are AIAN: Billings and Sioux Falls. Billings and Sioux Falls are also noteworthy because these cities are near a few Indian reservations of interest (e.g., Pine Ridge).[27]

To study discrimination against Native Hawaiians, we applied to jobs in Honolulu, the city with the most Native Hawaiians. We also applied for some jobs in Los Angeles with Native Hawaiian applicants, as Los Angeles is the most common mainland city for Native Hawaiians to live in (Hixson, Hepler, and Kim 2012).

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[25] We specifically chose these small towns to match with each reservation such that both the reservation and small towns were about an equal distance from the city (see Online Appendix Table A2).

[26] We excluded cities from within states already represented. Those excluded were Tulsa (rank of 6) since it is similar to Oklahoma City (rank of 4) and San Antonio (rank of 10) since it is similar to Houston (rank of 9).

[27] The Pine Ridge Indian Reservation is notable because of its extremely high poverty rates and its many other challenges. See, e.g., Pickering (2000) and media coverage such as https://www.cnn.com/videos/politics/2017/05/26/pine-ridge-indian-reservation-forgotten-americans-orig-js.cnn (accessed January 22, 2019.)

Occupations

We chose occupational categories where there were many jobs posted online that usually allowed applications by email and were common for applicants of about age 30. Tables 2 and 3 show the popularity of our selected occupations and these tables present statistics on the race and gender of those in each occupation, based on the Current Population Survey (CPS).[28] The most popular occupations differed significantly by gender, and much less by race.[29] Accordingly, we settled on jobs in five broad occupations: retail sales, kitchen staff, server, janitors, and security guards.[30] [31] We used male and female applicants for all occupations except security guard as women infrequently hold that position.

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[28] For this analysis of CPS data, we use an age range of 25 to 35, we define "white" as "white only," and we define AIAN (NHPI) as "AIAN (NHPI) alone or in combination." See Online Appendix A for additional details. This appendix also has expanded tables (Online Appendix Tables A3 and A4) showing similar statistics for other occupations, allowing a comparison of our selected occupations to other popular occupations.

[29] Of the 38 most popular occupations for white men (Online Appendix Table A3) and white women (Online Appendix Table A4), only 13 appear on both lists. For men, 25 of the 38 most popular occupations for AIAN men (18 for NHPI men) are also in the top 38 for white men. This is 27 (23) for AIAN women (NHPI women), compared to the list of 38 for white women.

[30] We note that other occupations that we did not select were also feasible. We chose security instead of drivers since driver jobs are commonly moving to companies like Uber and Lyft and because we already had the inputs to make security resumes from a previous study (Neumark, Burn, and Button, forthcoming). We also found security interesting to study given the relatively higher concentration of Indigenous men. We opted for server and kitchen staff over customer service because customer service has some overlap with retail sales, which we had already included. While we could have applied for administrative and secretarial positions as in Neumark, Burn, and Button (forthcoming), we decided to avoid doing so since the applications to those jobs in that study elicited many spam responses that made data collection less accurate and more time-consuming. This occupation was also only common for women.

[31] We group the occupational categories from the CPS into broader occupations, to match the job postings, as follows: retail sales (corresponding to retail salespersons; cashiers; counter and rental clerks; sales representatives, services, all other; and sales and related workers, all others, in the Census occupational classification), kitchen staff (cooks; food preparation workers; dishwashers; combined food preparation and serving workers, including fast food; counter attendants, cafeteria, food concession, and coffee shops; food servers, non-restaurant; and dining room and cafeteria attendants and bartender helpers), server (waiters and waitresses; bartenders; and hosts and hostesses, restaurant, lounge, and coffee shop), janitors (janitors and building

Education

All applicants had a high school diploma only. We focused on this group for a few reasons. First, it is much less common for Indigenous Peoples to have a post-secondary education.[32] Second, advanced degrees are usually not required in our selected occupations. Third, we wanted to focus on somewhat less-educated individuals who might be closer to the margins of poverty.[33]

Job Histories

We modeled our resume design and descriptions off of real publicly-posted resumes from Indeed.com. This improved the external validity of our experiment. We randomly assigned three jobs with matching job descriptions from a list of twelve possible jobs per city and occupation combination. The employer, job title, and address were taken from actual resumes or collected from active businesses. We randomly generated job tenure distributions, conditional on all three jobs spanning high school graduation to near the present.[34] All applicants within each pair were either both employed with 25% probability or both unemployed (as of the month before the job application) with 75% probability.[35] Since kitchen staff jobs are very heterogeneous, covering

cleaners and grounds maintenance workers), and security guards (security guards and gaming surveillance officers).

[32] According to data from the Current Population Survey, 33.2% of those who identify as white only and non-Hispanic have at least a bachelor's degree, while this is only 15.2% (22.3%) for those who identify as AIAN alone (NHPI alone) (see Online Appendix Table G1.)

[33] While it is possible to create resumes for applicants without a high school diploma, almost all jobs require this or a GED. Assigning a GED is also possible, but these are also not particularly common, and we wanted to focus our statistical power on detecting the effects of race.

[34] We randomly set the transition period between jobs to be the same month, one month later, two months later, or three months later, all with equal probability.

[35] During the field experiment, every month we moved the ending date of the most recent job forward one month so that unemployment durations did not lengthen during the experiment.

experienced cooks down to entry-level dishwashers, we created separate resumes for cooks and more entry-level positions (e.g., food preparation, fast-food, dishwasher).[36]

Age and Names

We set the age of all applicants to be approximately 29 to 31, via a high school graduation year of either 2004 or 2005, randomly chosen (we applied for jobs in 2017). We used first names that were common for those of this age based on common baby names taken from Social Security data.[37] For last names, we randomly assigned one of the last names used in Neumark, Burn, and Button (forthcoming) who used names from Social Security Administration tabulations of popular last names by birth year.

Residential Addresses, Phone Numbers, and Email Addresses

Within each set of applications sent in response to an ad, all applications were from different residential addresses, which were randomly assigned. We used addresses from Neumark, Burn, and Button (forthcoming) and Neumark et al. (2018).[38] We assigned each of our applicants a unique email address and one of 88 different phone numbers.[39]

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[36] While we pool all these kitchen staff jobs together in our analysis, our results are the same if we analyze cook jobs separately from the others. These results are available upon request.
[37] See https://www.ssa.gov/oact/babynames/#andht=1 (accessed May 20, 2016). We borrowed the list of names from Neumark, Burn, and Button (forthcoming).
[38] These addresses were selected carefully to ensure that they did not signal a race other than white and were not likely to send an unusual signal (positive or negative) about the socioeconomic status of the applicant. These addresses also were not too far from the central business district(s) in the metro areas.
[39] We purchased enough phone numbers to assign unique numbers to bins of job applicants defined by city, race (white or Indigenous), and occupation (retail sales, server, kitchen staff, janitor, and security, with janitor and security pooled into one set of numbers). This resulted in 88 unique phone numbers. With all of these numbers and other matching methods (further discussed in Online Appendix A), it was highly unlikely that we could not assign a response to an applicant.

Collecting Data

Pairing Resumes to Send to Job Ads

After creating the final resumes, we combined them into pairs to apply to each job (see Figure 1). Each pair always had one white and one Indigenous applicant. The tribal group of the Indigenous applicants depended on the city in which we applied. Table 4 presents our allocations. All other resume characteristics were randomized with replacement except the following: first and last names, resume template styles, addresses, email address domain, employers listed in the job history, exact phrasing describing skills or jobs on the resume or cover letter, and the specific volunteer experience. This was to ensure that the resumes were sufficiently differentiated.

Sample Size

In our pre-analysis plan,[40] we conducted a power analysis to determine how many observations would be necessary to detect meaningful differences in callback rates between Indigenous and white applicants. Based on previous studies, we decided that we wanted to have the power to detect at least a three percentage point difference in the callback rate. Based on our calculations, we anticipated needing to apply to 4,211 jobs (8,422 applications). We ultimately decided to collect more data (13,516 total applications) to have the power to detect differences smaller than three percentage points and to detect other mediators of discrimination with more precision (e.g., reservation upbringing, geography, gender, and occupation). We followed our commitment in our pre-analysis plan to do our principal analysis both with the ultimate sample size (13,516) and with 8,422 applications. Our results are similar either way (see Online Appendix Table B1).

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[40] See Online Appendix B and https://www.socialscienceregistry.org/trials/2299 (accessed December 26, 2017).

Identifying Job Ads

We identified viable jobs to apply for using a common job-posting website.[41] The jobs needed to fit the correct description for our occupational categories, be for non-manager or non-supervisor roles, and not require in-person applications, inquiries by phone, or application through an external website. We ignored job ads that required documents that we did not prepare (e.g., headshots or salary history) or required skills,[42] training, or education that our resumes did not have. We applied for jobs between March 2017 and December 2017.

Emailing Applications

We used a different email subject line, opening, body, closing, and signature order for each application in a pair to ensure that applicants from the same pair were not perceived as related. We based some of these scripts on examples and advice from job search experts.[43] The content of our emails mirrored cover letters, and we followed the standard practice for these jobs of including this content in the body of the email (requests for separate cover letters were rare).

Coding Employer Responses

We coded employer responses as positive (e.g., "Please call to schedule an interview"), ambiguous (e.g., "We reviewed your application and have a few questions"), or negative (e.g., "We have filled the position"). To avoid having to classify the heterogeneous ambiguous responses through a subjective process, we follow others (e.g., Neumark, Burn, and Button, forthcoming) and treat only positive and ambiguous responses as callbacks, but our results are robust to using

[41] We discuss the process that our research assistants followed in detail in Online Appendix A.
[42] We also ignored job ads that required a quality element (e.g., a skill) that was part of the vector of randomized quality features that we added to the resumes to correct for the variance of unobservables issue. See Online Appendix C for more details.
[43] See https://www.thebalance.com/writing-a-letter-of-application-for-employment-2061570 (viewed August 20, 2016).

strict interview requests only (see Online Appendix Table D5).

Data Analysis Methodology

We started by testing how callback rates differed by Indigenous status, then explored how any possible discrimination varied by Indigenous group, Indian reservation upbringing, occupation, gender, or by city. We then conducted a battery of robustness checks, including testing how our discrimination estimates varied by the Indigenous signal(s) we used.

Callback Rates by Indigenous Status and Indian Reservation Upbringing

We first assessed callback rates by race without regression controls. For this analysis, we computed raw callback rates by race and used an exact Fisher test (two-sided) to test whether callback differences were statistically significantly different by race. First, we pooled all Indigenous groups together to test for a difference between white and Indigenous applicants. Then we compared Native American, Alaska Native, Native Hawaiian, and white applicants separately.

We then moved to a regression model and controlled for other resume features to improve precision and to test the sensitivity of the results to the inclusion of control variables. More importantly, we added controls for city to account for how we sent different types of resumes (Indigenous status, Indian reservations, rural upbringing controls) by city. In this regression, we also investigated whether discrimination against Native Americans differed if they had an upbringing on an Indian reservation. Our regression is:[44]

[44] In our pre-analysis plan, we originally committed to using a probit model. However, we became aware that it was more common to use a linear probability model due to issues with coefficients on interaction terms in non-linear models (Ai and Norton 2003; Greene 2010). Our probit results are similar and we present them in Online Appendix Table D1.

$$Callback_i = \beta_0 + \beta_1 NA_i + \beta_2 NA_i * Reservation_i + \beta_3 NA_i * Reservation_i * Reservation\,Job_i + \beta_4 AN_i + \beta_5 NH_i + \beta_6 Rural_i + \beta_7 Rural_i * Rural\,Job_i + Controls_i \beta_8 + \varepsilon_i \quad [1]$$

where *i* indexes each application, *NA* is an indicator variable for being Native American, *AN* is an indicator variable for being Alaska Native, *NH* is an indicator variable for being Native Hawaiian, *Reservation* is an indicator variable for being a Native American applicant who grew up on an Indian Reservation, *Reservation Job* is an indicator variable for being a Native American applicant who grew up on an Indian Reservation and their oldest job listed on the resume (first job out of high school) was on the reservation, *Rural* is an indicator variable for being a white applicant who grew up in a rural area, and *Rural Job* is an indicator variable for being a white applicant who grew up in a rural town and their oldest job was in the rural town. White is the excluded racial category, so all estimates reflect callback differences relative to white applicants. *Controls* is a vector of resume controls. We used three versions: (1) no resume controls (to match the raw tabulations), (2) regular controls[45] (the default for all our analysis), and (3) full controls, which includes additional controls[46] on top of the regular controls.

Following Neumark, Burn, and Button (forthcoming), we cluster our standard errors on the resume. There may also be random influences at the level of the job ad, which would suggest clustering on the job, or multi-way clustering on the job and the resume simultaneously (Cameron,

[45] The regular controls are indicator variables for employment status, resumes skills (Spanish, no typos in cover letter, better cover letter, and two occupation-specific skills), occupation, gender, resume sending order, volunteer experience, and city.
[46] The additional controls included in full controls are graduation year (we randomize between two years), resume naming style, e-mail script version, e-mail format, e-mail subject, e-mail opening line, e-mail body, e-mail signature format, e-mail domain, voicemail greeting, oldest job (job 3) start month, gap (in months) between job 3 and job 2, gap between job 2 and 1, and duration of volunteer experience (in months).

Gelbach, and Miller 2011). The difficulty with clustering on the job is that we cannot match all responses perfectly to job ads, leading to a restricted sample.[47] However, our results are unchanged regardless of how we cluster our standard errors (see Online Appendix Table D2).

After conducting this primary analysis, we then conduct regressions to analyze callback rates for Indigenous Peoples, compared to whites, separately by occupation, occupation and gender, and by city. In these and all subsequent analysis we use the regular controls.

Results

Effects by Race and Indian Reservation Upbringing

Table 5 presents the raw callback rates by race. The callback rates were nearly identical for whites and Indigenous Peoples at 19.8% and 20.1%, respectively. By subgroup, the callback rates were 19.6% for Native Americans, 21.3% for Native Hawaiians, and 25.5% for Alaska Natives. Exact Fisher tests (two-sided) find that Alaska Natives had a statistically significantly higher callback rate compared to both whites and Native Americans (both at 5% level).[48] However, these estimates do not control for city-specific callback rates, and higher callback rates for all applicants in Anchorage almost certainly explain these results.[49]

In Table 6 we estimate regressions, following Equation [1], to determine callback differences by race. The results without controls (column (1)) show again that Alaska Natives have

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[47] Since we assign multiple applicants the same phone number, we are sometimes not able to match a voicemail response to a specific job even if we can match it to a specific resume because the voicemail is sparse on details (e.g., applicant name, company) that would typically facilitate a match.
[48] This test treats the observations as independent. Our regression analyses that follow clustered our standard errors so as not to assume independence.
[49] The callback rate for white applicants in Anchorage was 24.8%, and this was much lower for whites in the entire sample (19.8%).

a statistically significantly higher callback rate compared to whites. However, adding the regular controls (column (2)), which includes city fixed effects, removes this difference. In the regression with regular controls, our preferred and default specification, Native American applicants (without a reservation upbringing) have only a 0.4 percentage point lower callback rate, but this is not statistically significant. Alaska Natives (Native Hawaiians) have a 0.5 percentage point higher (0.3 percentage point lower) callback rate, but this is again not statistically significant.

After adding controls, such as city fixed effects (column (2)), the callback rates are identical for Native Americans with and without a reservation upbringing. Callback rates are 0.6 percentage points higher for those who worked on the Indian reservation, compared to those who just went to high school on the reservation, but this is again not statistically significant. All these near zero or small estimates are robust to the inclusion of the full set of controls (column (3)). Therefore, these regression estimates show no evidence of discrimination.

Effects by Occupation and Gender

Table 7 presents the results by occupation. For all occupations except security, the callback rates are nearly identical for Indigenous and white applicants. For security we see a 1.1 percentage point higher callback rate for Indigenous applicants, but this is again statistically insignificant.

Table 8 presents results by occupation and gender. The estimates show no differential treatment of Indigenous men compared to white men. We find a strong preference for female applicants for server positions, a 6.5 percentage point higher callback rate for white women compared to white men (who have a callback rate of 13.3%). Similarly, and as found in previous work (e.g., Neumark, Burn, and Button, forthcoming; Neumark et al. 2018), we find a preference for women in retail sales: a 3.7 percentage point higher callback rate for white women compared to white men (who have a callback rate of 16.3%).

Effects by City

Table 9 shows results by city. Again, there are largely no differential results.[50] Callback differences are within two percentage points for all cities except Phoenix (Albuquerque) where Indigenous applicants have a 4.1 percentage point higher (3.7 percentage point lower) callback rate. Only the estimate for Phoenix is statistically significant, but only at the 10% level.[51]

Robustness Checks and Putting our Results in Context

We discuss numerous additional robustness checks and considerations to convince the reader that our results are generally not due to errors or choices in our experimental design or analysis. Our broader discussion and battery of checks highlighted below brings attention to the limitations of our experiment, but also to the limitations of other studies. We emphasize that these checks would be useful for others to do, regardless of the outcome of their studies. We also hope that this broader discussion puts our results in the proper context, and makes clear what we learn, and still do not know, about discrimination faced by Indigenous Peoples in the United States.

Estimates by Indigenous Signal Type

To explore whether our results differed based on the four ways we signal Indigenous status (volunteer experience, language, Native Hawaiian first name, and Navajo last name), we analyzed callback rates by Indigenous signal type as follows:

[50] We also ran an additional regression but with additional three-way interactions between *NA*, *Reservation*, city, to see if the effects of reservation upbringing also varied by city. The results, presented in Online Appendix Table D12, show no differences by city.

[51] Although with 11 cities, we would expect about one city, on average, to have a significant estimate at the 10% level even absent any actual effects.

$$
\begin{aligned}
Callback_i = \; & \beta_0 + \beta_1 Volunteer\ Only_i + \beta_2 Language\ Only_i + \beta_3 First\ Name\ Only_i \\
& + \beta_4 Last\ Name\ Only_i + \beta_5 Two\ Signals_i + \beta_6 Three\ Signals_i \\
& + \beta_{12} Boys\&Girls_i + \beta_{12} FoodBank_i + \beta_{12} Gaelic_i + Controls_i \beta_{13} + \varepsilon_i
\end{aligned} \qquad [2]
$$

where *Volunteer Only* is an indicator variable for being an Indigenous applicant with the volunteer (Big Brothers & Big Sisters) signal only, *Language Only* is an indicator variable for being an Indigenous applicant with the language signal only, *First Name Only* is an indicator variable for being a Native Hawaiian applicant with the first name signal only, *Last Name Only* is an indicator variable for being a Native American applicant of Navajo ancestry with a Navajo last name only, *Two (Three) Signals* is an indicator variable for any combinations of two (three) signals, *Boys & Girls* is an indicator variable for having the Boys & Girls Club control volunteer experience, *Food Bank* is an indicator variable for having the food bank control volunteer experience,[52] and *Gaelic* is an indicator variable for having the Irish Gaelic control language.[53]

Table 10 presents the estimates by signal type, from Equation [2]. The results do not differ by the signal. For Indigenous applicants who have the volunteer signal only, the callback rate is 0.6 percentage points lower, but this is statistically insignificant (standard error of 1.0). The estimates on the controls for volunteer experiences are also statistically insignificant, which suggests that regardless of which control volunteer experience is used (Boys & Girls Club, Food Bank, Big Brothers Big Sisters without Indigenous signal), there is no difference in callback rates.

[52] The excluded category is the Big Brothers & Big Sisters control volunteer experience, which is added randomly to one of the resumes in pairs where the Indigenous applicant does not use the volunteer signal.

[53] We also replaced the single *First Name* and *Last Name* variables with indicator variables for each possible Native Hawaiian first name (Maile, Kekoa, Ikaika, and Keoni) and each possible Navajo last name (Begay, Tsosie, Benally, Yazzie). This was to see if the results differ by the randomly chosen name, which was not the case. These results are available upon request.

Results are similar for the language signal. For Indigenous applicants who have the language signal only, the callback rate difference is also small (0.6 percentage points higher). The control for the Indigenous language (Irish Gaelic) is statistically insignificant but is larger and negative (a 1.7 percentage point lower callback rate).

The estimates with two or three signals are positive but again statistically insignificant. These estimates are imprecise, however, for three signals, given that most resumes had only one or two signals. Thus, there is no evidence to support that having multiple signals decreases the callback rate. The fact that there is no difference in callback rates by Indian reservation upbringing is further evidence that our discrimination estimates do not vary by signal type or by saliency.

Saliency of Signals

A key question in any correspondence study is whether the tested subjects detected and correctly interpreted the signal(s) of minority status. Usually this is just assumed to be the case. We are only aware of a few studies that carefully test for saliency and interpretation of signals (Kroft, Notowidigdo, and Lange, 2013; Lahey and Oxley, 2018). If the signal is not detected, or is only detected sometimes, then results are attenuated towards zero. If the signal is interpreted differently than intended (e.g., a different minority is assumed, or the signal also conveys socioeconomic status) then the results may not reflect what the experimenters expect to test (Fryer and Levitt, 2004; Gaddis, 2017; Barlow and Lahey, 2018). We use four different signals in our study (volunteer experience, language, Native Hawaiian first name, and Navajo last name). Despite our results not differing by signal type, or when more than one signal is used (Table 10), it still may be the case that each signal has different levels of saliency. To investigate this, we fielded two surveys, both described in more detail in Online Appendix E (“resume survey”) and Online Appendix F (“names survey”).

