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EMPLOYMENT DISCRIMINATION AGAINST INDIGENOUS PEOPLES IN THE UNITED STATES: EVIDENCE FROM A FIELD EXPERIMENT

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ABSTRACT

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 13,516 resumes for common jobs (retail sales, kitchen staff, server, janitor, and security) in 11 cities and compared callback rates. We signaled Indigenous status in one of four different ways. We almost never find any differences in callback rates, regardless of the context. These findings hold after numerous robustness checks, although our checks and discussions raise multiple concerns that are relevant to audit studies generally.

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A data appendix is available at
http://www.nber.org/data-appendix/w25849
A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/2299

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). 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). Poverty rates among those who identify as AIAN alone (NHPI) are nearly double (1.5 times) the rates of those in the general population (U.S. Census Bureau, 2015; WHIAAPI 2010). For NHPIs, this is These disparities are even more substantial for the 22% of AIANs who reside or used to reside on one of the 326 federal

[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.

or state Indian reservations (U.S. Census Bureau 2015; Gitter and Reagan 2002; Taylor and Kalt 2005) or on Alaska Native Statistical Areas (U.S. Census Bureau 2011).[3] 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 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

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

[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., Kuhn and Sweetman 2002; Krishna and Ravi 2011; Feir 2013) and Australia (e.g., Booth, Leigh, and Varganova, 2012). Many discrimination studies 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-correspondence study of our nature for discrimination against Native Americans would be useful (pp. 25).

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 status constant (Neumark 2018; Bertrand and Duflo 2017; 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,

[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).

2004; Lahey, 2008; Neumark, Burn, and Button, 2019) 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 (Collett, Limb, and Shafer 2016), economic conditions are worse (Gitter and Reagan 2002; Taylor and Kalt 2005; Akee and Taylor 2014) (Gitter and Reagan 2002; Taylor and Kalt 2005), and educational quality can be lower (DeVoe, Darling-Churchill, and Snyder, 2008).[10] 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 an 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 discrimination in callbacks against Indigenous Peoples. This holds even when we analyze the data separately by occupation, occupation-and-gender, and by city. Our 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

[10] As noted above, 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). The per capita income of American Indians on reservations is less than half the US average (Akee and Taylor 2014). We provide some details on the prevalence of Native Americans presently living on or near a reservation in Online Appendix Table I1.

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 (Neumark 2012; 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, in most cases, 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 this and previous audit studies more broadly and should be considered by future researchers. We also propose a larger battery of robustness checks that those doing audit studies should consider, regardless of the results of their experiment.

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.[11]

[11] The online appendix is available at http://www.patrickbutton.com/research.

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.

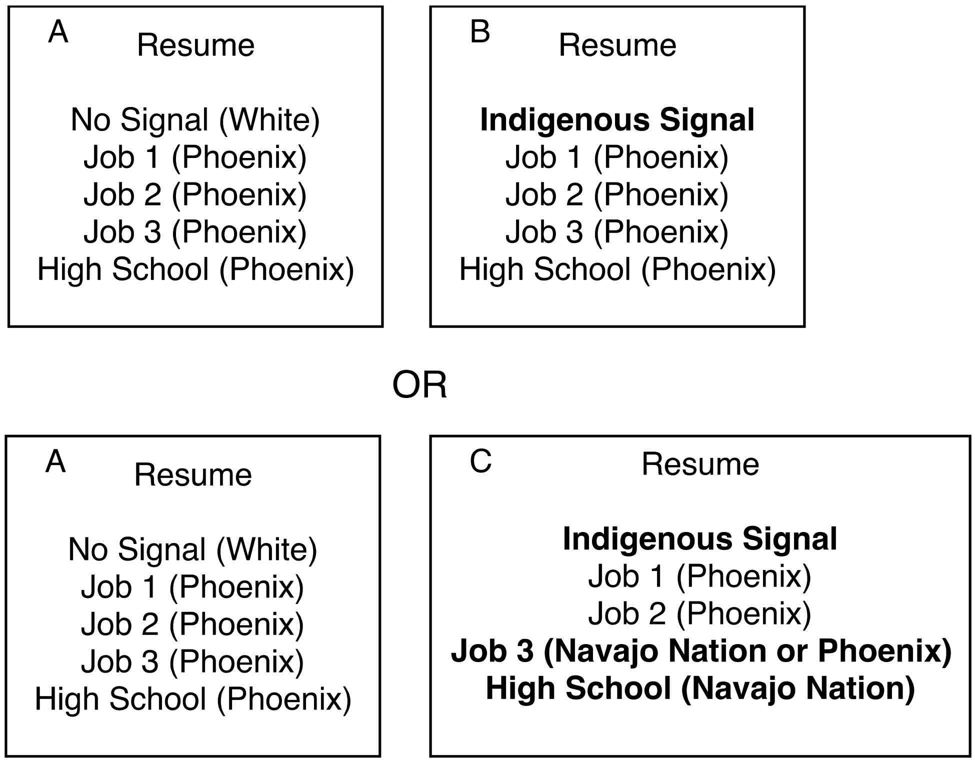
Pre-Analysis Plan

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.[12] 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.[13]

[12] 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).