First, we fielded the resume survey, a survey similar to Kroft, Notowidigdo, and Lange (2013). Specifically, we asked individuals on Amazon Mechanical Turk to read one of the resumes from our study and to consider the candidate for a job position in the relevant occupation. We then asked the subjects to recall characteristics of the applicant (race or ethnicity, languages spoken, age, education, employment status). We included surveys showing resumes without signals (white) or with some combination of signals for either Native American or Native Hawaiian applicants. We included respondents from both a national sample and separately an Arizona and New Mexico only sample for the Navajo resumes given that relatively more Indigenous Peoples live in those states.[54]

More details and results from this resume survey are in Online Appendix E. To summarize, the white resumes (no signals) are usually identified as white (86.8% of the time). However, resumes with a Native American (Native Hawaiian) signal were detected as AIAN (NHPI) at rates between 18.8% to 74.2% (26.4% to 82.0%).[55] More specifically, the Navajo last name only signal is very weak (18.8%) compared to the language signal only (32.4%) or the volunteer signal only (37.2%), which are stronger, but still not strong. Saliency is significantly higher when using more than one signal, ranging from 58.0% for Navajo last name and volunteer experience to 74.2% for Navajo last name and Navajo language listed. Looking just at respondents in Arizona and New Mexico, the probability that applicants were identified correctly as AIAN was significantly higher,

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[54] The additional surveys in Arizona and New Mexico were for two reasons. First, oversampling those two states more closely aligns our survey sample with our experiment sample. Second, we can explore how saliency differs when exposure to Indigenous Peoples is higher.

[55] For Native American resumes, conditional on not guessing AIAN, the most common guess was white. Interestingly, a non-trivial proportion of respondents (12.9% to 15.8%) indicated that they thought the Native American individual was instead NHPI when signal(s) other than Navajo last name were used (columns (4), (5), and (8)). In these cases, they identified the applicant as Indigenous but of the wrong racial or tribal group.

ranging from 58.3% (Navajo last name only) to 76.7% (Navajo last name and Navajo language listed). Saliency for the Native Hawaiian resumes is 26.4% for first name only, 82.0% for language only, and 75.0% for volunteer and language. We would expect these saliency rates to be even higher in a Hawaii-only sample relative to this national sample.

For comparison, and following Kroft, Notowidigdo, and Lange (2013), we also measured the saliency of other aspects of the resume that are often used to signal minority status or other essential resumes features. We measured the saliency of gender, age, highest completed education, employment status (employed vs. unemployed), duration of the last job held, and whether a second language was listed. Across all tested resumes, survey respondents correctly identified gender 71.4% of the time,[56] highest completed education 86.4% of the time, employment status 68.3% of the time, and correctly recalled whether there was a second language on the resume 75.3% of the time. As for age (duration of the last job held), the mean of identified minus actual was -1.60 years (-0.90 years), with a standard deviation of 4.69 years (3.15 years). These results suggest that other signals range from having only moderate strength (e.g., employment status) to being reasonably strong, but were not always detected (e.g., highest completed education).

We learn two things from all these results of the resume survey. First, our signal combinations are occasionally detected more often than other resume features, where saliency may be assumed to be obvious (e.g., employment status, 68.3% saliency). This suggests that one should never assume that signals will always be detected. Researchers should generally test for the saliency of their signals and discuss how this effects their results. Second, our signals on average are less salient than other resume features so our results could be attenuated.

[56] We calculate this using a sample of the first names that strongly signal gender (i.e., no ambiguous names like Pat, Casey, or Taylor) and generally signal that the individual is white.

This attenuation concern prompted us to explore further how our results vary by signal type, going beyond our analysis by signal type in Table 10. Since the saliency of the Navajo last name signal only was low, mostly outside Arizona and New Mexico, we conducted three additional robustness checks where we: (1) recoded those with Navajo last names as the only signal as “white”; (2) controlled for resumes with the Navajo last name only with a separate indicator variable; and (3) re-estimated Equation [2] (like in Table 10) but recoded the signals as if the Navajo last name signal did not exist. As shown in Online Appendix Tables D3 and D4 these tests again do not change our results.

Despite this Navajo last name signal being particularly weak, more-so outside of Arizona and New Mexico, we learn from this survey and our experimental results that individuals with these names are only sometimes going to be perceived as Indigenous and do not face hiring discrimination at the callback stage just based on their name. This is not the case for the vast majority of other minorities as the numerous studies using other minority names find discrimination (Neumark 2018; Baert 2018).

We also learn from this that it is essential to test the names used to ensure that they signal what is intended. Here we echo concerns in recent work that carefully explores how names signal race, ethnicity, and socioeconomic status finding that individual names may not signal what researchers assume and specific names can drive results in unexpected ways (Barlow and Lahey 2018; Gaddis 2017b, 2017a). We tested the names we used in the resume survey, discussed earlier. We also fielded a second survey on Amazon Mechanical Turk specifically on our Navajo last names, similar to how Gaddis (2017a, 2017b) tests names. We simply showed those surveyed a name (e.g., Daniel Begay, Emily Adams) and asked them to indicate to which race they thought that individual belonged.

We present more details and full results from both surveys in Online Appendix E (resume survey) and Online Appendix F (names survey). For example, in the names survey, out of the Navajo last names, saliency was highest for Tsosie (47.1% nationally thought this person was AIAN and 70.0% in Arizona and New Mexico only), followed by Yazzie (12.5%, 28.6%), Begay (10.0%, 35%), and Benally (5.7%, 15%).[57] [58] We also learn from both surveys that individuals perceive Indigenous Peoples to be more likely to have been born outside the United States – an odd result, but one seen in other research including using the Native Implicit Association Test (Native IAT).[59]

Statistical Power

A possible reason generally for a lack of statistically significant results is low power, but this is not a problem we face for our main results. As discussed earlier, we have significantly more observations than our power analysis required, and we have the seventh largest sample size relative to the other 113 resume-correspondence studies of hiring discrimination summarized in Baert

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[57] The saliency of the names in the names survey is higher than in the resume survey, likely because the resume survey showed resumes instead of just names, so recall was lower. Imperfect recall may also explain why individuals do not always remember less controversial signals like employment status.

[58] For Native Hawaiian first names in the resume survey, using a national sample, the most salient name was Keoni (58% NHPI) followed by Ikaika (24%), Kekoa (14%), and Maile (10%), suggesting that most names were not salient to Americans in general. Due to issues with Amazon Mechanical Turk, we were unable to conduct this survey using a sample of Hawaii residents only. We would expect saliency of these names to be significantly higher in Hawaii, and also, that they would be higher for the names survey (see footnote above).

[59] See https://implicit.harvard.edu/implicit/Launch?study=/user/demo.us/demo.nativeamer.0002/nativeamdemo.expt.xml (accessed July 1, 2018). In the names survey, those with white names are seen as having been born in the United States 92.1% of the time in the national sample (96.0% of the time in the Arizona and New Mexico sample), relative to 64.8% for those with Navajo last names (72.3% in the Arizona and New Mexico sample.)

(2018) and Neumark (2018).[60] Our standard errors, in many cases, are also precise enough to rule out large amounts of discrimination in our main results, suggesting that even if there is differential treatment, it is uncommon.[61] Of course, our results are not precise enough to rule out discrimination in every circumstance. For example, we cannot rule out discrimination in small cities or town, such as Billings and Sioux Falls.

The "Heckman-Siegelman Critique" and the Neumark (2012) Correction

Audit and correspondence studies, especially resume-correspondence studies like ours, could face the "Heckman-Siegelman critique" (Heckman, 1998; Heckman and Siegelman, 1993). This critique holds that while these studies control for average differences in observable characteristics (information included in the job application), discrimination estimates can still be biased, in either direction, through differences in the variance of unobservable characteristics. Neumark (2012) shows how this can occur using a model of hiring decisions, and Neumark and Rich (2016) show that about half of the resume-correspondence studies they evaluated were biased because of this issue. We discuss this issue in more detail, including with a formal model, and test for this bias in Online Appendix C.

To summarize, we correct for this possible bias by randomly adding quality features[62] to the applications. As discussed in Neumark (2012) and Online Appendix C, these quality features

[60] The studies with more job applications than us are: Neumark, Burn, and Button, (forthcoming); Agan and Starr (2018); López Bóo, Rossi, and Urzúa (2013); Maurer-Fazio (2012); Maurer-Fazio and Lei (2015); and Zhou, Zhang, and Song (2013). Our records of the sample sizes (applications sent, unique jobs) for each study are available upon request.

[61] For example, in Table 6, our preferred estimate (column (2)) for Native American is a 0.4 percentage point decrease in the callback rate, with a standard error of 0.9 percentage points. The 95% confidence interval is -2.2 to 1.4 percentage points. So even this upper bound of discrimination, a 2.2 percentage point lower callback rate, is not particularly large relative to the baseline callback rate for white applicants (19.8%) and importantly is not statistically significant.

[62] Half of the application pairs are made to be "higher quality", and these higher-quality applications get four out of the five possible quality features: Spanish as a second language, a

shift the probability of a callback, allowing us to identify to what extent differences in the variance of unobservables between white and Indigenous applicants lead to bias in our original estimates. We find no evidence of bias in our main results due to the variance of unobservables issue. The estimated variances of unobservables are nearly equal for white and Indigenous applicants for the combined analysis (all occupations) and each occupation separately.[63] Thus, our lack of estimated discrimination is robust to this critique.

Do Callbacks Capture Hiring Discrimination?

Since resume-correspondence studies quantify hiring discrimination by comparing callbacks, there is the recurring question of whether callbacks truly measure hiring discrimination. Many others discuss this issue (e.g., Neumark, Burn, and Button, forthcoming; Booth, Leigh, and Varganova 2012). To summarize, callbacks are highly correlated with job offers. Discrimination, of course, could occur after the interview offer stage, but this is less likely. At the interview offer stage, is it far less likely that discrimination can be detected or enforced, relative to later when company personnel systems may have more detailed records of applicants (Neumark, Burn, and Button, forthcoming). At the callback stage, employers are also more likely to make quick decisions and fall victim to implicit bias (Bertrand et al. 2005; Rooth 2010). Audit studies that have actors and actresses go to interviews, and thus can observe job offers too, show that 75% (Bendick, Brown, and Wall 1999) to 90% of discrimination occurs at the callback stage.[64] Thus,

more detailed cover letter that summarizes employment experiences, a lack of typos in the cover letter, and two occupational-specific skills. See Online Appendix C for additional details.

[63] Our most significant difference in the variance of unobservables occurs for kitchen jobs, suggesting that whites have a slightly higher variance of unobservables. This suggests a negative bias in the estimate, rather than a positive bias. However, there is no statistically significant difference between these variables and applying the Neumark (2012) correction does not change the results in all our cases.

[64] See discussion of International Labor Organization (ILO) studies of ethnic discrimination in Riach and Rich (2002).

we do not believe that our study is failing to capture the extent of hiring discrimination because we are not able to observe job offers.

Population and Occupation Weighting

We attempted to apply for all eligible job openings that met our criteria in each city and occupation. Since our main estimates are unweighted, this means we oversampled populous cities. What would generate more population-representative results for Indigenous Peoples would be to weight the estimates by city so that they reflect the population distribution of Indigenous Peoples across these cities.[65] Similarly, we can weight by the frequency of occupations according to the CPS data in case our sample of jobs by occupation differs significantly from the national data. We can also weight by both. In Online Appendix D, we discuss how we construct these weights, and we present our main results, from Table 6, under different types of weighting (Indigenous population in the city, occupational popularity, and both) (see Online Appendix Table D9). Our results are unaffected by how we weight the data.

Choice of Occupations and Type of Jobs

We chose common occupations for those around age 30. These positions do skew more low-skilled or lower-experience relative to some other possible occupations, although this is a broader concern facing resume-correspondence studies in general (Neumark 2018; Baert 2018).

Were our chosen occupations ones that do not have discrimination? Numerous studies also used retail sales, server, and kitchen staff positions and found discrimination. Neumark, Burn, and Button (2016, forthcoming) also apply for janitor and security jobs and find some evidence of

[65] We are only aware of one other paper, Neumark et al. (2018), who also discuss the issue of weighting in audit studies.

discrimination, although these results are not robust to other considerations.[66] Therefore, we are not convinced, given this prior work using our same occupations, that our selected occupations just happened to be ones where discrimination does not occur in general.

Would discrimination be more common or less common in our chosen occupations relative to alternatives? For a few reasons, we argue that discrimination is more likely in our occupations and jobs relative to others. Research suggests that there is *more* discrimination in low-skilled positions (Helleseter, Kuhn, and Shen 2014; Kuhn and Shen 2013). Similarly, smaller firms, which are more likely to use the job board we used,[67] are less likely to have Human Resources departments and are less likely to be covered by Title VII of the Civil Rights Act, which applies to firms with at least 15 employees.

On the other hand, Sociology research suggests that individuals sometimes “type” jobs as being more suitable for individuals of certain races or genders (Kaufman 2002). While we found no research on this typing for Indigenous Peoples, we do not think that Indigenous Peoples are typed into retail sales or server positions. In these occupations, there is a significant amount of customer interaction such that customer discrimination may cause a preference for whites. Typing, however, may be relevant for kitchen staff, janitor, and security jobs. For kitchen staff, there is the potential notion that people of color are more likely to be “back of the house” (kitchen) than “front of the house” (servers, hosts, bartenders) staff, and this manifests in the CPS data.[68] However,

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[66] Some studies other than Neumark, Burn, and Button (2016, forthcoming) also used janitor and security positions, but these jobs were generally included in a larger pool of jobs that were analyzed, so it is hard to determine what the estimates were for these occupations specifically.

[67] Larger companies are more likely to have their own applications processes that do not allow them to be included in most resume-correspondence studies such as ours, even if they post on common job boards.

[68] Using the data from Tables 2 and 3, the ratio of waiters and waitresses to cooks for white men (women) is 0.57 (2.65). These ratios are much lower for AIAN men (0.15), AIAN women (0.72), NHPI men (0.03), and NHPI women (0.26).

discrimination does not appear to vary by occupation (Tables 7 and 8), suggesting that this concern did not affect our results. A related issue is that typing could vary by city, especially by the size of the Hispanic population.[69] In Online Appendix Table D10 we show estimates of the relative size of the Hispanic population in each occupation-city-gender combination. We used this information to re-estimate our main results (Table 6, Column (2)) excluding occupation-city-gender combinations where Hispanics outnumber whites. Our results are unchanged.[70]

Timing of the Study and Labor Market Tightness

Discrimination could occur more often when economic conditions are worse (Neumark and Button 2014; Johnston and Lordan 2016; Baert et al. 2015; Kroft, Notowidigdo, and Lange 2013). Therefore, resume-correspondence studies could generate larger (smaller) discrimination estimates during a downturn (a boom) in labor markets. We compare the timing of our study to all other resume-correspondence or employment audit studies conducted in the United States that were listed in the summary tables in either Baert (2018) or Neumark (2018)'s reviews of the literature. Online Appendix Table D11 presents the timing of data collection in each study and the national, seasonally-adjusted unemployment rates during that time.

This table shows that our study was during a time with lower unemployment rates (16th to 24th percentile of the seasonally-adjusted rate from 1948 to 2018).[71] This percentile range of our

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[69] We thank Randall Akee, and others, for raising this helpful point.
[70] These results are available upon request.
[71] We collected data from March 2017 to December 2017, where the seasonally-adjusted national unemployment rate ranged from 4.1 to 4.4 percent. Compared to the national, seasonally-adjusted, unemployment rate estimates from all months from January 1948 to October 2018, our unemployment rates fall into the 16th to the 24th percentile (the median unemployment rate is 5.6, the 10th percentile is 3.8, and the 90th percentile is 7.9). We calculated this using Bureau of Labor Statistics data from series LNS14000000 (accessed November 23, 2018, from https://data.bls.gov/timeseries/lns14000000). Our data and calculations are available upon request.

unemployment rates overlaps with the ranges of Pager (2003) (23rd to 56th percentile) and Kleykamp (2009) (21st to 35th), both which find statistically significant effects. The unemployment rates during our study were not as extreme as over a third of the other studies which occurred during the Great Recession, where unemployment rates reached record highs.[72] While better economic conditions at the time of our study could have affected our results, we do not see a clear case for this, and we do not think that this could have led to our result of no discrimination.[73]

Oaxaca-Blinder Decomposition of Earnings and Unemployment Gaps

Our field experiment shows no evidence of discrimination, suggesting that the significant disparities in economic outcomes between Indigenous Peoples and whites are more likely due to factors other than discrimination. To explore this further, we also conduct an Oaxaca-Blinder decomposition (Oaxaca and Ransom 1994), similar to Hurst (1997) and Feir (2013), using monthly IPUMS-CPS data from 2010 to 2017 (Flood et al., 2015). We used the "oaxaca" Stata command outlined in Sinning, Hahn, and Bauer (2008) to decompose gaps in earnings into an explained portion, explained by observable factors such as education, occupation, and geography, and into an unexplained (residual) portion, which could reflect unemployment discrimination. We expand on prior wage decomposition studies (e.g., Hurst 1997; Feir 2013; Baldwin and Choe 2014; Kruse et al. 2018) by also decomposing gaps in unemployment rates and unemployment durations, given

[72] Of the 21 studies, eight have a percentile range that includes at least the 90th percentile of unemployment rates, if not higher (Jacquemet and Yannelis 2012; Bailey, Wallace, and Wright 2013; Wright et al. 2013; Decker et al. 2015; Nunley et al. 2015; Gaddis 2015; Hipes et al. 2016; Farber, Silverman, and von Wachter 2017). We argue that many of these studies are just case studies of the Great Recession and may not tell us about discrimination in general.
[73] We do argue that more work needs to be done to determine how economic cycles affect discrimination, especially in light of many studies being case studies of the Great Recession, which may not reflect normal economic times.

that these are more directly related to the hiring discrimination we estimate in our field experiment.[74]

Our Oaxaca-Blinder decomposition is a useful complement to our experiment as it allows us to see whether discrimination might occur outside the context of our field experiment. It also allows us to determine which factors explain the disparities in economic outcomes that we see in the raw data, with the caveat that the Oaxaca-Blinder decomposition is not the preferred way to measure discrimination since it cannot control for all factors other than race, unlike an experiment.

We discuss the methodology for our Oaxaca-Blinder decomposition in-depth and present more detailed results in Online Appendix G, with results summarized in Tables 11 (AIAN) and 12 (NHPI). To summarize, we find that for those who identify as AIAN alone compared to non-Hispanic white alone,[75] most of the raw gap in hourly wages (a 15.6% gap) is explained by lower educational levels and lower-paying occupations, leading to a small unexplained gap (1.2%). For NHPIs, the raw gap in hourly wages is smaller (9.1%) and is explained by education and occupations but is offset by differences in the state of residence, whereby Native Hawaiians are more likely to live in Hawaii, where earnings are higher. In net, there is a slightly larger

[74] Discrimination in hiring directly leads to a lower arrival rate of job offers, with lower arrival rates being mechanically linked in job search theory models to both higher unemployment rates and longer unemployment rates, so long as reservation wages do not adjust completely to offset these effects, which is unlikely (Cahuc, Carcillo, and Zylberberg 2014). Exploring gaps in earnings, however, measures wage discrimination rather than hiring discrimination when occupation fixed effects are included. When these are not included, then the discrimination estimates ("unexplained") from an Oaxaca-Blinder decomposition do capture some hiring discrimination if hiring discrimination manifests as different eventual occupations, but then this analysis cannot control for occupational choices, outside of discrimination, that create differences.

[75] Results are similar using AIAN alone or in combination, and results using NHPI alone or in combination are also similar to the results for NHPI alone (see Online Appendix Tables G2, G3, and G4).

unexplained gap (4.2%). This suggests the potential for minimal amounts of wage discrimination against AIANs and the potential for some wage discrimination against NHPIs.

For unemployment rates, the raw gap of a 4.5 percentage point higher unemployment rate for AIAN alone is almost entirely unexplained (4.3 percentage points unexplained). For NHPI, the raw gap is smaller (1.7 percentage points) but is partially explained (0.7 percentage points left unexplained). However, for unemployment durations, the evidence differs for AIAN and NHPI individuals. Both AIAN and NHPI individuals have negative raw gaps, suggesting shorter unemployment durations (1.7 weeks shorter for AIAN alone, 2.9 weeks shorter for NHPI alone). After the decomposition, there is a positive unexplained portion for AIAN: unemployment durations that are 1.6 weeks longer for AIANs.[76] In contrast, the duration for NHPI is entirely unchanged and unexplained in the decomposition. These unemployment results for AIANs point consistently towards potential hiring discrimination while the results for NHPI are unclear.

There are two possible explanations for why our Oaxaca-Blinder results, namely for AIANs and for unemployment rates and duration, differ from the results of our field experiment. First, there is the standard criticism that unexplained gaps in Oaxaca-Blinder decompositions are not necessarily evidence of discrimination, but instead show an upper-bound to discrimination (hence *potential* discrimination). This is because it is not possible to use survey data to control for all differences to make Indigenous and non-Indigenous Peoples identical in all aspects other than race, as can be done in the field experiment (Neumark 2018; Bertrand and Duflo 2016). Thus, uncontrolled differences other than discrimination could explain these unexplained gaps. The most relevant uncontrolled difference would be differences in reservation wages.

[76] This flip from a raw gap in favor of AIANs to an unexplained disparity occurs primarily from controlling for differences in age and Hispanic ancestry.

Conversely, it is possible that hiring discrimination does exist and is picked up by this decomposition but it is missed entirely by the field experiment. As discussed, our field experiment is only a case study: discrimination among common occupations (retail sales, server, kitchen staff, janitor, and security) in 11 cities for applicants who have only a high school education and are of about age 30. While we argue that discrimination is more likely to occur in low-skilled occupations and for the small employers that are more likely to be included in our sample, we cannot entirely rule out that there could be discrimination in other occupations or contexts.

To better understand whether the results from our Oaxaca-Blinder decompositions reflect potential discrimination outside of our case study, we re-ran our decompositions where we restricted the sample to include only observations that better aligned with our experiment.[77] Our results, available upon request, are relatively unchanged in these restricted samples, suggesting that contexts outside our field experiment are not driving the potential discrimination we see in the Oaxaca-Blinder decompositions. We see it is far more likely that the unexplained higher unemployment rates (and durations for AIAN) reflect uncontrolled factors rather than hiring discrimination existing in general and missed by our experiment. However, a more thorough analysis, similar to Hurst (1997) and Feir (2013), would be helpful but is beyond the scope of this paper.

Conclusion

Our results from a large-scale field experiment of hiring discrimination where we sent 13,516 job applications of on-average identical applicants who were either Indigenous or white to

[77] For example, restricting to individuals in our age range who are high school graduates in the occupations and states that we tested.

jobs as retail salespersons, servers, kitchen staff, janitors, or security guards show a lack of hiring discrimination, in net, against Indigenous Peoples. We also do not find bias against Native American applicants from Indian reservations. We do not find discrimination even when we estimate separately by city, occupation, or occupation and gender.

Our results are robust in several ways, including to the inclusion or exclusion of controls, to how we signal Indigenous status (volunteer, language, name), to the Neumark (2012) correction for potential bias from the variance of unobservables, to how the regressions are weighted, to how callbacks are coded, and to how we cluster our standard errors. We discuss how our results are unlikely to be due to better economic conditions at the time of the experiment, the ways in which we signal Indigenous status, the saliency of our signals of Indigenous status, or a lack of statistical power. We also argue that our choice of occupations and the type of jobs or employers we tested was unlikely to have generated our result of no discrimination, but we again emphasize that we cannot rule out discrimination in all occupations or all contexts.

Our results suggest that the significant economic disparities faced by Indigenous Peoples have little to do with discrimination and more to do with other factors, such as differences in education. Directly addressing these inequalities could help alleviate these inequalities. Since we find little evidence of discrimination, it is less likely that supply-side investments in Indigenous peoples or communities (e.g., education and job training) will have their impacts frustrated by discriminatory employers. Determining which policies best help narrow economic disparities would be fruitful, especially given the shortage of economics research on Indigenous Peoples (Feir and Hancock 2016).

This study is one of the first, and few, to explore the extent to which Indigenous Peoples face discrimination. Future work can explore this in many ways. First, our case studies cannot rule out discrimination in all occupations or all cities, so future researchers could continue to investigate whether discrimination occurs in other circumstances that we were not able to study. Second, discrimination can occur more broadly, as shown in experimental audit studies of discrimination in health care (e.g. Sharma, Mitra, and Stano 2015), in housing (e.g., Hanson and Hawley 2011; Hanson et al. 2016), in access to local government services (e.g., Giulietti, Tonin, and Vlassopoulos 2017), and in political representation (e.g., Butler and Broockman 2011). While there are a few non-experimental studies that uncover disparities or suggest discrimination against Indigenous Peoples in these other contexts such as in policing (Gorsuch and Rho 2018), access to credit (Jorgensen and Akee 2017), in housing and institutionalization (Feir and Akee 2018), and in business and economic development (Akee and Jorgensen 2014), more research is needed to fully understand to what extent Indigenous Peoples face discrimination more broadly.

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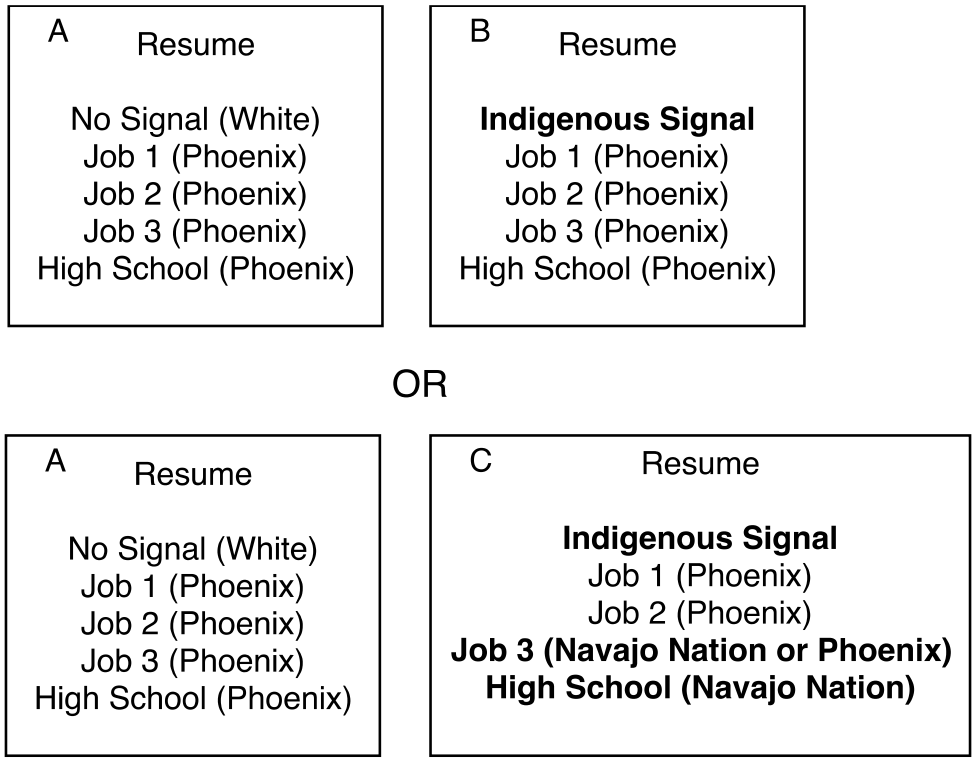
Figure 1 – Example of Pairs of Applicants for Jobs in Phoenix with Navajo Applicants



Notes: We always sent the A-B pair when the Indigenous applicant was Native Hawaiian or Alaska Native as type C is not possible for these groups. For pair with a Native American applicant, half of the jobs get the A-B pair, and the other half get the A-C pair. Half the A-C pairs have Job 3 for type C be a job on the Indian reservation while the other half have the equivalent job in the local city as in type A.

Table 1 – Summary of Possible Racial Signals by Indigenous Group

Indigenous Group	Possible Signals of Indigenous Status: Volunteer Experience	Language	First Name	Last Name	Indian Reservation Possible
Navajo	X	X (Navajo)		X	X (Navajo Nation)
Apache	X	X (Apache)			X (Fort Apache or San Carlos)
Blackfeet	X				X (Blackfeet)
Tohono O'odham	X	X (Pima)			X (Tohono O'odham)
Oglala Lakota	X	X (Lakota)			X (Pine Ridge)
Osage	X				X (Osage)
Alaska Native	X	X (Yup'ik)			
Native Hawaiian	X	X (Hawaiian)	X		

Notes: The language signal is not possible for Blackfeet or Osage because Indigenous language use for those tribes is not sufficiently common (see Online Appendix Table A1).

Table 2 – Frequency of our Selected Occupations for Men, by Race

Occupation (Rank)	Proportion of Entire Race			Ratio to White	
	White	AIAN	NHPI	AIAN	NHPI
Retail salespersons 41-2031 (#5)	2.18%	0.83%	0.46%	1.19%	0.20%
Grounds maintenance workers 37-3010 (#6)	2.06%	2.36%	2.11%	3.59%	0.97%
Cooks 35-2010 (#9)	1.65%	3.73%	2.51%	7.07%	1.44%
Janitors and building cleaners 31-201X (#10)	1.49%	1.68%	2.00%	3.55%	1.28%
Waiters and waitresses 35-3031 (#24)	0.94%	0.57%	0.08%	1.89%	0.08%
Cashiers 41-2010 (#31)	0.84%	1.26%	0.50%	4.69%	0.56%
Security Guards and Gaming Surveillance Officers (#37)	0.74%	1.44%	2.74%	6.14%	3.53%

Notes: Data come from all months of the 2015 Current Population Survey. Estimates are weighted using population weights. Occupations are ranked based on the decreasing share of white men that have this occupation out of all white men. White corresponds to those who report that they are white only, while AIAN (NHPI) correspond to those who report AIAN (NHPI) either alone or in combination with another race. Our sample includes those aged 25 to 35 only. See Online Appendix A and Online Appendix Table A3 for a larger table with other occupations.

Table 3 – Frequency of our Selected Occupations for Women, by Race

Occupation (Rank)	Proportion of Entire Race			Ratio to White	
	White	AIAN	NHPI	AIAN	NHPI
Cashiers 41-2010 (#4)	2.65%	3.30%	3.25%	5.03%	1.13%
Waiters and waitresses 35-3031 (#5)	2.65%	0.80%	0.47%	1.22%	0.16%
Retail salespersons 41-2031 (#8)	2.00%	1.94%	1.50%	3.91%	0.69%
Cooks 35-2010 (#27)	1.00%	1.11%	1.81%	4.49%	1.67%
Bartenders 35-3011 (#34)	0.81%	0.32%	0.86%	1.61%	0.98%
Janitors and building cleaners 31-201X (#38)	0.75%	0.40%	1.03%	2.17%	1.27%

Notes: See the notes to Table 2. Occupations are ranked based on the decreasing share of white women that have this occupation out of all white women. See Online Appendix A and Online Appendix Table A4 for a larger table with other occupations.

Table 4 – Applicant Types Sent by City

City	Applicant Types Sent
Albuquerque	White (A), Navajo (60%)/Apache (40%) (B or C, 50% probability each)
Anchorage	White (A), Alaska Native (B)
Billings	White (A), Blackfeet (B or C, 50% probability each)
Chicago	White (A), Navajo (25%)/Apache (15%)/Blackfeet (15%)/Osage (15%)/Tohono O'odham (15%)/Oglala Lakota (15%) (B or C, 50% probability each)
Honolulu	White (A), Native Hawaiian (B)
Houston	See *Chicago*
Los Angeles	White (A), Native Hawaiian (B) (25%) or White (A), Navajo (18.75%)/Apache (11.25%)/Blackfeet (11.25%)/Osage (11.25%)/Tohono O'odham (11.25%)/Oglala Lakota (11.25%) (B or C, 50% probability each)
New York	See *Chicago*
Oklahoma City	White (A), Osage (B or C, 50% probability each)
Phoenix	White (A), Navajo (40%)/Apache (20%)/Tohono O'odham (40%) (B or C, 50% probability each)
Sioux Falls	White (A), Oglala Lakota (B or C, 50% probability each)

Notes: Two applications, one Indigenous and one white, were sent in random order to each job ad. A, B, and C refer to the major resume types presented in Figure 1, where A is always a white applicant, B is always an Indigenous application who grew up in the urban center, and C is always a Native American applicant who grew up on an Indian reservation.

Table 5 – Mean Callback Differences by Indigenous Status

Callback:	No	Yes	Total
White	80.2%	19.8%	
	(5,421)	(1,337)	6,758
Indigenous	79.9%	20.1%	
	(5,397)	(1,361)	6,758
Native American	80.4%	19.6%	
	(4,187)	(1,018)	5,205
Native Hawaiian	78.7%	21.3%	
	(1,000)	(271)	1,271
Alaska Native	74.5%	25.5%	
	(210)	(72)	282
Total	80.0%	20.0%	
	(10,818)	(2,698)	13,516
Test of independence (p-value):	**White**	**N.A.**	**N.H.**
White	…	…	…
Native American	0.763	…	…
Native Hawaiian	0.165	0.132	…
Alaska Native	0.022	0.017	0.153

Notes: The p-values reported for the tests of independence are from Fisher's exact test (two-sided).

Table 6 – Callback Estimates by Race and Indian Reservation Upbringing

	No Controls (1)	Regular Controls (2)	Full Controls (3)
Native American	-0.011	-0.004	-0.005
	(0.010)	(0.009)	(0.009)
… x Reservation	0.000	-0.000	-0.000
	(0.015)	(0.012)	(0.012)
… x Reservation x Reservation Job	0.022	0.006	0.005
	(0.020)	(0.016)	(0.016)
Alaska Native	0.052**	0.005	0.003
	(0.026)	(0.035)	(0.035)
Native Hawaiian	0.012	-0.003	-0.002
	(0.013)	(0.013)	(0.013)
Non-Reservation Rural	-0.038**	-0.016	-0.015
	(0.016)	(0.013)	(0.013)
… x Rural Job	0.018	0.002	0.002
	(0.023)	(0.018)	(0.018)
Callback Rate for White:		19.8%	

Notes: N=13,516. Standard errors are computed based on clustering at the resume level. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). The regular controls are indicator variables for employment status, added quality features (Spanish, no typos in the cover letter, better cover letter, and two occupation-specific skills), occupation, gender, resume sending order, volunteer experience, and city. The full controls include the regular controls and graduation year, resume naming style, e-mail script version, e-mail format, e-mail subject, e-mail opening line, e-mail body, e-mail signature format, e-mail domain, voicemail greeting, oldest job (Job 3) start month, gap (in months) between Job 3 and Job 2, gap between Job 2 and 1, indicator variables for each company used on the resume, and duration of volunteer experience (in months).

Table 7 – Discrimination Estimates by Occupation

Indigenous	Estimate	Callback Rate for Whites	N
… x Retail	0.004	17.3%	2,926
	(0.013)		
… x Server	-0.001	16.4%	2,774
	(0.013)		
… x Kitchen	-0.006	22.2%	4,858
	(0.012)		
… x Janitor	-0.001	16.8%	1,652
	(0.016)		
… x Security	0.011	27.4%	1,306
	(0.022)		

Notes: N=13,516. See the notes to Table 6. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Regressions use the “Regular Controls” from Table 6 (Column (2)).

Table 8 – Discrimination Estimates by Occupation and Gender

	Indigenous	Female	Indigenous x Female	Callback Rate for White Men
… x Retail	0.006	0.037**	-0.003	16.3%
	(0.017)	(0.018)	(0.025)	
… x Server	-0.002	0.065***	0.002	13.3%
	(0.016)	(0.017)	(0.024)	
… x Kitchen	-0.007	0.000	0.001	21.5%
	(0.014)	(0.015)	(0.021)	
… x Janitor	0.003	-0.012	-0.008	17.7%
	(0.021)	(0.022)	(0.031)	
… x Security	0.011	N/A	N/A	27.4%
	(0.022)			

Notes: N=13,516. See the notes to Table 6. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Regressions use the "Regular Controls" from Table 6 (Column (2)). Note that we did not send female applicants to security jobs.

Table 9 – Discrimination Estimates by City

Indigenous	Estimate	N
… x Albuquerque	-0.037	700
	(0.029)	
… x Anchorage (AK Native)	0.005	564
	(0.035)	
… x Billings	0.012	212
	(0.062)	
… x Chicago	-0.009	1,466
	(0.018)	
… x Honolulu (Native HI)	0.002	2,034
	(0.016)	
… x Houston	-0.002	1,112
	(0.024)	
… x Los Angeles (Native Am.)	-0.001	1,866
	(0.014)	
… x Los Angeles (Native HI)	-0.014	440
	(0.019)	
… x New York	-0.011	2,758
	(0.011)	
… x Oklahoma City	0.018	616
	(0.033)	
… x Phoenix	0.041*	1,526
	(0.023)	
… x Sioux Falls	-0.004	154
	(0.078)	

Notes: N=13,516. See the notes to Table 6. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Regressions use the "Regular Controls" from Table 6 (Column (2)).

Table 10 – Discrimination Estimates by Indigenous Signal Type

Indigenous	Estimate	N
… x Volunteer Only	-0.006	3,029
	(0.010)	
… x Language Only	0.006	1,723
	(0.010)	
… x First Name (Native Hawaiian) Only	-0.017	475
	(0.018)	
… x Last Name (Navajo) Only	-0.007	222
	(0.026)	
… x Two Signals	0.003	823
	(0.015)	
… x Three Signals	0.038	92
	(0.037)	
Boys & Girls Club (Volunteer Control)	-0.007	3,298
	(0.009)	
Food Bank (Volunteer Control)	-0.006	3,460
	(0.009)	
Irish Gaelic (Language Control)	-0.017	831
	(0.013)	

Notes: N=13,516 for the entire sample, and N in the table is the number of resumes with that feature. See the notes to Table 6. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Regressions use the "Regular Controls" from Table 6 (Column (2)). The excluded volunteer control is Big Brothers Big Sisters without the racial signal.

Table 11 – Oaxaca-Blinder Decomposition Estimates (AIANs vs. Whites)

	Log Hourly Wage	Unemployment Rates	Unemployment Duration in Weeks
Total Difference	-0.145*** (0.006)	0.045*** (0.001)	-1.705*** (0.502)
Explained	-0.133*** (0.006)	0.003*** (0.000)	-3.313*** (0.263)
Occupation	-0.072*** (0.005)	0.013*** (0.000)	0.495*** (0.156)
Education	-0.053*** (0.002)	0.007*** (0.000)	1.330*** (0.081)
State	0.017*** (0.001)	0.001*** (0.000)	-1.086*** (0.081)
Hispanic	-0.014*** (0.001)	-0.019*** (0.000)	-2.466*** (0.120)
Age	-0.010*** (0.001)	-0.000*** (0.000)	-2.744*** (0.173)
Married	-0.006*** (0.000)	0.003*** (0.000)	0.503*** (0.080)
Gender	0.005*** (0.001)	-0.000** (0.000)	0.088** (0.041)
Metro Status	-0.003*** (0.000)	0.000*** (0.000)	-0.074*** (0.025)
Experience	0.003** (0.001)	-0.001*** (0.000)	1.226*** (0.114)
Survey Timing	0.001** (0.001)	-0.000*** (0.000)	-0.304*** (0.100)
Children	-0.000** (0.000)	0.000*** (0.000)	-0.282*** (0.035)
Unexplained	-0.012*** (0.003)	0.043*** (0.000)	1.609*** (0.410)
White Mean	$19.13	0.037	30.11
Observations	239,981	2,186,764	81,543

Notes: Data from IPUMS-CPS monthly data from 2010-2017 (Flood et al., 2005). Statistically significantly different from at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Robust standard errors are in parentheses. AIANs include only those who identify as AIAN alone. Results including AIAN in combination are similar and are presented in Online Appendix Tables G2, G3, and G4. Hourly wage is determined as either the hourly wage for those paid hourly and not top-coded, or the hourly wage is estimated by dividing weekly earnings by the usual hours worked. Estimates are weighted using population weights.

Table 12 – Oaxaca-Blinder Decomposition Estimates (NHPIs vs. Whites)

	Log Hourly Wage	Unemployment Rates	Unemployment Duration in Weeks
Total Difference	-0.087*** (0.012)	0.017*** (0.001)	-2.876** (1.383)
Explained	-0.046*** (0.011)	0.010*** (0.001)	0.010 (0.646)
Occupation	-0.053*** (0.007)	0.009*** (0.001)	0.068 (0.308)
Hispanic	-0.010* (0.006)	-0.005*** (0.000)	0.731* (0.396)
Education	-0.026*** (0.003)	0.004*** (0.000)	0.858*** (0.165)
Married	-0.002*** (0.001)	0.002*** (0.000)	-0.434*** (0.151)
State	0.049*** (0.003)	0.002*** (0.000)	0.694*** (0.138)
Experience	0.000 (0.003)	-0.001*** (0.000)	1.647*** (0.228)
Metro Status	0.008*** (0.001)	-0.000*** (0.000)	0.260*** (0.035)
Age	-0.018*** (0.004)	0.000*** (0.000)	-3.461*** (0.344)
Children	-0.000 (0.000)	0.000*** (0.000)	-0.295*** (0.058)
Survey Timing	0.003*** (0.001)	-0.000 (0.000)	0.151 (0.215)
Gender	0.005*** (0.002)	-0.000 (0.000)	-0.209** (0.082)
Unexplained	-0.041*** (0.012)	0.007*** (0.001)	-2.887** (1.219)
White Mean	$19.13	0.037	30.11
Observations	237,105	2,167,445	79,036

Notes: See notes to Table 11. Statistically significantly different from at 1-percent level (***), 5-percent level (**) or 10-percent level (*). NHPIs include those who identify as NHPI alone. Results including NHPI in combination are similar and are presented in Online Appendix Tables G2, G3, and G4.

Online Appendices for: “Employment Discrimination against Indigenous Peoples in the United States: Evidence from a Field Experiment”

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Contents:

Online Appendix A: Additional Details About the Experimental Design

Language as a Racial Signal

Here we provide additional details on how we determined which Indigenous languages were appropriate, in which circumstances, to signal Indigenous status. We used Indigenous languages to signal Indigenous status in some cases for most (but not all) of the tribal groups since Indigenous language use varies by tribal group. We used two approaches to determine which languages are spoken by which tribal groups. The first was to ascertain the languages historically spoken by the tribe. The second was to determine which Indigenous languages are spoken by individuals who live on the Indian reservation associated with the tribe.

While not all individuals from a tribe live or have lived on a reservation, this was the only data-driven approach for us to investigate language use by the tribal group. Online Appendix Table A1 presents the languages that we selected for each American Indian tribal group and the proportion of individuals who report speaking this language at home and live on the associated reservations, using Census data. We did not use language to signal Indigenous status for individuals from the Osage or Blackfeet tribes since Indigenous language use by these tribes is very low (less than 1% for Osage) or sufficiently uncommon (less than 10% for Blackfeet).

First Names as a Racial Signal

Using first names is a natural way to signal minority status in audit-correspondence studies. This approach is evident and easy for gender (for names that are gender-specific and well-known), but signaling race by name is more complicated. For race, names are used to signal African-American status (e.g., Bertrand and Mullainathan, 2004), Arab, Muslim, or Middle Eastern descent (e.g., Rooth, 2010), Turkish or Moroccan descent (e.g., Baert and De Pauw, 2014), and Asian, Roma, Ashkenazi Jewish, African, Indian, and Pakistani descent, among others (Booth, Leigh, and

Varganova, 2012; Fershtman and Gneezy, 2001; McGinnity and Lunn, 2011; Oreopoulos, 2011), and caste (e.g., Siddique, 2011). Using names as a signal improves external validity since names are required. However, first names can signal socioeconomic status in some cases, which some argue (Fryer and Levitt 2004) is the case in studies such as Bertrand and Mullainathan (2004).

We settled on three male names: Kekoa, Ikaika, and Keoni, and one female name: Maile. Malia also appeared on the top 100 list of names for girls, but we avoided using this name in case it sent a different signal given that this is the name of President Obama's daughter. We also did not use Alana since it is also a name of Irish origin. We opted not to use Leilani as there was some evidence that this name is common for those who are not Native Hawaiian.

Last Names as a Racial Signal

For those who identify as AIAN only, AIAN-specific last names are not common, but they are also not unusual. From our Census data, there are 268 last names where at least 80% of those with that name identified as AIAN only. Further, 5.5% of individuals who identified as AIAN only have one of these 268 last names.[1] A broader list of names where at least 30% of those with the name identified as AIAN only has 660 names, and 11.0% of those who identified as AIAN only have one of these 660 names.

To determine feasible last names, we first extracted a list of 268 last names that met the criteria where at least 80% of the people with those last names identified as AIAN alone. We then narrowed this list to 12 AIAN-specific last names that had at least 0.2 people per 100,000 with that

[1] We calculated this by taking the number of people with that name per 100,000 people and multiplying it by the share that identified as AIAN only to create an estimate of the number of people per 100,000 with that last name that identified as AIAN. Using the 80% criteria for AIAN-specific names, 3,326 people per 100,000 identified as AIAN only and have an AIAN-specific last name, compared to 56,790 people per 100,000 who identified as AIAN only and do not have an AIAN-specific last name.

last name. Finally, we selected four last names from this list where we could identify the tribal group (Navajo): Begay (5.96 people per 100,000, 94.98% identified as AIAN alone), Yazzie (5.16, 96.10%), Benally (1.87, 95.99%), and Tsosie (1.80, 96.23%).[2]

There are costs and benefits to this last name signal. Last names have the benefit of being a natural signal, since one cannot realistically put a different last name on the resume, but one could refuse to disclose relevant experience or skills that signal Indigenous status (e.g., the volunteer or language signals, discussed earlier) or applicants may re-phrase the experience in attempts to obscure racial signals. However, it may be less likely that employers understand that these are Native American last names relative to, say, understanding African-American first names, making the last name signal weaker. We investigate this in the robustness section and our resume survey (Online Appendix E) and name survey (Online Appendix F).

Another issue with using last names as a signal of race is that they are a weaker signal for women since they may take the last name from her spouse. This is especially an issue given the increase in interracial marriages after the 1970s (Fryer 2007). Thus, if discrimination against Native American women occurs less than for men, using the last name as the only signal, then this suggests that discrimination is weaker for women, that this is a weaker signal of race for women, or both. In contrast, using Native Hawaiian first names as the only signal may present a different set of implications. A Native Hawaiian first name and a non-Native Hawaiian last name (although Native Hawaiian last names appear uncommon) may imply that the applicant is multi-racial or it

[2] Our primary sources were Ancestry.com (e.g., http://www.ancestry.com/name-origin?surname=begay (accessed October 30, 2016)) and http://tribalemployee.blogspot.com/2013/03/navajo-last-names.html (accessed June 25, 2016). While these sources identified other names on our list of 12 as being Navajo, we could not sufficiently corroborate this with other sources. We also found many other sources through a web search that confirmed that Begay, Yazzie, Benally, and Tsosie were Navajo.

may separately or additionally imply interracial marriage for female applicants. However, we do not find discrimination regardless of gender or the signal used.

Phone Numbers and Email Addresses

We purchased phone numbers for our applicants from the companies *Vumber* and *GoTo Phone*. These appear the same as regular phone numbers but have the benefit that they do not require physical phones and store all the voicemails into a central account. We gave each phone number a typical and generic voicemail greeting that instructs the caller to leave a detailed message after the tone. When employers called, they did not always leave a message that provided enough information to match them to an exact applicant (let alone the job ad). Assigning a unique phone number to every job application would solve this problem but was not feasible. We purchased enough phone numbers to assign unique numbers to bins of job applicants defined by city, race (white or Indigenous), and occupation (retail sales, server, kitchen staff, janitor, and security, with janitor and security pooled into one set of numbers). This resulted in 88 unique phone numbers. With all of these numbers and other matching methods (discussed below), it was highly unlikely that we could not assign a response to an applicant.

We bought domains to create a large number of email addresses such that each applicant almost always had a unique email address, which allowed us to match, almost perfectly, the email responses to job applications.

Working with Research Assistants on Data Collection

We continually worked with the research assistants to standardize their job search methods so that each research assistant conducted their search the same way in each city and occupation and applied the same criteria to identify appropriate jobs. In addition to providing an instruction sheet (available upon request) and updating it when we learned about additional confusing cases, we supervised the research assistants in a few ways. These included direct supervision of research assistants (e.g., working nearby them and checking their work in person occasionally), an online forum where research assistants could post questions and receive quick answers, and regular meetings of the entire research team to discuss procedures and clarify ambiguities.

To check that our research assistants followed the guidelines, we required for one week early on that all research assistants saved every job ad that they opened, instead of just saving the job ads that they deemed eligible to apply to. For each ad, research assistants either saved it as a rejected ad or an eligible ad and for rejected ads they indicated why they rejected them. This allowed us to spot-check their work and make suggestions for improvement.

Sending Out Applications

Once research assistants determined that a job was eligible to apply to, they entered information about the job into a spreadsheet. They entered the job ID number (unique to each job posting), day and city for the job posting, occupation, email address for the application, subject line to be used (e.g., whether the employer requested a particular subject line; otherwise we randomized subject lines that were realistic), and whether the employer requested a resume in Microsoft Word format rather than PDF (by default we sent resumes as PDF documents). We then used Python and SQL code created by Nanneh Chehras for Neumark, Burn, Button, and Chehras (2018) to email these job applications automatically with a delay of a few hours between emails to the same employer. We ran the code at least twice per week, usually on set days (e.g., Monday and Thursday); though, we often ran it daily to minimize the time between finding the job and applying to it.

Each day was randomly assigned a different pair of resumes in terms of skill levels, employed or unemployed, and the gender of the applicants, as these factors are set to be the same within resume pairs. Within each pair, we randomized the application ordering of the two resumes. To distinguish further resumes in a pair further, we randomly name the computer files slightly differently. One resume in the pair was named "FirstLastResume," where First and Last were the applicant's first and last names, and the other resume was named "ResumeFirstLast."

Matching Responses to Jobs and Applications

Responses to job applications could be received by email or by phone. All email responses forwarded to a central email account, and all voicemails forwarded to that same account as email attachments. A research assistant then read each email and listened to each voicemail to code the response. We anticipated that the email or voicemails received would not always be enough to match the response to a specific job ad. However, we designed the email addresses and chose phone numbers in a way to improve our ability to match responses to specific applications and job ads.

Matching responses to specific applications and job advertisements was easier if the response from the employer was through email. If the employer replied directly to the original application email (sent to the employer through an email relay system), then the email response contained the unique ID number for the job ad. Each job ID number provides a one-to-one match to a job ad. However, if employers responded directly to the individual (by typing in the email address rather than hitting reply), then we did not observe this job ID. In this case, we used other information from the email, such as the company name or type, job ad title, and location. While our email addresses were not perfectly unique,[3] we also looked through records of which

[3] A few email addresses were randomly repeated based on the randomization process to generates names and email address. So, there may be more than one unique applicant with the same or similar name that uses the same email address, but this only occurs a few times. Also,

applications used which email addresses, and for which job ads, to narrow down the likely matches.

Voicemail responses conveyed less information which made matching more difficult, but usually possible. Based on how we assigned phone numbers, we always knew the city and Indigenous status of the applicant, and we almost always knew the occupation (only janitor and security jobs got the same phone numbers). We then used information in the voicemail message itself to try to match to an exact applicant or job advertisement. We assigned first and last names such that the combination of phone number and first or last name gave us the unique job applicant (except in a few cases for janitor or security). This improved our matching since employers almost always mentioned the first or last name of the applicant they called.

However, since we assign each pair of applicants to a particular day of the month, these applicants may apply to multiple jobs. Given this, additional information was required to make a match to a specific job advertisement. The additional information that helped us make a match was often the phone number of the employer and in the content of their voicemail message (e.g., they mention their employer by name).

When we could not match to a job ad, we matched to the next most specific level, which was the applicant.[4] This still allows us to run all of our regressions, including those with resume control variables. The only restriction, which is irrelevant in our case, is that these observations would need to be dropped if we wanted to use any information from the job ads.

since we assign each day to be a different pair of applicants, an applicant with a particular email may apply to multiple jobs in one day.

[4] For only a handful of voicemail responses, we did not have enough information even to match it to the applicant.

Online Appendix Table A1 – Non-English Languages and Indian Reservations

Indian Reservation	Tribal Group	Population	% Who Speak an "Other" Language	Language Assigned
Blackfeet Indian Reservation and Off-Reservation Trust Land, MT	Blackfeet	10,037	8.1	None
Fort Apache Reservation, AZ	Apache	13,179	54.4	Apache
Navajo Nation Reservation and Off-Reservation Trust Land, AZ-NM-UT	Navajo	161,009	67.2	Navajo
Osage Reservation, OK	Osage	45,257	0.7	None
Pine Ridge Reservation, SD-NE	Oglala Lakota	17,165	22.8	Lakota
San Carlos Reservation, AZ	Apache	9,145	33.9	Apache
Tohono O'odham Nation Reservation and Off-Reservation Trust Land, AZ	Tohono O'odham	9,154	33.7	Pima

Notes: Our data source is the U.S. Census Bureau (2014). "Other" language is a language other than English, Spanish, or an Indo-European or an Asian or Pacific Island language. The "Language Assigned" column corresponds to the language column in Table 1.

Online Appendix Table A2 - Rural City and Reservation Matches for the Rural Control for Indian Reservation Upbringing

Matching Urban City	Matching Reservation	Driving Distance	Control Rural Town	Driving Distance
Albuquerque	Navajo	3 h 26 m	Holbrook, AZ	3 h 19 m
Albuquerque	Fort Apache	4 h 23 m	Eagar, AZ	3 h 12 m
Albuquerque	San Carlos	6 h 18 m	Willcox, AZ	5 h 14 m
Billings	Blackfeet	5 h 32 m	Polson, MT	5 h 55 m
Oklahoma City	Osage	2 h 11 m	Newkirk, OK	1 h 49 m
Phoenix	Navajo	5 h 27 m	Fredonia, AZ	5 h 17 m
Phoenix	Fort Apache	2 h 59 m	Taylor, AZ	2 h 56 m
Phoenix	San Carlos	2 h 30 m	San Manuel, AZ	2 h 2 m
Phoenix	Tohono O'odham	2 h 13 m	Ajo, AZ	1 h 48 m
Sioux Falls	Pine Ridge	5 h 8 m	Wall, SD	4 h 1 m

Notes: We determined the distances between the city and the Indian reservation and the rural town using Google Maps.

Online Appendix Table A3 – Demographics of Occupations for Men Aged 25-35

Occupation	Proportion of Entire Race			Ratio to White	
	White	AIAN	NHPI	AIAN	NHPI
Driver/sales workers and truck drivers 53-3030	3.04%	3.07%	4.41%	3.17%	1.38%
Construction laborers 47-2061	2.80%	2.04%	3.74%	2.29%	1.27%
Managers, all other (11-9199)	2.55%	1.22%	2.62%	1.50%	0.98%
First-line sups./managers of retail sales workers 41-1011	2.36%	1.92%	1.81%	2.54%	0.73%
Retail salespersons 41-2031	2.18%	0.83%	0.46%	1.19%	0.20%
Grounds maintenance workers 37-3010	2.06%	2.36%	2.11%	3.59%	0.97%
Carpenters 47-2031	1.97%	1.90%	1.75%	3.02%	0.84%
Laborers & freight, stock, and material movers, hand 53-7062	1.90%	3.02%	3.65%	4.99%	1.83%
Cooks 35-2010	1.65%	3.73%	2.51%	7.07%	1.44%
Janitors and building cleaners 31-201X	1.49%	1.68%	2.00%	3.55%	1.28%
Automotive service technicians and mechanics 49-3023	1.34%	1.22%	2.74%	2.85%	1.94%
Software developers, apps. and systems software 15-113X	1.23%	1.01%	0.00%	2.57%	0.00%
Sales representatives, wholesale and manufacturing 41-4010	1.21%	0.55%	0.30%	1.41%	0.24%
Electricians 47-2111	1.19%	1.14%	0.94%	3.00%	0.75%
Miscellaneous agricultural workers 45-2090	1.18%	0.65%	0.14%	1.72%	0.11%
Stock clerks and order fillers 43-5081	1.14%	1.09%	0.68%	2.98%	0.57%
Customer service representatives 43-4051	1.09%	1.39%	1.20%	3.98%	1.05%
Accountants and auditors 13-2011	1.08%	0.01%	0.69%	0.03%	0.61%
Welding, soldering, and brazing workers 51-4120	1.05%	1.64%	0.96%	4.90%	0.87%
Police and sheriff's patrol officers 33-3051	1.03%	0.96%	0.52%	2.95%	0.48%
Production workers, all other 51-9199	0.98%	1.93%	0.44%	6.18%	0.43%
Elementary and middle school teachers 25-2020	0.95%	0.46%	0.60%	1.53%	0.59%
Pipelayers, plumbers, pipefitters, and steamfitters 47-2150	0.95%	0.74%	0.23%	2.43%	0.23%
Waiters and waitresses 35-3031	0.94%	0.57%	0.08%	1.89%	0.08%
Food service managers (11-9051)	0.88%	0.29%	1.01%	1.02%	1.09%
Painters, construction and maintenance 47-2141	0.87%	0.54%	0.38%	1.94%	0.41%
General and operations managers (11-1021)	0.86%	0.47%	1.51%	1.71%	1.66%
Lawyers, Judges, magistrates, and other jud. workers 23-1011	0.86%	0.38%	0.00%	1.38%	0.00%
Miscellaneous assemblers and fabricators 51-2090	0.86%	1.43%	1.98%	5.24%	2.20%
Construction managers (11-9021)	0.84%	0.16%	0.00%	0.59%	0.00%
Cashiers 41-2010	0.84%	1.26%	0.50%	4.69%	0.56%
First-line sups./managers of non-retail sales workers 41-1012	0.81%	0.05%	1.93%	0.20%	2.26%
Postsecondary teachers 25-1000	0.77%	0.13%	1.29%	0.52%	1.58%
Marketing and sales managers (11-2020)	0.77%	0.00%	0.14%	0.00%	0.17%
First-line sups./managers of prods. and oper. workers 51-1011	0.77%	0.33%	0.53%	1.33%	0.66%
… of construction trades and extraction workers 47-1011	0.76%	1.43%	0.27%	5.93%	0.34%
Security Guards and Gaming Surveillance Officers	0.74%	1.44%	2.74%	6.14%	3.53%
Heating, A/C, and fridge mechanics and installers 49-9021	0.72%	0.43%	0.25%	1.87%	0.33%

Notes: This data comes from all months of the 2015 Current Population Survey. We weight these estimates using population weights. We sort occupations by the decreasing share of white men that have this occupation out of all white men.

Online Appendix Table A4 – Demographics of Occupations for Women Aged 25-35

Occupation	Proportion of Entire Race			Ratio to White	
	White	AIAN	NHPI	AIAN	NHPI
Elementary and middle school teachers 25-2020	4.61%	1.27%	2.19%	1.12%	0.44%
Registered nurses 29-1141	4.27%	1.66%	4.11%	1.57%	0.89%
Secretaries and administrative assistants 43-6010	3.23%	1.45%	4.36%	1.81%	1.24%
Cashiers 41-2010	2.65%	3.30%	3.25%	5.03%	1.13%
Waiters and waitresses 35-3031	2.65%	0.80%	0.47%	1.22%	0.16%
First-line supervisors/managers of retail sales workers 41-1011	2.21%	1.60%	3.44%	2.92%	1.44%
Customer service representatives 43-4051	2.16%	2.01%	2.43%	3.76%	1.04%
Retail salespersons 41-2031	2.00%	1.94%	1.50%	3.91%	0.69%
Nursing, psychiatric, and home health aides 31-1010	1.87%	2.94%	4.34%	6.36%	2.14%
Managers, all other (11-9199)	1.87%	0.82%	1.77%	1.77%	0.87%
Child care workers 39-9011	1.65%	1.79%	1.01%	4.37%	0.56%
Receptionists and information clerks 43-4171	1.59%	1.34%	4.29%	3.40%	2.49%
Maids and housekeeping cleaners 37-2012	1.47%	2.41%	2.88%	6.65%	1.81%
Accountants and auditors 13-2011	1.43%	0.49%	2.03%	1.38%	1.31%
Office clerks, general 43-9061	1.38%	1.39%	3.06%	4.07%	2.04%
Preschool and kindergarten teachers 25-2010	1.32%	0.60%	0.43%	1.85%	0.30%
Hairdressers, hairstylists, and cosmetologists 39-5012	1.27%	0.79%	0.27%	2.52%	0.20%
Secondary school teachers 25-2030	1.24%	0.39%	1.08%	1.29%	0.80%
First-line sups./mngrs. of office and admin. support 43-1011	1.21%	0.83%	2.99%	2.77%	2.29%
Health diag. and treating practitioner support techs. 29-2050	1.17%	0.63%	0.00%	2.18%	0.00%
Counselors 21-1010	1.09%	0.48%	0.23%	1.77%	0.20%
Medical assistants 31-9092	1.07%	0.89%	1.07%	3.35%	0.92%
Designers 27-1020	1.04%	0.15%	0.63%	0.60%	0.56%
Personal and home care aides 39-9021	1.03%	2.01%	3.98%	7.86%	3.56%
Food service managers (11-9051)	1.02%	1.10%	1.82%	4.36%	1.65%
Social workers 21-1020	1.02%	0.71%	0.00%	2.84%	0.00%
Cooks 35-2010	1.00%	1.11%	1.81%	4.49%	1.67%
Bookkeeping, accounting, and auditing clerks 43-3031	1.00%	0.66%	0.08%	2.66%	0.07%
Postsecondary teachers 25-1000	0.97%	0.12%	0.53%	0.52%	0.50%
Marketing and sales managers (11-2020)	0.93%	0.03%	0.00%	0.12%	0.00%
Human resource workers 13-1070	0.91%	0.10%	1.39%	0.45%	1.41%
Teacher assistants 25-9041	0.90%	0.99%	1.65%	4.42%	1.69%
Financial managers (11-3031)	0.87%	0.74%	0.19%	3.44%	0.20%
Bartenders 35-3011	0.81%	0.32%	0.86%	1.61%	0.98%
Other teachers and instructors 25-3000	0.80%	0.05%	1.26%	0.24%	1.46%
Lawyers, Judges, magistrates, and other jud. workers 23-1011	0.78%	0.06%	0.00%	0.32%	0.00%
Licensed practical and licensed vocational nurses 29-2061	0.76%	0.54%	0.20%	2.90%	0.24%
Janitors and building cleaners 31-201X	0.75%	0.40%	1.03%	2.17%	1.27%

Notes: See the notes to Online Appendix Table A3. We sort occupations by the decreasing share of white women that have this occupation out of all white women.

Online Appendix B: Pre-Analysis Plan

Before putting this experiment into the field, we filed a pre-analysis plan (PEP) and registered it with the American Economic Association's Randomized Control Trial Registry (socialscienceregistry.org).[5] Our goal was to pre-specify any variables, models, sample sizes, or decisions that could easily be data mined.

In this experiment, there is only one outcome – callbacks – so there is little to no risk of a typical data mining issue where a researcher can select a subset of outcome variables that show statistically significant results (Olken 2015). We did, however, pre-specify a few things. First, we specified how we could code callbacks by including ambiguous responses with callbacks (e.g., "We reviewed your application, and we have some questions for you."), as done in previous work (e.g., Neumark, Burn, and Button, forthcoming.) We also chose to pre-specify some control variables and models to avoid less pivotal possibilities of data mining, such as choosing resume control variables or models specifically to affect the results. This sort of decision of which control variables or model to use, and how that could lead to p-hacking or data mining, is not unique to our study by any means. While it is not common to pre-specify these, it has been done before with some benefit (e.g., Neumark, 2001) and we wanted to be upfront about decisions that we knew made the most sense to take beforehand. In this pre-analysis plan we sought to commit to approaches to prevent possibilities of data mining or p-hacking whenever we could while also not tying our hands too much in ways that would negatively affect our ability to conduct this research later (see Olken, 2015, p. 71 and Lahey and Beasley, 2018, for some useful discussion of the costs of pre-analysis plans.) In retrospect, we believe that we struck a good balance, but we did pre-specify a few things that we really should not have (e.g., probit models instead of linear probability

[5] See https://www.socialscienceregistry.org/trials/2299 (accessed January 20, 2019).

models), but this did force us to be transparent about our deviations from our pre-analysis plan and justify those deviations.

In this pre-analysis plan, we pre-specified the way callbacks would be coded, the primary models and tabulations, and the main control variables. We also committed to using a particular sample size, in addition to using all our data, for our main results to mitigate concerns of data mining if our sample size exceeded the minimum sample size required based on the power analysis. As shown in Online Appendix Table B1, our main results are virtually identical using the smaller sample size of 8,422, suggested by our power analysis.

We primarily adhered to the core of the pre-analysis plan but made a few minor deviations. The first minor deviation is in our full controls (see Table 6, column (3)), in which we planned to include indicator variables for each company used on the resume in our vector of full controls.[6] Including these company indicator variables ended up making interpretation of the coefficients on $Reservation\,Job$ and $Rural\,Job$ impossible since some companies are assigned based on if the applicant had an upbringing and job on an Indian Reservation or in a small rural town. For this reason, we do not include these company indicator variables in the full controls regression in Table 6, column (3). However, our estimates outside of those for $Reservation\,Job$ and $Rural\,Job$ do not change when we add company indicator variables (online Appendix Table B2.)

[6] For reference, the regular controls, which are the default for all tables, are indicator variables for employment status, added resumes quality features (Spanish, no typos in the cover letter, better cover letter, and two occupation-specific skills), occupation, gender, resume sending order, volunteer experience, and city. The full controls include the regular controls and graduation year, resume naming style, e-mail script version, e-mail format, e-mail subject, e-mail opening line, e-mail body, e-mail signature format, e-mail domain, voicemail greeting, oldest job (Job 3) start month, gap (in months) between Job 3 and Job 2, gap between Job 2 and 1, and the duration of volunteer experience (in months).

The second minor deviation is in the statistical model that we used to run regressions. We originally specified using a probit, but we later learned that it is problematic to interpret interactions in a probit model (Ai and Norton 2003). For this reason, we switched to presenting the main results from a linear probability model. However, our results are similar using a probit (see Appendix Tables D6 through D9).

The third minor deviation is in weighting our results. In our pre-analysis plan, we considered our population-weighted estimates to be the preferred specification. Since we now realize that there is more than one way to weight the estimates, we instead include the unweighted estimates in the main paper for ease of presentation. However, we present main estimates with and without all types of weighting in Online Appendix D. Our results never differ in a meaningful way regardless of how we weight, if at all.

Online Appendix Table B1 – Callback Estimates by Race and Indian Reservation Upbringing – Results by Sample Size

	(1)	(2)
Native American	-0.003	-0.004
	(0.010)	(0.009)
… x Reservation	-0.007	-0.000
	(0.013)	(0.012)
… x Reservation x Reservation Job	-0.009	0.006
	(0.018)	(0.016)
Alaska Native	-0.008	0.005
	(0.046)	(0.035)
Native Hawaiian	-0.009	-0.003
	(0.018)	(0.013)
Non-Reservation Rural	-0.025*	-0.016
	(0.013)	(0.013)
… x Rural Job	0.006	0.002
	(0.018)	(0.018)
	N=8,422	N=13,516

Notes: See the notes to Table 6. All regressions use the regular controls (Column (2) of Table 6). Column (1) uses the first 8,422 observations, per our power analysis calculation. Column (2) uses all observations. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*).

Online Appendix Table B2 – Callback Estimates by Race and Indian Reservation Upbringing – Full Controls vs. Full Controls plus Company Indicators

	(1)	(2)
Native American	-0.005	0.005
	(0.009)	(0.010)
… x Reservation	-0.000	-0.004
	(0.012)	(0.013)
… x Reservation x Reservation Job	0.005	N/A
	(0.016)	
Alaska Native	0.003	0.013
	(0.035)	(0.034)
Native Hawaiian	-0.002	-0.005
	(0.013)	(0.016)
Non-Reservation Rural	-0.015	0.001
	(0.013)	(0.014)
… x Rural Job	0.002	N/A
	(0.018)	

Notes: N=13,516. See the notes to Table 6. Both regressions include the full controls (Column (2) of Table 6). Column (2) includes the added company indicator variables, which removes the separate effects of reservation job and rural job since it controls for each possibly company that could be listed for those. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*).

Online Appendix C: The "Heckman-Siegelman" Critique and the Neumark (2012) Correction

Introduction and Theoretical Model

Audit-Correspondence (AC) studies suffer from the "Heckman-Siegelman critique" (Heckman, 1998; Heckman and Siegelman, 1993). The critique is that while AC studies control for average differences in observable characteristics (what is included in the application), discrimination estimates can still be biased through the variance of unobservable characteristics (what is not seen on the resume). Neumark (2012) shows how this can occur using a model of hiring decisions, which we summarize very briefly here following the notation of Neumark, Burn, and Button (2016).

Assume that productivity depends linearly and additively on two characteristics: observable (on the resume) characteristics, which are denoted X^I and unobservable characteristics (not on the resume), which are denoted as X^{II}. Let N denote Indigenous ("Native") applicants and let W denote white applicants. AC studies standardize X^I to be the same for N and W at some level X^{I*}, such that $X^I_N = X^I_W = X^{I*}$. Let γ be an additional linear, additive, term that reflects discrimination against Indigenous Peoples. This term can either reflect taste discrimination, where the productivity of Indigenous Peoples is undervalued, or statistical discrimination, where firms believe that the average unobservable characteristics are different between groups (i.e., that $E(X^{II}_N) \neq E(X^{II}_W)$). AC studies seek to estimate γ as a linear function of X^I and an indicator for race (N).

Applicants are given an interview (T = 1) if expected productivity exceeds a threshold, c:

$$
\begin{aligned}
T(X^{I*}, X^{II}_N) \mid (N = 1) &= 1 \; if \; \beta_1 X^{I*} + X^{II}_N + \gamma N > c \\
T(X^{I*}, X^{II}_W) \mid (N = 0) &= 1 \; if \; \beta_1 X^{I*} + X^{II}_W > c
\end{aligned} \qquad [C1]
$$

If X^{II}_N and X^{II}_W are normally distributed with means of zero and standard deviations of σ^{II}_N and σ^{II}_W, respectively, then the interview offer probability is:

$$\Phi[(\beta_1 X^{I*} + \gamma N - c)/\sigma_N^{II}]\ if\ N = 1$$
$$\Phi[(\beta_1 X^{I*} - c)/\sigma_W^{II}]\ if\ N = 0. \qquad [C2]$$

The Heckman critique arises because it is not possible to identify γ unless the ratio between σ^{II}_N and σ^{II}_W is known. To illustrate why this is the case, suppose that Indigenous people have a larger variance of unobservables (i.e., $\sigma^{II}_N > \sigma^{II}_W$). This is likely the case as evidence suggests that other racial minorities also have a larger variance of unobservables (e.g., Neumark, 2012). For firms that require very productive workers (c is high), and the standardized observables on the resumes are of somewhat low quality, then the larger variance for Indigenous applicants means that they are more likely to pass this high standard than white applicants. This negatively biases the estimate of γ. This bias becomes more positive when the interview standard is lower, or the observables are standardized at a higher level. Regardless, the estimate of γ is a function of the ratio of σ^{II}_N to σ^{II}_W, and to the level of standardization of the observables (X^{I*}).

Neumark (2012) develops a method to address this by using different quality standardizations that are introduced when quality features are added to the applicants. This allows γ to be identified under the assumption that β_1 is equal for Indigenous and white applicants. Neumark (2012) also shows that if there are multiple added quality features then there is an over-identification test that can be used to test this assumption.

Quality Features

Any resume or applicant feature that shifts the quality of the resume in the eyes of the employer can be used in the Neumark (2012) correction. Of course, one can randomly add quality features using resume randomization tools (Lahey and Beasley, 2018, 2009) and then let the data "speak" about what features, according to the employer, boost quality (Lahey and Beasley, 2018).

However, we feel that it is essential to incorporate some quality features beforehand that are believed to affect callback rates, with the goal to ensure that there is enough variation in applicant quality in order for this correction to be sufficiently powered. This is crucial since the Neumark (2012) correction requires significantly more power than the standard uncorrected analysis.

In this experiment, we made half of the applicants high-quality and half of them low-quality by assigning four of five quality elements to the high-quality applicants. So as not to take identifying variation away from detecting the effects of Indigenous status, we assign either all resumes within a set sent to an employer to be high or low quality, but the four randomly chosen quality elements can vary between resumes sent to the same employer. Like Neumark, Burn, and Button (forthcoming), we chose which quality elements to include based on what is commonly listed on actual resumes or in job applications. These five quality elements are fluency in Spanish as a second language, a more detailed cover letter (e.g., an additional two or so sentences on their cover letter that briefly summarizes their work experience), the lack of typos in the cover letter (that is, resumes without this quality feature have either a missing comma after the opening line, a missing period at the end of the first sentence, or a misspelled word somewhere on the cover letter), and two occupation-specific skills. All high-quality resumes randomly receive all but one of these skills. This allows for some variation to identify the effects of each quality feature separately.

For retail jobs, the occupation-specific skills are knowledge of programs used to monitor inventory (VendPOS, AmberPOS, and Lightspeed), the ability to learn new programs, and experience with Microsoft Office applications. For janitor, this is a certificate in using particular machines and a certification in janitorial and cleaning sciences. For security, this is CPR and First Aid and stating that they are licensed in their state. For server, this is CPR, First Aid, and experience with point-of-service (POS) software used in food service. For kitchen staff, this is

CPR, First Aid, and a certificate or training in food safety. An example of some of these skills are shown in the resume examples later in this appendix, and additional resumes are available upon request.

Of course, not all added quality features will have a positive effect,[7] and some other randomly added features (e.g., certain employers, template styles) might have positive or negative effects. Neumark (2012) shows the iterative process to select from among the resume features to be used in the Neumark (2012) correction. This mirrors the process outlined in Lahey and Beasley (2018) for letting the data "speak" about which features actually affect callback rates.

[7] For example, Spanish, a college degree, and the occupation-specific skills often boosted interview rates in Neumark, Burn, and Button (forthcoming), while adding typos to the resume (missing periods or commas), volunteer experience, and employee of the month awards did not have positive effects, sometimes having negative ones. Lahey and Beasley (2018) also discuss a similar issue for typos. These differential results by quality element prompted us to choose some different quality elements.

Online Appendix Table C1 – Heteroskedastic Probit Estimates

	Combined	**Retail**	**Server**	**Kitchen**	**Security**	**Janitor**
	(1)	(2)	(3)	(4)	(5)	(6)
	Common quality features	All quality features	All quality features	All quality features	All quality features	All quality features
A. Probit estimates						
Indigenous (marginal)	0.003 (0.006)	0.009 (0.013)	0.003 (0.012)	-0.003 (0.011)	0.014 (0.021)	0.002 (0.014)
B. Heteroskedastic probit estimates						
Indigenous (marginal)	0.001 (0.006)	0.009 (0.013)	0.004 (0.011)	-0.006 (0.010)	0.014 (0.021)	0.002 (0.014)
Overidentification test: ratios of coefficients on quality features for Indigenous relative to white are equal (p-value, Wald test)	0.993	1.000	1.000	0.999	0.693	0.992
Standard deviation of unobservables, Indigenous/white	0.911	1.003	1.037	0.858	1.015	1.047
Test: homoscedastic vs. heteroskedastic probit (p-value, Wald test for equal variances)	0.282	0.986	0.824	0.181	0.960	0.880
Indigenous-level (marginal)	0.024 (0.021)	0.008 (0.036)	-0.005 (0.041)	0.030 (0.029)	0.011 (0.058)	-0.009 (0.073)
Indigenous -variance (marginal)	-0.022 (0.021)	-0.001 (0.035)	0.009 (0.040)	-0.036 (0.027)	0.003 (0.059)	0.011 (0.074)
N	13,516	2,926	2,774	4,858	1,306	1,652

Notes: See Neumark (2012) and Neumark, Burn, and Button (forthcoming) for a discussion of this methodology. See also the notes in Table 6. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Regressions use the “Regular Controls” from Table 6. All higher-quality resumes randomly receive all but one of the following quality features: fluency in Spanish as a second language, a more detailed cover letter, the lack of typos in the cover letter, and two occupation-specific skills. The occupation-specific skills for retail included knowledge of programs used to monitor inventory (VendPOS, AmberPOS, and Lightspeed) and experience with Microsoft Office applications; janitor included a certificate in using particular machines and a certification in janitorial and cleaning sciences; security included CPR and First Aid and stating that they are licensed in their state; server included CPR and First Aid and experience with point-of-service (POS) software used in food service; kitchen staff included CPR and First Aid and a certificate or training in food safety.

Online Appendix D: Additional Robustness Checks

Probit vs. Linear Probability Model

As noted, we originally committed to using a probit model in our pre-analysis plan. However, we became aware that it was more common to use a linear probability model due to issues with coefficients in probit models (Ai and Norton 2003; Greene 2010). Our main results (Table 6) are nearly identical regardless of if we use a linear probability model or a probit model (either with average marginal effects or marginal effects at the means.) We present these results in Online Appendix Table D1.

Clustering

In resume-correspondence studies, there are two levels of clustering. First, there is clustering on the resume. This occurs because we do not control for every detail on the resume or in the application, given all the randomized inputs into each resume. Resumes are also sent out more than once. Each day, a particular pair of resumes is sent out to all job openings in that city and occupation. For this reason, it is essential to cluster on the resume so as to not understate the standard errors. Second, there is clustering on the employer, who is likely to treat both applicants somewhat similarly given the particulars of their position and candidate search.

Dealing with these two possible levels of clustering is not straightforward. Our main results cluster our standard errors on the resume. The difficulty with clustering on the job, however, is that we cannot match all responses perfectly to job ads.[8] However, for the pairs of applications that we can match to jobs, our standard errors are nearly identical regardless of if

[8] This occurs because we do not have a unique phone number for each applicant. Since we assign multiple applicants the same number, we are sometimes not able to match a voicemail response to a specific job even if we can match it to a specific resume. More details on how this is addressed generally can be found in Online Appendix A.

we cluster on the resume, job, or multi-way cluster on both. We present these results in Online Appendix Table D2.

Estimates by Signal Type and Saliency of Signals

In Online Appendix Tables D3 and D4, we re-estimate our main results in two different ways to deal with the issue that our Navajo names signal may not have been salient. In Online Appendix Table D3 we present in column (1) the main results for comparison from Table 6 column (2). In column (2) we re-code resumes with the Navajo last name signal as no longer Native American (*NA = 0*). In column (3) we instead create a separate control variable for resumes that have only the Navajo last name signal, such that *NA* identifies only those resumes with at least one other signal. The results are unchanged in all cases.

Online Appendix Table D4 presents the results by signal type from Table 10, but with the Navajo name signal re-coded as not being a signal. So, a resume with a Navajo name signal and a language signal, for example, is re-coded as only having a language signal. This re-coding also does not affect the results. Across both these tables, there is no evidence of our results changing when we drop or otherwise separately control for the Navajo last name signal.

The "Heckman-Siegelman" Critique and the Neumark (2012) Correction

See Online Appendix C for a detailed discussion of this issue, with a model and full results.

Do Callbacks Capture Hiring Discrimination?

We coded two forms of employer responses: (1) callbacks, and (2) explicit interview offers only. The former is used as the default in many other resume correspondence studies. Callbacks include explicit interview offers but also more ambiguous positive responses (e.g., "I have reviewed your application and have some additional questions for you."). Online Appendix

Table D5 compares how our main results from Table 6 change when we use explicit interview offers instead of callbacks. Our results do not vary.

Population and Occupation Weighting

We attempted to apply for all eligible job openings that met our criteria in each city and occupation. Since our main estimates are unweighted, this means that we oversampled populous cities. What would be more realistic would be to weight the estimates by city so that they reflect the population distribution of Indigenous Peoples across these cities. Similarly, we can weight by the frequency of occupations according to the CPS data. This helps us balance the sample if we over- or under-sampled certain occupations. For example, some research assistants may have been more consistent about finding jobs to apply to or the proportions of job ads by occupation on the job website we use may not match the national distribution. This is indeed possible, although we expect the number of jobs that we applied to in each occupation to be highly correlated with the actual frequencies of those jobs. Neumark, Burn, Button, and Chehras (2018) grappled with this issue at around the same time as us, and we direct the reader there for a more detailed discussion about weighting.

Online Appendix Table D6 describes how we created population weights. We first used population counts for AIANs and NHPIs from Norris, Vines, and Hoeffel (2012) and Hixson, Hepler, and Kim (2012), respectively. We used two different population estimates: AIAN (NHPI) alone or AIAN (NHPI) alone or in combination ("in comb"). We constructed population weights by dividing the number of jobs applied to, by city, and by the AIAN or NHPI population in each city, and then normalizing such that a value of one meant no relative weight (neither up nor down) is applied to that city.[9] Weights greater than (less than) one meant that our number of observations for that city was lower (higher) relative to the Indigenous population compared to other cities, and thus the observations for that city needed to be up-weighted (down-weighted). This table indicates that, as expected, we over-sampled Chicago and Houston, large cities with a small proportion of Indigenous Peoples, and under-sampled Honolulu, Anchorage, and other cities with a larger proportion of Indigenous Peoples.

Online Appendix Table D7 presents our construction of occupation weights. To construct these weights, we used all months of the 2015 Current Population Survey (CPS) to estimate the proportion of those aged 25 to 35 who were employed in each occupation. To match the narrower occupational coding in the CPS to our broader occupations (retail, kitchen, server, janitor, and security), we add up occupation counts for each CPS occupation that matched our broader occupations.[10] Online Appendix Tables A3 and A4 present most of the occupation frequencies for these narrower occupations. These occupation weights suggest that relative to the nationally-representative employment estimates in the CPS, we oversampled server and security and under-sampled retail.

[9] We split our applications to jobs in Los Angeles into two groups and weighted them differently since we sent either Native American/white pairs or Native Hawaiian/white pairs to each job opening, and these are weighted differently.

[10] Our broader occupation of retail corresponds to retail salespersons, cashiers, counter and rental clerks, sales representatives (services, all other), and sales and related workers (all others); kitchen, our broadest occupational category, corresponds to cooks, food preparation workers, dishwashers, combined food preparation and serving workers (including fast food), counter attendants (cafeteria, food concession, and coffee shops), food servers (non-restaurant), and dining room and cafeteria attendants and bartender helpers; server corresponds to waiters and waitresses, bartenders, and hosts and hostesses (restaurant, lounge, and coffee shop); janitor corresponds to janitors and building cleaners and grounds maintenance workers; and security corresponds only to security guards and gaming surveillance officers.

Online Appendix Table D8 presents our occupation-by-population weights. We calculated these by multiplying the occupation and population weights together. These weights have a high range, from 0.11 (Chicago, servers, using “in combination”) to 5.20 (Honolulu, retail, “in combination”).

Finally, in Online Appendix Table D9, we present our main results (replicating Table 6, column (2)) under different types of weighting (Indigenous population in the city, occupational popularity, and both). Our results are unchanged regardless of how we weight (or do not weight) the results.[11]

Robustness to the Proportion Hispanic in each Occupation and City.

Related to the concern about whether jobs are “typed” to be more appropriate for certain racial groups is that typing could vary by city, especially by the size of the Hispanic population. Thoughtful discussions with Randall Akee and others made it clear that we need to explore if discrimination varies by how often Hispanics take certain jobs in our occupation and city combinations.

We re-analyzed our data, dropping some occupation-city-gender combinations where Hispanics outnumber whites, finding similar results (results are available upon request). While our analysis of occupations in Tables 2 and 3 showed that all our occupations are common for whites, this analysis used national data. We re-did this analysis to present the proportion of individuals, by sex, in each occupation and city who are white (defined as white only and non-Hispanic), AIAN (alone or in combination, independent of Hispanic ancestry), or Hispanic (independent of race).

[11] Our other results, replicating other tables, are also fundamentally the same, regardless of which type of weighting we use. These results are available upon request.

This more detailed analysis shows that, while whites are common in all occupation-city-sex combinations, they are outnumbered by Hispanics in some cases. This is especially the case in kitchen staff and janitor occupations, where Hispanics outnumber whites everywhere except in Oklahoma City (women and men) and Chicago (women only). This is also especially the case for Los Angeles, where Hispanics outnumber whites in all cases. Outside of kitchen staff, janitor, and Los Angeles, Hispanics outnumber whites in only a few cases: retail sales for women in Albuquerque and Houston and servers for men in Albuquerque.

To investigate whether our results are robust to the proportion of Hispanics in each occupation by city, we re-estimated the results in Tables 6 to 9 dropping any occupation-city-gender combination where Hispanics outnumber whites. These results, available upon request, do not show any different results. We also re-estimate the regression in Table 6, column (2) (based off Equation [1]), but we add an interaction between the Native American (*NA*) indicator variable and a variable equal to the ratio of whites to Hispanics in each occupation-city-gender cell. The coefficient on this interaction variable is not statistically significant and is not of a meaningful magnitude (it is 0.005, with a standard error of 0.005 in the preferred specification). Thus, it does not appear that our discrimination estimates vary with the proportion of people in the occupation who are Hispanic.

Online Appendix Table D1 – Main Results Under Linear Probability and Probit Models

	Probit, Marginal Effects at Means (1)	Probit, Average Marginal Effects (2)	Linear Probability Model (3)
Native American	-0.004	-0.004	-0.004
	(0.010)	(0.009)	(0.009)
… x Reservation	0.000	0.000	-0.000
	(0.012)	(0.012)	(0.012)
… x Reservation x Reservation Job	0.008	0.008	0.006
	(0.017)	(0.016)	(0.017)
Alaska Native	0.004	0.004	0.005
	(0.030)	(0.030)	(0.035)
Native Hawaiian	-0.004	-0.004	-0.003
	(0.012)	(0.012)	(0.013)
Rural	-0.016	-0.016	-0.016
	(0.014)	(0.014)	(0.013)
… x Rural Job	0.002	0.002	0.002
	(0.020)	(0.020)	(0.018)
Callback Rate for White:		19.8%	

Notes: N = 13,516. See the notes to Table 6. Column (3) presents the main results from Table 6 (Column (2).)

Online Appendix Table D2 – Robustness of the Estimates in Table 6 to Alternative Standard Error Clustering

	Cluster on Resume (1)	Cluster on Job (2)	Multi-way Cluster, Resume and Job (3)
Native American	-0.002	-0.002	-0.002
	(0.006)	(0.008)	(0.008)
… x Reservation	-0.003	-0.003	-0.003
	(0.009)	(0.009)	(0.009)
… x Reservation x Reservation Job	-0.005	-0.005	-0.005
	(0.012)	(0.012)	(0.012)
Alaska Native	-0.004	-0.004	-0.004
	(0.014)	(0.024)	(0.024)
Native Hawaiian	-0.007	-0.007	-0.007
	(0.007)	(0.012)	(0.012)
Rural	-0.019	-0.019	-0.019
	(0.010)	(0.010)	(0.010)
… x Rural Job	0.004	0.004	0.004
	(0.014)	(0.014)	(0.014)
Callback Rate for White:		19.8%	

Notes: See the notes to Table 6. N=11,759 since we dropped 1,757 applications that could not be matched to a specific job.

Online Appendix Table D3 – Replicating Table 6, Column (2), Ignoring Navajo Last Name Signals

	All Signals (1)	Navajo Last Name Signals Dropped (2)	Navajo Name Signal as a Control (3)
Native American	-0.004	-0.003	-0.004
	(0.009)	(0.009)	(0.009)
… x Reservation	-0.000	-0.001	0.000
	(0.012)	(0.012)	(0.012)
… x Reservation x Reservation Job	0.006	0.006	0.006
	(0.016)	(0.016)	(0.016)
Alaska Native	0.005	0.005	0.005
	(0.035)	(0.035)	(0.035)
Native Hawaiian	-0.003	-0.003	-0.003
	(0.013)	(0.013)	(0.013)
Rural	-0.016	-0.015	-0.016
	(0.013)	(0.013)	(0.013)
… x Rural Job	0.002	0.002	0.002
	(0.018)	(0.018)	(0.018)
Navajo Last Name Signal	…	…	-0.007
			(0.026)
Callback Rate for White:		19.8%	

Notes: N=13,516. Column (1) is Column (2) from Table 6. For column (2), any Indigenous resume with the only signal being a Navajo last name signal was recoded as being a non-Indigenous resume. For column (3), Navajo last name signals were added as a separate control variable to the regression in Column (1).

Online Appendix Table D4 – Replicating Table 10, Ignoring Navajo Last Name Signals

	Default (1)	N	Ignore Navajo Name (2)	N
Indigenous				
… x Volunteer Only	-0.006	3,029	-0.007	3,118
	(0.010)		(0.010)	
… x Language Only	0.006	1,723	0.006	1,801
	(0.010)		(0.010)	
… x First Name (Native Hawaiian) Only	-0.017	475	-0.016	475
	(0.018)		(0.018)	
… x Last Name (Navajo) Only	-0.007	222	N/A	0
	(0.026)			
… x Two Signals	0.003	823	0.013	802
	(0.015)		(0.016)	
… x Three Signals	0.038	92	0.028	65
	(0.037)		(0.044)	
Boys & Girls Club (Volunteer Control)	-0.007	3,298	-0.006	3,298
	(0.009)		(0.009)	
Food Bank (Volunteer Control)	-0.006	3,460	-0.005	3,460
	(0.009)		(0.009)	
Irish Gaelic (Language Control)	-0.017	831	-0.016	831
	(0.013)		(0.013)	
Callback Rate for White:	19.8%			

Notes: N=13,516. See the notes to Tables 6 and 10. Regressions use the "Regular Controls" from Table 6 (column (2)). Column (1) presents the results from Table 10 for comparison. Column (2) repeats this analysis, pretending that there is no Navajo last name signal. This re-codes some resume with a last name signal and one other signal as just having that one other signal, and re-codes resumes with the last name signal, volunteer signal, and language signal as "Two Signals." Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*).

Online Appendix Table D5 – Estimates from Tables 6, 8, 9, and 10, Comparing Results Using Interview Rates Instead of Callback Rates

	Callback (1)	Interview (2)
	Panel (a) (Corresponding to Table 6)	
Native American	-0.004 (0.009)	-0.002 (0.008)
… x Reservation	-0.000 (0.012)	0.007 (0.010)
… x Reservation x Reservation Job	0.006 (0.016)	0.001 (0.014)
Alaska Native	0.005 (0.035)	0.010 (0.030)
Native Hawaiian	-0.003 (0.013)	-0.001 (0.011)
	Panel (b) (Corresponding to Table 8)	
Indigenous		
… x Retail	0.006 (0.017)	0.013 (0.015)
… x Server	-0.002 (0.016)	0.008 (0.015)
… x Kitchen	-0.007 (0.014)	0.007 (0.013)
… x Janitor	0.003 (0.021)	0.009 (0.018)
… x Security	0.011 (0.022)	-0.005 (0.018)
… x Female x Retail	-0.003 (0.025)	-0.018 (0.022)
… x Female x Server	0.002 (0.024)	-0.007 (0.022)
… x Female x Kitchen	0.001 (0.021)	-0.011 (0.018)
… x Female x Janitor	-0.008 (0.031)	-0.023 (0.024)
	Panel (c) (Corresponding to Table 9)	
Indigenous		
… x Phoenix	0.041 (0.023)	0.032 (0.019)
… x Chicago	-0.009 (0.018)	-0.013 (0.014)
… x Los Angeles (NA)	-0.001 (0.014)	0.006 (0.011)
… x Los Angeles (NH)	-0.014 (0.019)	-0.016 (0.015)
… x Alaska (AN)	0.005 (0.035)	0.010 (0.030)
… x Honolulu (NH)	0.002 (0.019)	0.005 (0.015)
… x Billings	0.012 (0.062)	-0.024 (0.054)
… x Albuquerque	-0.037 (0.029)	-0.036 (0.027)
… x New York City	-0.011 (0.011)	-0.002 (0.010)
… x Oklahoma City	0.018 (0.033)	0.001 (0.028)
… x Sioux Falls	-0.004 (0.078)	0.023 (0.073)
… x Houston	-0.002 (0.024)	0.005 (0.020)
	Panel (d) (Corresponding to Table 10)	
Indigenous		
… x Volunteer	-0.006 (0.010)	0.000 (0.008)
… x Language	0.006 (0.010)	0.009 (0.009)
… x First Name (Native Hawaiian)	-0.017 (0.018)	-0.023 (0.015)
… x Last Name (Navajo)	-0.007 (0.026)	-0.011 (0.025)
Two Signals	0.003 (0.015)	0.004 (0.013)
Three Signals	0.038 (0.037)	0.033 (0.034)

Notes: N=13,516. See the notes to Tables 6, 8, 9, and 10. Column (1) repeats the results from these tables. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*).

Online Appendix Table D6 – Construction of Population Regression Weights

Panel (a): Cities with Native American and Alaska Native Applicants								
	Total	AIAN alone or in combination		AIAN alone		Jobs Applied	Population Weight	
City	Population	%	Count	%	Count		In Comb.	Alone
New York	8,175,133	1.4%	111,749	0.7%	57,512	2,756	0.85	0.85
Los Angeles	3,792,621	1.4%	54,236	0.7%	28,215	1,866	0.61	0.62
Phoenix	1,445,632	3.0%	43,724	2.2%	32,366	1,530	0.60	0.86
Oklahoma City	579,999	6.3%	36,572	3.5%	20,533	614	1.25	1.36
Anchorage	291,826	12.4%	36,062	7.9%	23,130	564	1.34	1.67
Albuquerque	545,852	6.0%	32,571	4.6%	25,087	700	0.97	1.46
Chicago	2,695,598	1.0%	26,933	0.5%	13,337	1,466	0.38	0.37
Houston	2,099,451	1.2%	25,521	0.7%	14,997	1,106	0.48	0.55
Sioux Falls	153,888	3.6%	5,540	2.7%	4,155	154	0.75	1.10
Billings	104,170	6.0%	6,251	4.4%	4,584	212	0.62	0.88
National	308,745,538	1.7%	5,220,579	0.9%	2,932,248	10,968		

Panel (b): Cities with Native Hawaiian Applicants								
	Total	NHPI alone or in combination		NHPI alone			Population Weight	
City	Population	%	Count	%	Count	Jobs Applied	In Comb.	Alone
Honolulu	953,207	24.5%	233,637	9.5%	90,878	2,020	2.42	1.84
Los Angeles	3,792,621	0.6%	20,924	0.3%	10,079	508	0.86	0.81
National	308,745,538	0.4%	1,225,195	0.2%	540,013	2,290		

Notes: We split Los Angeles into two samples since we sent either Native American/white pairs (NA) or Native Hawaiian/white pairs (NH) to each job opening. We construct population weights using 2010 Census population counts for AIANs and NHPIs from Norris, Vines, and Hoeffel (2012) and Hixson, Hepler, and Kim (2012), respectively. The percents for Los Angeles in Panel (b) are based on county-level rather than city-level data, from Hixson, Hepler, and Kim (2012). Weights are constructed by dividing the number of observations, by city, by the Indigenous population in each city, and then normalizing such that a value of one means no weight is applied to that city. Weights greater than (less than) one mean that our number of observations for that city is lower (higher) relative to the Indigenous population, compared to for other cities, and thus the observations for that city need to be up-weighted (down-weighted.)

Online Appendix Table D7 – Construction of Occupation Regression Weights

	Jobs Applied (1)	Employment Share (2)	Occupation Weight (3)
Retail	2,926	3.81%	2.15
Kitchen	4,858	2.18%	1.23
Server	2,774	0.49%	0.28
Janitor	1,652	1.84%	1.04
Security	1,306	0.53%	0.30

Notes: See the notes to Online Appendix Table D6. Estimates from Column (2) are the proportion of those aged 25 to 35 who are employed and report that occupation (instead of another occupation), using all months of the 2015 Current Population Survey.

Online Appendix Table D8 – Construction of Occupation-by-Population Regression Weights

Occupation (weight)	Retail (2.15)		Kitchen (1.23)		Server (0.28)		Janitor (1.04)		Security (0.30)	
City	In Comb. (3)	Alone (4)	In Comb. (5)	Alone (6)	In Comb. (7)	Alone (8)	In Comb. (9)	Alone (10)	In Comb. (11)	Alone (12)
New York	1.82	1.55	1.04	0.89	0.24	0.20	0.88	0.75	0.25	0.22
Los Angeles (NA)	1.31	0.81	0.75	0.46	0.17	0.10	0.63	0.39	0.18	0.11
Phoenix	1.29	1.11	0.74	0.64	0.17	0.14	0.62	0.54	0.18	0.15
Oklahoma City	2.68	3.66	1.53	2.09	0.35	0.47	1.30	1.77	0.37	0.51
Anchorage	2.88	4.82	1.65	2.75	0.37	0.62	1.39	2.33	0.40	0.67
Albuquerque	2.09	3.06	1.20	1.75	0.27	0.40	1.01	1.48	0.29	0.43
Chicago	0.83	0.31	0.47	0.18	0.11	0.04	0.40	0.15	0.12	0.04
Houston	1.04	0.57	0.59	0.33	0.13	0.07	0.50	0.28	0.14	0.08
Sioux Falls	1.62	1.78	0.93	1.02	0.21	0.23	0.78	0.86	0.23	0.25
Billings	1.33	1.17	0.76	0.67	0.17	0.15	0.64	0.57	0.18	0.16
Honolulu	5.20	9.56	2.98	5.47	0.67	1.24	2.52	4.62	0.72	1.33
Los Angeles (NH)	1.85	1.50	1.06	0.86	0.24	0.19	0.90	0.73	0.26	0.21

Notes: See the notes to Online Appendix Tables D6 and D7. The combined occupation and population weights are created by multiplying the occupation and population weights together.

Online Appendix Table D9 – Robustness of the Estimates in Table 6 to Different Weights

	Un-Weighted (1)	Pop. Weights (Alone) (2)	Pop. Weights (+ in Comb.) (3)	Occ. Weights (4)	Occ. + Pop. Weights (Alone) (5)	Occ. + Pop. Weights (+ in Comb.) (6)
Native American	-0.002	-0.005	-0.004	-0.006	0.015	0.009
	(0.006)	(0.011)	(0.010)	(0.011)	(0.023)	(0.018)
… x Reservation	-0.003	-0.000	-0.003	0.004	-0.050*	-0.039*
	(0.009)	(0.013)	(0.013)	(0.014)	(0.030)	(0.023)
… x Reservation	-0.005	0.003	0.005	0.007	0.036	0.031
x Reservation Job	(0.012)	(0.018)	(0.017)	(0.018)	(0.033)	(0.028)
Alaska Native	-0.004	0.005	0.005	-0.005	-0.013	-0.013
	(0.014)	(0.035)	(0.035)	(0.040)	(0.041)	(0.041)
Native Hawaiian	-0.007	-0.001	0.000	-0.002	-0.006	-0.008
	(0.007)	(0.014)	(0.014)	(0.016)	(0.015)	(0.014)
Rural	-0.019	-0.021	-0.019	-0.019	-0.026	-0.020
	(0.010)	(0.014)	(0.013)	(0.014)	(0.033)	(0.026)
… x Rural Job	0.004	0.007	0.011	0.019	0.045	0.026
	(0.014)	(0.021)	(0.020)	(0.014)	(0.047)	(0.036)
Callback Rate for White:	19.8%					

Notes: See the notes to Table 6. N=13,516. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*).

Online Appendix Table D10 – Demographics of Each Occupational Grouping, by City and Gender

	% of Men in the Occupation that are:			% of Women in the Occupation that are:		
	White Only	Hispanic	AIAN	White Only	Hispanic	AIAN
				Retail		
Albuquerque	48.8	37.2	8.3	34.6	**43.8**	13.8
Chicago	64.5	14.8	0.5	51.2	22.7	0.9
Houston	37.7	33.6	1.9	31.6	**39.0**	0.9
Los Angeles	30.2	**47.5**	1.8	25.3	**52.4**	3.9
New York	46.5	21.2	0.7	42.1	25.5	0.5
Oklahoma City	74.4	7.3	5.9	65.8	7.9	14.4
Phoenix	67.5	24.0	0.6	58.9	28.3	2.1
				Server		
Albuquerque	39.6	**44.6**	10.0	45.5	38.4	7.2
Chicago	59.9	28.4	0.7	64.0	20.0	0.5
Houston	39.8	35.6	1.7	40.8	42.1	0.6
Los Angeles	31.4	**46.7**	2.1	35.2	**36.2**	1.8
New York	41.8	25.1	1.5	41.8	25.1	1.5
Oklahoma City	70.8	11.2	1.7	61.6	16.0	7.1
Phoenix	54.2	35.7	2.4	64.8	22.8	4.2
				Kitchen		
Albuquerque	24.8	**55.9**	14.4	21.6	**59.6**	10.3
Chicago	25.8	**54.8**	1.2	40.9	38.2	0.7
Houston	14.1	**57.3**	4.9	14.7	**66.8**	1.2
Los Angeles	12.8	**71.7**	1.6	13.9	**68.8**	2.1
New York	22.1	**51.5**	2.1	33.8	**34.1**	2.9
Oklahoma City	50.5	19.5	10.7	55.7	21.4	8.8
Phoenix	37.6	**49.0**	2.2	39.9	**45.8**	2.3
				Janitor		
Albuquerque	20.0	**69.5**	9.7	20.5	**76.0**	3.5
Chicago	37.8	**45.3**	0.7	45.5	32.9	1.9
Houston	12.7	**69.5**	1.3	9.7	**72.8**	3.9
Los Angeles	8.1	**81.6**	3.0	5.1	**84.5**	2.9
New York	29.9	**49.1**	1.4	24.6	**59.6**	1.0
Oklahoma City	50.7	23.0	13.3	54.9	26.9	9.0
Phoenix	22.9	**69.3**	2.8	15.6	**67.8**	8.0
				Security		
Albuquerque	45.7	41.1	11.4	N/A	N/A	N/A
Chicago	38.8	18.1	1.0	N/A	N/A	N/A
Houston	33.4	17.6	4.5	N/A	N/A	N/A
Los Angeles	22.9	**41.3**	2.9	N/A	N/A	N/A
New York	25.1	20.9	2.3	N/A	N/A	N/A
Oklahoma City	59.2	8.2	12.1	N/A	N/A	N/A
Phoenix	62.7	17.4	7.3	N/A	N/A	N/A

Notes: Bolded number indicate when the % Hispanic > % white. Calculated from Current Population Survey data from IPUMS-CPS (Flood et al., 2015). White only includes those who only report white as a race and do not report being Hispanic. Hispanic includes those who reporting being Hispanic, regardless of race. AIAN includes those who report being AIAN alone or in part, regardless of if they report being Hispanic or report another race as well. The occupational groupings correspond to the following occupational codes: retail sales (retail salespersons; cashiers; counter and rental clerks; sales representatives, services, all other; and sales and related workers, all others, in the Census occupational classification), kitchen staff (cooks; food preparation workers; dishwashers; combined food preparation and serving workers, including fast food; counter attendants, cafeteria, food concession, and coffee shops; food servers, non-restaurant; and dining room and cafeteria attendants and bartender helpers), server (waiters and waitresses; bartenders; and hosts and hostesses, restaurant, lounge, and coffee shop), janitors (janitors and building cleaners and grounds maintenance workers), and security guards (security guards and gaming surveillance officers).

Online Appendix Table D11 – Comparison of the Timing of Our Study with Others in the US

Study	Timing	Unemployment Rates During Timing	Percentile Range
This Paper	March to December 2017	4.1-4.5	16th-24th
Agan and Starr (2018)	Jan, Feb, May, June 2015	5.3-5.7	42nd-55th
Ameri et al. (2015)	June to August 2013	7.2-7.5	80th-85th
Bailey et al. (2013)	March to May 2010	9.6-9.9	**97th-99th**
Bendick et al. (1997)	March to June 1993	7.0-7.1	77th-79th
Bendick et al. 1999	March 1995 to March 1996	5.4-5.8	44th-59th
Bertrand and Mullainathan (2004)	July 2001 to May 2002	4.6-5.8	25th-59th
Darolia et al. (2016)	May 2013 to May 2014	6.3-7.5	69th-86th
Decker et al. (2015)	June to August 2011, June to August 2012*	8.1-9.1	**90th-96th**
Farber et al. (2017)	March to May 2012, July to September 2012	7.8-8.2	**89th-91st**
Gaddis (2015)	March to August 2011	9.0-9.1	**95th-96th**
Hipes et al. (2016)	June 2011 to May 2012	8.2-9.1	**91st-96th**
Jacquement and Yannelis (2012)	August 2009 to February 2010	9.6-10.0	**97th-99th**
Kleykamp (2009)	Year of 2007*	4.4-5.0	21st-35th
Lahey (2008)	February 2002 to February 2003	5.7-6.0	55th-65th
Mishel (2016)	March, April, May 2014*	6.3-6.7	69th-74th
Neumark et al. (forthcoming)	January to June 2015	5.3-5.7	41st-56th
Nunley et al. (2015)	January to July 2013	7.3-8.0	**82nd-91st**
Pager (2003)	June to December 2001	4.5-5.7	23rd-56th
Tilcsik (2011)	Year of 2005*	4.9-5.4	30th-45th
Widner and Chicoine (2011)	February and March 2008*	4.9-5.1	30th-37th
Wright et al. (2013)	July to October 2009	9.5-10.0	**96th-99th**

Notes: This table includes resume or audit studies listed in the tables in Neumark (2018) and Baert (2018) that were done in the United States. Unemployment rates are national and seasonally adjusted and come from series LNS14000000 (accessed November 25, 2018 from https://data.bls.gov/timeseries/LNS14000000) using January 1948 to October 2018. The percentile rank is calculated as the percentile for the unemployment range, given all unemployment rate estimates since 1948. Bolding of the percentile rank indicates studies where the percentile range includes at least the 90th percentile. For those timing allocations with a *, we estimated the timings as follows, based on vague descriptions from the paper: Decker et al. (2015) "two 16-week periods during the summer of 2011 and during the same timeframe in 2012", Kleykamp (2009) "six-month period" (no year specified), Mishel (2016) "spring of 2014", Tilcsik (2011) "six-month period in 2005", Widner and Chicoine (2011) "In February 2008, we began sending…"

Online Appendix Table D12 – Discrimination Estimates by City with Reservation Signal Interactions

Indigenous x Reservation	Estimate	N Applicants
… x Albuquerque	0.0116	163
	(0.0397)	
… x Billings	0.0457	45
	(0.0897)	
… x Chicago	0.0166	290
	(0.0251)	
… x Houston	-0.0026	276
	(0.0359)	
… x Los Angeles (Native Am.)	-0.0224	423
	(0.0214)	
… x New York	-0.0099	588
	(0.0149)	
… x Oklahoma City	-0.0693	177
	(0.0471)	
… x Phoenix	0.0238	385
	(0.0335)	
… x Sioux Falls	0.0079	32
	(0.1190)	

Notes: N=13,516. See the notes to Table 6. Regressions use the "Regular Controls" from Table 6. Different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*).

Online Appendix E: Additional Details and Results from the Resume Survey

We fielded two surveys on Amazon Mechanical Turk to test the saliency of our signals of Indigenous status. The first survey ("resume survey") was similar to Kroft, Notowidigdo, and Lange (2013), where we asked individuals what they remember about applicants after reading our resumes. We present the questions from this survey at the end of this appendix.

More Details on the Resume Survey

First, we asked surveyed individuals to read one of the resumes from our study and consider the candidate for a job position in the relevant occupation. Specifically, the survey prompted the subjects with the following right above the resume that appeared on screen: "Suppose you were a hiring manager in a firm who is hiring for an entry-level (retail/cook/server/janitor/security guard) position. Please spend up to a minute reading the resume."

The specific resumes we tested had the following signals of Indigenous status (or no signal):[12]

1. Language signal only (N = 323)
2. Volunteer signal only (N = 173)
3. Volunteer + language (N = 170)
4. Navajo last names only (N = 281; Begay, Tsosie, Benally, or Yazzie)
5. Navajo last names + language (N = 255)
6. Navajo last names + volunteer (N = 176)
7. Navajo last names + language + volunteer (N = 161)
8. Hawaiian first names (N = 201; Keoni, Kekoa, Ikaika, or Maile)
9. White (N = 205; no signals, three versions)

[12] The actual resumes are available upon request.

We then asked the subjects to recall or guess the socioeconomic and demographic characteristics of the applicant to see what was detected and remembered from the resume (see below for the entire list of questions). We asked individuals what they thought about the job applicant's race or ethnicity, likelihood of being born in the US, age, and gender. We also asked individuals to recall aspects featured on the resume, such as employment status, duration of the last job, if they spoke a second language spoken, and their highest educational attainment. We asked these additional questions to determine how often these aspects were detected and recalled, compared to our signals of Indigenous status.

Resume Survey Questions

1) What is the race or ethnicity of this applicant?
2) How likely is it that this person was born in the US?
3) How old, in years, do you think the applicant is? Please enter a number (e.g., 35)
4) What's the gender of the applicant?
5) Was the applicant currently employed?
6) How long, in years, did the applicant hold their last job? Please enter as a number (e.g., 2.5)
7) Does the applicant speak a second language?
8) If you answered yes to Q7, which language is it?
9) What is the highest degree this applicant earned?
10) Please guess the total combined family income for the applicant's household for the past 12 months. This should include income (before taxes) from all sources, wages, rent from properties, social security, disability and/or veteran's benefits, unemployment benefits, workman's compensation, help from relatives (including child payments and alimony), and so on.
11) Do you think that the applicant grew up in a rural, suburban, or urban environment?
12) What is your State of residence?
13) What is your age?
14) Which category(s) best describe(s) your race?
15) Are you Spanish, Hispanic, or Latino/Latina?
16) What is the highest degree or level of school you have completed? If currently enrolled, highest degree received.
17) What is your current employment status?
18) What is your gender?

More Detailed Resume Survey Results

Online Appendix Table E1 – Responses to "What is the race or ethnicity of this applicant?" from the Resume Survey, Full Sample

Group	Distribution of Responses										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
White	**86.8%**	35.8%	58.9%	46.8%	32.2%	17.0%	23.9%	21.8%	20.5%	10.0%	20.1%
American Indian or Alaska Native	1.5%	1.5%	**18.8%**	**32.4%**	**37.2%**	**74.2%**	**58.0%**	**59.4%**	**62.1%**	0%	0%
Native Hawaiian or Pacific Islander	0%	**26.4%**	2.1%	14.5%	15.8%	3.8%	4.0%	12.9%	6.8%	**82.0%**	**75.0%**
Hispanic	1.5%	6.5%	8.5%	2.3%	4.3%	2.1%	5.1%	1.2%	3.7%	0%	4.2%
Black	4.4%	19.9%	4.6%	2.3%	3.4%	1.7%	2.8%	0.6%	3.1%	2.0%	0%
Asian	0%	1.5%	1.1%	0%	0.9%	0%	1.1%	1.2%	0%	2.0%	0%
Other	5.9%	8.5%	6.0%	1.7%	6.2%	1.3%	5.1%	2.9%	3.7%	4.0%	0%
No Signals (White)	x										
Native Hawaiian First Name		x									
Navajo Last Name			x			x	x		x		
Language (Navajo)				x		x		x	x		
Volunteer (Native American)					x		x	x	x		
Language (Hawaiian)										x	
Volunteer + Language (Hawaiian)											x
N	205	201	282	173	323	236	176	170	161	50	24

Notes: The sample includes both a national sample (no restriction based on state of residence) and an oversample of Arizona and New Mexico. Estimates are bolded to highlight the race that is intended to be signaled in each case. Row totals are non-exclusive, with values in the lower half of the table being nested within those values from the upper half of the table.

Online Appendix Table E2 – Responses to “What is the race or ethnicity of this applicant?” from the Resume Survey, Arizona and New Mexico Only

Group	Distribution of Responses						
White	23.6%	0%	17.1%	17.5%	21.1%	16.9%	18.0%
American Indian or Alaska Native	**58.3%**	**71.1%**	**73.2%**	**76.7%**	**70.7%**	**78.3%**	**68.5%**
Native Hawaiian or Pacific Islander	5.5%	3.6%	4.9%	2.9%	3.3%	2.4%	6.3%
Hispanic	4.7%	2.4%	3.7%	1.9%	1.6%	2.4%	5.4%
Black	0.8%	0%	1.2%	0%	0%	0%	0%
Other	7.1%	22.9%	0%	1.0%	3.3%	0%	1.8%
Navajo Last Name	x			x	x		x
Language (Navajo)		x		x		x	x
Volunteer (Native American)			x		x	x	x
N	127	83	82	103	123	83	111

Notes: See the notes to Online Appendix Table F1. Results include only the oversample of Arizona and New Mexico. Row totals are non-exclusive, with values in the lower half of the table being nested within those values from the upper half of the table.

Online Appendix F: Additional Details and Results from the Names Survey

In addition to fielding the resume survey on Amazon Mechanical Turk, we also fielded a second survey ("names survey"), which was a simpler version of the resume survey. It showed individuals one of the full names from our study and asked them questions about their perceptions of that name, most importantly the perceived race. This allowed us to focus more data collection on the saliency of our name signals. Below we list all the questions from this survey and summarize the results from questions about race and national original in more depth.

Names Survey Questions

1. Consider the name [e.g., Emily Adams]. What comes to mind when you think of a person with this name? What characteristics do you think this person might have?
2. What race or ethnicity do you associate with the name [e.g., Emily Adams]? Choose one answer.
    a. American Indian or Alaska Native
    b. Asian
    c. Black or African American
    d. Hispanic/Latino(a)
    e. Native Hawaiian or Pacific Islander
    f. Other
    g. White
3. How confident are you in your answer to Question 2?
4. How likely do you think it is that [e.g., Emily Adams] was born and raised in the United States?
    a. Extremely likely
    b. Somewhat likely
    c. Neither likely nor unlikely
    d. Somewhat unlikely
    e. Extremely unlikely
5. Consider the name [e.g., Daniel Begay]. What comes to mind when you think of a person with this name? What characteristics do you think this person might have?
6. What race or ethnicity do you associate with the name [e.g., Daniel Begay]? Choose one answer.
    a. American Indian or Alaska Native
    b. Asian
    c. Black or African American
    d. Hispanic/Latino(a)
    e. Native Hawaiian or Pacific Islander
    f. Other
    g. White

7. How confident are you in your answer to Question 6?
8. How likely do you think it is that [e.g., Daniel Begay] was born and raised in the United States?
    a. Extremely likely
    b. Somewhat likely
    c. Neither likely nor unlikely
    d. Somewhat unlikely
    e. Extremely unlikely
9. What is your current age?
10. What is your race? (Mark one or more)
11. Are you Spanish, Hispanic, or Latino/a?
12. Which best describes your gender?
13. What is the highest level of education you've completed?
14. Which best describes your annual household income before taxes in 2016?

More Detailed Name Survey Results

Online Appendix Table F1 presents a summary of the survey results for what race individuals think those with white names and Navajo last names are in terms of race. Unsurprisingly, the white names are almost always perceived as white, regardless of which sample is used (92.8% white in the Arizona and New Mexico sample, 91.0% white in the national sample). Perceptions of the Navajo names differ geographically and by the specific name used. The signal ranges from moderately salient (52.4% AIAN, Daniel Begay) to not salient (5.4% AIAN, Sarah Benally) in the Arizona and New Mexico sample, with the average perception across all four Navajo names being 47.5% white and 27.8% AIAN. For the national sample, this was 60.2% white and 9.4% AIAN. Thus, the last name signal of Navajo status was weak, especially in the national sample. These results were similar in the resume survey for resumes where only Navajo last name signals were used.

Online Appendix Table F1 – Racial Perceptions from the Names Survey for White and Navajo Names

Name	Sample	
	AZ + NM (N)	National (N)
Zachary White	92.1% White, 0.0% AIAN (36)	90.8% White, 0.7% AIAN (100)
Emily Adams	100% White, 0.0% AIAN (42)	97.1% White, 0.0% AIAN (104)
Benjamin Miller	94.3% White, 0.0% AIAN (35)	90.0% White, 2.0% AIAN (100)
Grace Baker	84.2% White, 0.0% AIAN (38)	85.9% White, 1.0% AIAN (99)
All White Names	92.8% White, 0.0% AIAN (151)	91.0% White, 0.9% AIAN (403)
Grace Tsosie	41.3% White, 26.7% AIAN (36)	54.1% White, 10.2% AIAN (99)
Daniel Begay	28.6% White, 52.4% AIAN (42)	58.7% White, 11.5% AIAN (104)
Zachary Yazzie	40.0% White, 22.9% AIAN (35)	47.0% White, 12.0% AIAN (100)
Sarah Benally	81.1% White, 5.4% AIAN (37)	81.0% White, 4.0% AIAN (100)
All Navajo Names	47.5% White, 27.8% AIAN (150)	60.2% White, 9.4% AIAN (403)

Notes: Sample sizes are in parentheses. AZ + NM is a separate sample of Arizona and New Mexico residents, only, while the national sample includes no restriction on state of residence. The national sample does not include those from the AZ + NM sample but does include some other individuals from those states.

Online Appendix Table F2 – Nationality Perceptions from the Names Survey: Percent Who Said Individual with Name was "Extremely Likely" or "Very Likely" Born in the United States

Name	Sample	
	AZ + NM (N)	National (N)
Zachary White	100% (36)	96.0% (100)
Emily Adams	100% (42)	95.2% (104)
Benjamin Miller	94.3% (35)	89.0% (100)
Grace Baker	89.5% (38)	88.0% (99)
All White Names	96.0% (151)	92.1% (403)
Grace Tsosie	63.9% (36)	57.0% (99)
Daniel Begay	86.0% (42)	63.5% (104)
Zachary Yazzie	62.9% (35)	59.0% (100)
Sarah Benally	73.7% (37)	80% (100)
All Navajo Names	72.3% (150)	64.8% (403)

Notes: Sample sizes in parenthesis.

Online Appendix Table F3 – Detailed Racial Perception Results from the Names Survey – White Names

Question	All	AZ	NM	AZ + NM	National
What race or ethnicity do you associate with the name Zachary White?					
American Indian or Alaska Native	0.5%	0.0%	0.0%	0.0%	0.7%
Asian	0.4%	0.0%	0.0%	0.0%	0.5%
Black or African American	6.9%	7.2%	5.6%	6.6%	7.0%
Hispanic/Latino(a)	0.5%	1.0%	0.0%	0.7%	0.5%
Native Hawaiian or Pacific Islander	0.2%	0.0%	0.0%	0.0%	0.3%
Other	0.4%	0.0%	1.9%	0.7%	0.3%
White	91.2%	91.8%	92.1%	92.1%	90.8%
N	136	24	12	36	100
What race or ethnicity do you associate with the name Benjamin Miller?					
American Indian or Alaska Native	1.5%	0.0%	0.0%	0.0%	2.0%
Asian	0.0%	0.0%	0.0%	0.0%	0.0%
Black or African American	5.9%	8.3%	0.0%	5.7%	6.0%
Hispanic/Latino(a)	0.7%	0.0%	0.0%	0.0%	1.0%
Native Hawaiian or Pacific Islander	0.7%	0.0%	0.0%	0.0%	1.0%
Other	0.0%	0.0%	0.0%	0.0%	0.0%
White	91.1%	91.7%	100.0%	94.3%	90.0%
N	135	24	11	35	100
What race or ethnicity do you associate with the name Grace Baker?					
American Indian or Alaska Native	0.7%	0.0%	0.0%	0.0%	1.0%
Asian	0.0%	0.0%	0.0%	0.0%	0.0%
Black or African American	13.1%	16.0%	7.7%	13.2%	13.1%
Hispanic/Latino(a)	0.7%	4.0%	0.0%	2.6%	0.0%
Native Hawaiian or Pacific Islander	0.0%	0.0%	0.0%	0.0%	0.0%
Other	0.0%	0.0%	0.0%	0.0%	0.0%
White	85.4%	80.0%	92.3%	84.2%	85.9%
N	137	25	13	38	99
What race or ethnicity do you associate with the name Emily Adams?					
American Indian or Alaska Native	0.0%	0.0%	0.0%	0.0%	0.0%
Asian	0.7%	0.0%	0.0%	0.0%	1.0%
Black or African American	1.4%	0.0%	0.0%	0.0%	1.9%
Hispanic/Latino(a)	0.0%	0.0%	0.0%	0.0%	0.0%
Native Hawaiian or Pacific Islander	0.0%	0.0%	0.0%	0.0%	0.0%
Other	0.0%	0.0%	0.0%	0.0%	0.0%
White	98.0%	100.0%	100.0%	100.0%	97.1%
N	146	24	18	42	104

Notes: Survey was implemented via Amazon Mechanical Turk in the spring of 2018. See description in Online Appendix F for more details.

Online Appendix Table F4 – Detailed Racial Perception Results from the Names Survey – Navajo Names

Question	All	AZ	NM	AZ + NM	National
What race or ethnicity do you associate with the name Daniel Begay?					
American Indian or Alaska Native	23.3%	33.3%	77.8%	52.4%	11.5%
Asian	4.8%	8.3%	0.0%	4.8%	4.8%
Black or African American	7.5%	0.0%	0.0%	0.0%	10.6%
Hispanic/Latino(a)	6.9%	4.2%	5.6%	4.8%	7.7%
Native Hawaiian or Pacific Islander	1.4%	0.0%	5.6%	2.4%	1.0%
Other	6.2%	12.5%	0.0%	7.1%	5.8%
White	50.0%	41.7%	11.1%	28.6%	58.7%
N	146	24	18	42	104
What race or ethnicity do you associate with the name Zachary Yazzie?					
American Indian or Alaska Native	14.8%	8.3%	54.6%	22.9%	12.0%
Asian	3.7%	8.3%	0.0%	5.7%	3.0%
Black or African American	10.4%	4.2%	0.0%	2.9%	13.0%
Hispanic/Latino(a)	4.4%	4.2%	0.0%	2.9%	5.0%
Native Hawaiian or Pacific Islander	5.2%	0.0%	9.1%	2.9%	6.0%
Other	16.3%	25.0%	18.2%	22.9%	14.0%
White	45.2%	50.0%	18.2%	40.0%	47.0%
N	135	24	11	35	100
What race or ethnicity do you associate with the name Grace Tsosie?					
American Indian or Alaska Native	14.7%	16.7%	44.4%	26.7%	10.2%
Asian	8.0%	10.4%	3.7%	8.0%	7.9%
Black or African American	8.5%	4.2%	3.7%	4.0%	10.2%
Hispanic/Latino(a)	4.5%	3.1%	1.9%	2.7%	5.2%
Native Hawaiian or Pacific Islander	3.6%	2.1%	5.6%	3.3%	3.7%
Other	10.1%	14.6%	13.0%	14.0%	8.7%
White	50.6%	49.0%	27.8%	41.3%	54.1%
N	135	24	12	36	99
What race or ethnicity do you associate with the name Sarah Benally?					
American Indian or Alaska Native	4.4%	4.2%	7.7%	5.4%	4.0%
Asian	2.2%	8.3%	0.0%	5.4%	1.0%
Black or African American	2.9%	0.0%	0.0%	0.0%	4.0%
Hispanic/Latino(a)	1.5%	0.0%	0.0%	0.0%	2.0%
Native Hawaiian or Pacific Islander	0.7%	0.0%	0.0%	0.0%	1.0%
Other	7.3%	4.2%	15.4%	8.1%	7.0%
White	81.0%	83.3%	76.9%	81.1%	81.0%
N	137	24	13	37	100

Notes: See the notes to Online Appendix Table F1.

Online Appendix Table F5 – Detailed Nationality Perception Results from the Names Survey – White Names

Question	All	AZ	NM	AZ + NM	National
How likely do you think it is that Zachary White was born and raised in the United States?					
Extremely likely	69.1%	62.5%	83.3%	69.4%	69.0%
Somewhat likely	27.9%	37.5%	16.7%	30.6%	27.0%
Neither likely nor unlikely	2.9%	0.0%	0.0%	0.0%	4.0%
Somewhat unlikely	0.0%	0.0%	0.0%	0.0%	0.0%
Extremely unlikely	0.0%	0.0%	0.0%	0.0%	0.0%
N	136	24	12	36	100
How likely do you think it is that Emily Adams was born and raised in the United States?					
Extremely likely	66.0%	56.0%	61.1%	58.1%	69.2%
Somewhat likely	30.6%	44.0%	38.9%	41.9%	26.0%
Neither likely nor unlikely	2.0%	0.0%	0.0%	0.0%	2.9%
Somewhat unlikely	1.4%	0.0%	0.0%	0.0%	1.9%
Extremely unlikely	0.0%	0.0%	0.0%	0.0%	0.0%
N	147	25	18	43	104
How likely do you think it is that Grace Baker was born and raised in the United States?					
Extremely likely	63.0%	64.0%	61.5%	63.2%	63.0%
Somewhat likely	25.4%	24.0%	30.8%	26.3%	25.0%
Neither likely nor unlikely	9.4%	12.0%	7.7%	10.5%	9.0%
Somewhat unlikely	2.2%	0.0%	0.0%	0.0%	3.0%
Extremely unlikely	0.0%	0.0%	0.0%	0.0%	0.0%
N	138	25	13	38	100
How likely do you think it is that Benjamin Miller was born and raised in the United States?					
Extremely likely	57.0%	66.7%	54.6%	62.9%	55.0%
Somewhat likely	33.3%	29.2%	36.4%	31.4%	34.0%
Neither likely nor unlikely	5.2%	4.2%	9.1%	5.7%	5.0%
Somewhat unlikely	4.4%	0.0%	0.0%	0.0%	6.0%
Extremely unlikely	0.0%	0.0%	0.0%	0.0%	0.0%
N	135	24	11	35	100

Notes: See the notes to Online Appendix Table F1.

Online Appendix Table F6 – Detailed Nationality Perception Results from the Names Survey – Navajo Names

Question	All	AZ	NM	AZ + NM	National
How likely do you think it is that Grace Tsosie was born and raised in the United States?					
Extremely likely	16.9%	16.7%	41.7%	25.0%	14.0%
Somewhat likely	41.9%	45.8%	25.0%	38.9%	43.0%
Neither likely nor unlikely	17.7%	8.3%	16.7%	11.1%	20.0%
Somewhat unlikely	16.9%	20.8%	16.7%	19.4%	16.0%
Extremely unlikely	6.6%	8.3%	0.0%	5.6%	7.0%
N	136	24	12	36	100
How likely do you think it is that Daniel Begay was born and raised in the United States?					
Extremely likely	32.0%	32.0%	66.7%	46.5%	26.0%
Somewhat likely	38.1%	48.0%	27.8%	39.5%	37.5%
Neither likely nor unlikely	15.7%	20.0%	5.6%	14.0%	16.4%
Somewhat unlikely	12.9%	0.0%	0.0%	0.0%	18.3%
Extremely unlikely	1.4%	0.0%	0.0%	0.0%	1.9%
N	147	25	18	43	104
How likely do you think it is that Zachary Yazzie was born and raised in the United States?					
Extremely likely	17.8%	8.3%	63.6%	25.7%	15.0%
Somewhat likely	42.2%	41.7%	27.3%	37.1%	44.0%
Neither likely nor unlikely	17.0%	20.8%	9.1%	17.1%	17.0%
Somewhat unlikely	19.3%	29.2%	0.0%	20.0%	19.0%
Extremely unlikely	3.7%	0.0%	0.0%	0.0%	5.0%
N	135	24	11	35	100
How likely do you think it is that Sarah Benally was born and raised in the United States?					
Extremely likely	36.2%	36.0%	46.2%	39.5%	35.0%
Somewhat likely	42.0%	40.0%	23.1%	34.2%	45.0%
Neither likely nor unlikely	17.4%	16.0%	30.8%	21.1%	16.0%
Somewhat unlikely	4.4%	8.0%	0.0%	5.3%	4.0%
Extremely unlikely	0.0%	0.0%	0.0%	0.0%	0.0%
N	138	25	13	38	100

Notes: See the notes to Online Appendix Table F1.

Online Appendix G: Secondary Data Analysis of Discrimination

Data Source and Sample Composition

We used data from the Current Population Survey (CPS) (Flood et al., 2015) to measure the unconditional and conditional gaps in economic outcomes between AIAN, NHPI, and white populations. We study disparities in log hourly wages, unemployment rates, and unemployment duration in weeks. We pooled data for the years 2010 to 2017 and we restricted the sample to individuals of age 25 to 64 of any gender. We also estimated results using some restricted samples that more closely match our experiment. These results were similar and are available upon request.

Coding Race

We code individuals as either (1) AIAN alone (NHPI alone), meaning they only report being AIAN (NHIP), or (2) AIAN alone and in combination (NHPI alone or in combination) which is a broader group that includes anyone who reports being AIAN (NHPI) in combination with other races. The main paper presents results for AIAN alone (NHPI alone). We present the full results below, which includes using AIAN (NHPI) alone and in combination. These results are similar. In all cases, we compare these Indigenous groups to non-Hispanic whites, who report being white only.

Measuring Economic Outcomes

To measure gaps in wages and earnings, we calculated the hourly wage for each individual. We calculated the hourly wage by setting it equal to the reported hourly wage if the individual was paid on an hourly basis or equal to weekly earnings divided by usual hours worked per week, if the individual was not paid on an hourly basis. We also measured differences in unemployment rates and unemployment duration, in weeks. Individuals were coded as unemployed if they were designated as "Unemployed," "Unemployed, experienced worker," or "Unemployed, new worker," and as not unemployed if they were designated as "At work" or "Has job, not at work last week." Duration of unemployment is measured as consecutive weeks unemployed or without a job and seeking work.

Oaxaca-Blinder Decomposition

We decomposed our outcome variables following an Oaxaca-Blinder decomposition (Oaxaca and Ransom 1994). Our description of this strategy mirrors (Feir 2013). Our estimating equation is:

$$Ln(Wage^0) - Ln(Wage^1) = \beta_0 (X_0 – X_1)' + (\beta_0 - \beta_1)X'_0, \qquad [G1]$$

where the superscript and subscript 0 signifies Indigenous workers while the superscript and subscript 1 signifies white workers, the X's represent productive characteristics for each respective group, and the βs represent the rates of return to the productive characteristics for each group. This equation comes from taking the difference between the expectation of log wages for each group:

$$E[Ln(Wage_i^0) = \beta_0 X'_{0i} + \varepsilon_{0i}] – E[Ln(Wage_i^1) = \beta_1 X'_{1i} + \varepsilon_{1i}], \qquad [G2]$$

where variables and estimators are the same as above with i additionally indexing the individual. The term $\beta_0 X_1$ is subtracted and added, and the entire equation is rearranged to obtain Equation G1.

The term $\beta_0(X_0 – X_1)'$ is the explained part of the wage differential while the term $(\beta_0 - \beta_1)X'_0$ is the unexplained part of the wage differential. The variables in X_0 and X_1 include: location (indicator variables for each state), marital status (indicator variables for each type of status including married with or without spouse present, separated, divorced, never married, widowed), occupation (indicator variables for each category, harmonized to 2010 variables), education (indicator variables for each highest grade, or range of grades, attained), whether the individual is Hispanic, age and age squared terms, indicators for the number of children, whether the individual is female, experience (indicator variables for minimum expected years of experience), indicators for month and year combinations, and whether the individual lives in metro or non-metro location.

Results

We present the more detailed results in Online Appendix Tables G1 through G4, with a summary of these results in the main paper (Tables 11 and 12).

Online Appendix Table G1 – Summary Statistics for Highest Educational Attainment, by Race

Outcome Variable	AIAN Alone	AIAN Alone or In Part	NHPI Alone	NHPI Alone or in Part	Non-Hispanic White Alone
Less Than High School Graduate	17.3%	15.1%	9.7%	8.3%	5.2%
High School Graduate	35.8%	33.3%	37.8%	38.4%	27.5%
Attended Some College	31.5%	33.3%	30.2%	30.6%	29.0%
College Graduate	10.4%	12.4%	16.0%	16.1%	24.5%
Masters Graduate	3.8%	4.6%	5.1%	5.2%	10.2%
Doctoral Graduate	1.2%	1.4%	1.2%	1.4%	3.6%
N	49,187	79,678	19,121	26,892	2,762,286

Notes: Calculated using IPUMS-CPS data from 2010 to 2017 (Flood et al., 2015). Categories were calculated using the "educ" variable, which encodes multiple levels of highest educational attainment. Those with anything less than a high school diploma or equivalent (which itself was coded as *High School Graduate*) was coded as *Less Than High School Graduate*. Those with any amount of college study short of a bachelor's degree (itself coded as *College Graduate*), including an Associate's degree, was coded as *Attended Some College*. For graduate degrees only completed degrees are coded, and professional school degree (which could include doctoral degrees like JD or MD or professional masters) were coded as *Doctoral Graduate*.

Online Appendix Table G2 – Oaxaca-Blinder Decomposition Estimates – Log Hourly Wage

	AIAN Alone	AIAN Alone or In Part	NHPI Alone	NHPI Alone or in Part
Total Difference	-0.145*** (0.006)	-0.128*** (0.004)	-0.087*** (0.012)	-0.068*** (0.011)
Explained	-0.133*** (0.006)	-0.113*** (0.004)	-0.046*** (0.011)	-0.039*** (0.010)
Occupation	-0.072*** (0.004)	-0.068*** (0.004)	-0.053*** (0.007)	-0.050*** (0.006)
Education	-0.053*** (0.002)	-0.042*** (0.002)	-0.026*** (0.003)	-0.021*** (0.003)
State	0.017*** (0.001)	0.018*** (0.001)	0.049*** (0.003)	0.052*** (0.003)
Hispanic	-0.014*** (0.001)	-0.013*** (0.000)	-0.010* (0.006)	-0.009* (0.005)
Age	-0.010*** (0.001)	-0.010*** (0.001)	-0.018*** (0.004)	-0.020*** (0.005)
Married	-0.006*** (0.000)	-0.006*** (0.000)	-0.002*** (0.001)	-0.004*** (0.001)
Gender	0.005*** (0.001)	0.005*** (0.001)	0.005*** (0.002)	0.004** (0.002)
Metro Status	-0.003*** (0.000)	-0.001*** (0.000)	0.008*** (0.001)	0.007*** (0.001)
Experience	0.003** (0.001)	0.003** (0.001)	0.000 (0.003)	-0.000 (0.004)
Survey Timing	0.001** (0.001)	0.001** (0.000)	0.003*** (0.001)	0.002*** (0.001)
Children	-0.000** (0.000)	-0.000*** (0.000)	-0.000 (0.000)	-0.000 (0.000)
Unexplained	-0.012*** (0.003)	-0.015*** (0.002)	-0.041*** (0.012)	-0.029*** (0.011)
Observations	239,981	242,856	237,105	237,895

Notes: These estimates use data from the outgoing rotation group (ORG) of the IPUMS-CPS monthly data from 2010-2017 (Flood et al., 2015). Statistically significantly different from at 1-percent level (***), 5-percent level (**) or 10-percent level (*). The mean hourly wage for non-Hispanic whites (the comparison group) is $19.13. Hourly wage was generated using the reported hourly wage for those who are paid hourly and are below the censored limit or the calculated hourly wage from weekly earnings divided by the usual working hours. Controls include indicator variables for state, marital status, occupation, education, number of children, sex, metro status, years of experience, month by year, whether the individual is Hispanic, and age and age squared terms, indicators for month and year combinations.

Online Appendix Table G3 – Oaxaca-Blinder Decomposition Estimates – Unemployment

	AIAN Alone	AIAN Alone or In Part	NHPI Alone	NHPI Alone or in Part
Total Difference	0.045*** (0.001)	0.042*** (0.000)	0.017*** (0.001)	0.015*** (0.001)
Explained	0.003*** (0.000)	0.004*** (0.000)	0.010*** (0.001)	0.009*** (0.001)
Hispanic	-0.019*** (0.000)	-0.015*** (0.000)	-0.005*** (0.000)	-0.004*** (0.000)
Occupation	0.013*** (0.000)	0.010*** (0.000)	0.009*** (0.001)	0.008*** (0.001)
Education	0.007*** (0.000)	0.006*** (0.000)	0.004*** (0.000)	0.003*** (0.000)
Married	0.003*** (0.000)	0.003*** (0.000)	0.002*** (0.000)	0.002*** (0.000)
Experience	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
State	0.001*** (0.000)	0.001*** (0.000)	0.002*** (0.000)	0.000*** (0.000)
Age	-0.000*** (0.000)	-0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Survey Timing	-0.000*** (0.000)	-0.000*** (0.000)	-0.000 (0.000)	0.000* (0.000)
Children	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Metro Status	0.000*** (0.000)	0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Gender	-0.000** (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Unexplained	0.043*** (0.000)	0.038*** (0.000)	0.007*** (0.001)	0.006*** (0.001)
Observations	2,186,764	2,208,140	2,167,445	2,173,346

Notes: These estimates use data from the IPUMS-CPS monthly data from 2010-2017 (Flood et al., 2015). Statistically significantly different from at 1-percent level (***), 5-percent level (**) or 10-percent level (*). The unemployment rate for non-Hispanic whites (the comparison group) is 0.037. Controls include indicator variables for state, marital status, occupation, education, number of children, sex, metro status, years of experience, month by year, whether the individual is Hispanic, and age and age squared terms, indicators for month and year combinations.

Online Appendix Table G4 – Oaxaca-Blinder Decomposition Estimates – Unemployment Duration in Weeks

	AIAN Alone	AIAN Alone or In Part	NHPI Alone	NHPI Alone or in Part
Total Difference	-1.705*** (0.502)	0.004 (0.360)	-2.876** (1.383)	-2.315* (1.218)
Explained	-3.313*** (0.263)	-3.573*** (0.201)	0.010 (0.646)	-0.168 (0.563)
Age	-2.744*** (0.173)	-2.483*** (0.138)	-3.461*** (0.344)	-3.361*** (0.298)
Hispanic	-2.466*** (0.120)	-2.490*** (0.093)	0.731* (0.396)	0.181 (0.352)
Education	1.330*** (0.081)	0.958*** (0.064)	0.858*** (0.165)	0.867*** (0.147)
Experience	1.226*** (0.114)	1.065*** (0.088)	1.647*** (0.228)	1.493*** (0.197)
State	-1.086*** (0.081)	-1.064*** (0.066)	0.694*** (0.138)	0.613*** (0.129)
Married	0.503*** (0.080)	0.601*** (0.063)	-0.434*** (0.151)	-0.246* (0.14)
Occupation	0.495*** (0.156)	0.392*** (0.119)	0.068 (0.308)	0.306 (0.280)
Survey Timing	-0.304*** (0.100)	-0.299*** (0.080)	0.151 (0.215)	0.187 (0.189)
Children	-0.282*** (0.035)	-0.235*** (0.025)	-0.295*** (0.058)	-0.292*** (0.051)
Gender	0.088** (0.041)	0.038 (0.034)	-0.209** (0.082)	-0.183** (0.073)
Metro Status	-0.074*** (0.025)	0.038*** (0.034)	0.260*** (0.035)	0.268*** (0.031)
Unexplained	1.609*** (0.410)	3.577*** (0.294)	-2.887** (1.219)	-2.147** (1.070)
Observations	81,543	83,125	79,036	79,263

Notes: See the notes to Online Appendix Table G3. Statistically significantly different from at 1-percent level (***), 5-percent level (**) or 10-percent level (*). The average unemployment duration for non-Hispanic whites (the comparison group) is 30.11.

Online Appendix H: Sample Resumes and Cover Letters

Sample Resume #1 – Type A (Non-Indigenous), Retail Sales

Christopher Johnson
4320 E Pearce Rd
Phoenix, AZ 85044
Phone
Email

Objective To obtain a position as a sales associate.

Work Experience

Sales Associate
Costco, Phoenix, AZ
Oct. 2009 - Present
Assist customers as they shop, answering questions and trying to find the merchandise that fits their needs the best. Straighten up merchandise to ensure a professional appearance. Ring up customers at check out.

Cashier
Walmart, Phoenix, AZ
July 2008 - Sept. 2009
Worked as a cashier and in customer service Primary responsibilities were related to working the cash register, but also assisted with stocking shelves. Occasionally, I checked merchandise for damage and incorrect tags.

Sales Associate
Target, Phoenix, AZ
Nov. 2004 - June 2008
Answer customers' questions. Ring up customers at checkout. Handle returns and other customer service responsibilities. Straighten up merchandise to insure a professional appearance at all times.

Volunteering

Volunteer
Warner A. Gabel Boys & Girls Club, Phoenix, AZ
Mar. 2014 - Present
I assisted kids with homework, played sports with them, and assisted staff in caring for the kids.

Education

High School Diploma
Chandler High School, 2004
Chandler, AZ

References References available upon request.

Sample Cover Letter #1 – Type A (Non-Indigenous), Retail Sales

From: "Christopher Johnson" *Email*
To: *Employer Email*
Subject: Application for *Position*
Attachment: ResumeChristopherJohnson.pdf

Dear Hiring Manager,

My name is Christopher Johnson and I am very interested in your posted job application and I would like to formally apply.

Please see my attached resume

I have significant experience in retail sales through positions at Costco and Walmart. In these positions, I gained significant experience serving customers, promoting products, and resolving customer issues and concerns.

Thank you for your time and considaration. I look forward to hearing from you.

Christopher Johnson
Email
Phone

[Note: This applicant got the randomly-assigned quality feature of a more detailed cover letter (the added paragraph "I have significant experience") but did not get the correction of typos quality feature. The typos, highlighted above, are intentionally added to this resume. All cover letters for applicants that were not given the "no typos" quality feature had one minor typo and one missing period at the end of a sentence.]

Sample Resume #2 – Type B (Native Hawaiian), Language Signal, Server

Emma Lewis
1607 Makiki St., Unit 9
Honolulu, HI 96822
Phone* *Email

Experience

Server
P. F. Chang's, Honolulu, HI
Mar. 2016 - Mar. 2017
Took orders, served food and drinks, managed and cleaned tables, and created a positive atmosphere for guests.

Server
Cheesecake Factory, Honolulu, HI
Feb. 2011 - Dec. 2015
Responsible for ensuring a great guest experience by greeting guests, taking their orders, answering questions, and keeping tables clean.

Server
Benihana, Honolulu, HI
Sept. 2005 - Dec. 2010
Communicated with guests, answered customer menu questions, handled food and drinks, and cleaned tables.

Education

High School Diploma
McKinley High School, Honolulu, HI, 2005

Skills

I speak English and Hawaiian (mother tongues).

Volunteering

Youth Mentor
Big Brothers Big Sisters of Honolulu, Honolulu, HI
Sept. 2013 - Dec. 2016
Mentored kids in my community. Helped them develop social and study skills and community involvement.

References are available on request.

Sample Cover Letter #2 - Type B (Native Hawaiian), Language Signal, Server

From: “Emma Lewis” *Email*
To: *Employer Email*
Subject: Application for *Position*
Attachment: EmmaLewisResume.pdf

Dear Hiring Manager,

My name is Emma Lewis and I am contracting you to respond to your recently posted job ad

I have enclosed my resume.

I am looking forward to hearing from you soon.

Sincerely,

Emma Lewis
Email
Phone

[Note: This applicant did not get the randomly-assigned quality features of a more detailed cover letter or a correction of typos. The typos, highlighted above, are intentionally added to this resume. All cover letters for applicants that were not given the “no typos” quality feature had one minor typo and one missing period at the end of a sentence.]

Sample Resume #3 – Type C (Native American Applicant, Reservation Upbringing) -Plus Language Signal and Occupation-Specific Skills, Cook

Tyler King
2415 Northwest Circle NW
Albuquerque, NM 87104
Phone, *Email*

Experience

Cook
P.F. Chang's, Albuquerque, NM
Apr. 2012 - Mar. 2017

- Cooked and prepared food, followed safety training, and mastered the use of multiple types of kitchen tools.

Cook
Texas Roadhouse, Albuquerque, NM
Feb. 2009 - Feb. 2012

- Cooked food, prepped food, and completed tasks on time and with high quality.

Cashier
Smith's, Albuquerque, NM
July 2005 - Jan. 2009

- I worked at the check out. I scanned items, collected payment, and gave change as appropriate.

Education

High School Diploma, 2005
Navajo Preparatory School
Farmington, Navajo Reservation, NM

Skills

Fluent in English and Navajo (both native languages).

I have received training in food safety.

I have received CPR/AED and First Aid training.

Volunteer Experience

Food Bank Volunteer
Roadrunner Food Bank, Albuquerque, NM
Mar. 2013 - Nov. 2016
I organized food donations and checked for damages and expiration dates.

References available upon request.

Sample Cover Letter #3 - Type C (Native American Applicant, Reservation Upbringing) -

Plus Language Signal and Occupation-Specific Skills, Cook

From: "Tyler King" *Email*
To: *Employer Email*
Subject: *Position* - Tyler King
Attachment: TylerKingResume.pdf

To Whom it May Concern,

My name is Tyler King and I contacting you to respond to your recently posted job ad.

I have enclosed my resume.

To briefly summarize my work history, I gained significant experience as a cook through positions at P.F. Chang's and Texas Roadhouse. In these positions, I learned how to properly prepare a wide variety of foods.

I am looking forward to hearing from you soon.

Sincerely,

Tyler King
Phone
Email

[Note: This applicant got both the randomly-assigned quality feature of a more detailed cover letter (the added paragraph "To briefly summarize…") and the correction of typos quality feature.]

Online Appendix I: References Not Included in the Main Paper

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