[13] 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.

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.

Signaling Indigenous Status

Indigenous people in the United States belong to numerous different tribal groups (the federal government recognizes 567 tribal nations National Congress of American Indians (2017)). Consequently, it is not possible to study all tribal groups.[14] Also, 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

[14] Given that we wanted to vary Indian reservation upbringing, for Native American applicants, we selected from tribal nations with reservations for their tribal affiliation. We then selected the tribal nations associated with the reservations we ultimately chose, as discussed later.

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 (Fryer and Levitt 2004; Barlow and Lahey 2018; Gaddis 2017a, 2017b). 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 stronger 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.

Table 1 – Summary of Possible Racial Signals by Indigenous Group

Indigenous Group	Possible Signals of Indigenous Status				Indian Reservation Possible
	Volunteer Experience	Language	First Name	Last Name	
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).

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.[15] 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

[15] However, similarly-constructed resume experiments did not find that the addition of similar volunteer experiences improved callback rates (Neumark, Burn, and Button, 2019).

either Boys & Girls Club or food bank.[16] 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. 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.[17] 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.[18] 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.[19]

[16] 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."

[17] 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.

[18] 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.

[19] 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.[20] 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 only. To determine possible first name, we first considered names within the top 100 baby names from Social Security records for the state of Hawaii in order to get a list of common first names only.[21] We then investigated which of these popular names were Native-Hawaiian, using various sources.[22] We settled on three male names: Kekoa, Ikaika, and Keoni, and one female name: Maile.

[20] 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).
[21] 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).
[22] 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/,

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.[23]

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.[24] 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.[25] These 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

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.

[23] 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.

[24] 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).

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

tropes of Native Americans from popular media (McLaurin 2012; Tan, Fujioka, and Lucht 1997).[26] That said, these sort of names would have been a stronger signal of Indigenous status, an issue what may have affected our results, which we discuss later.

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

[26] A referee brought up a helpful point that testing "stereotypical" names is useful to inform if those names lead to discrimination. The referee mentioned that there has been concern about passing along such names to children for fear that they would face discrimination.

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, 2019, and Neumark, Burn, Button, and Chehras, 2018).[27]

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. We added a high school in a small town to 25% of these white resumes, and then in half of these

[27] 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 also assigned a Job 3 location in that same rural town, mirroring the reservation job.[28] 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.[29] 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).[30]

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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[28] 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).

[29] 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). We note that it would have been useful to study rural areas nearby reservations as well, as discrimination could be more severe there. However, there are not job boards than can be feasibly used in those areas. We had difficulty getting a large enough sample size even in "larger" cities such as Sioux Falls and Billings.

[30] 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). We note that there are numerous reservations that are closer to the cities we test. We decided to focus on more populous reservations, but in an alternative approach we could have focused on reservations that were very close by, even if they were small. The distance between the city and the reservation could matter as could the size of the reservation. Despite these tradeoffs, we do not find discrimination regardless of reservation upbringing.

Occupations

We chose common 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).[31] The most popular occupations differed significantly by gender, and much less by race.[32] Accordingly, we settled on jobs in five broad occupations: retail sales, kitchen staff, server, janitors, and security guards.[33] [34] We used male and female applicants for all occupations except security guard as women infrequently hold that position.

Education

All applicants had a high school diploma only. We focused on this group for a few reasons.

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[31] 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.

[32] 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.

[33] 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, 2019). 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 (2019), 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.

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

First, it is much less common for Indigenous Peoples to have a post-secondary education.[35] 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.[36]

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%	0.0119	0.0020
Grounds maintenance workers 37-3010 (#6)	2.06%	2.36%	2.11%	0.0359	0.0097
Cooks 35-2010 (#9)	1.65%	3.73%	2.51%	0.0707	0.0144
Janitors and building cleaners 31-201X (#10)	1.49%	1.68%	2.00%	0.0355	0.0128
Waiters and waitresses 35-3031 (#24)	0.94%	0.57%	0.08%	0.0189	0.0008
Cashiers 41-2010 (#31)	0.84%	1.26%	0.50%	0.0469	0.0056
Security Guards and Gaming Surveillance Officers (#37)	0.74%	1.44%	2.74%	0.0614	0.0353

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. Ratio to white presents the number of AIAN (NHPI) individuals in the occupation for each white individual. 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%	0.0503	0.0113
Waiters and waitresses 35-3031 (#5)	2.65%	0.80%	0.47%	0.0122	0.0016
Retail salespersons 41-2031 (#8)	2.00%	1.94%	1.50%	0.0391	0.0069
Cooks 35-2010 (#27)	1.00%	1.11%	1.81%	0.0449	0.0167
Bartenders 35-3011 (#34)	0.81%	0.32%	0.86%	0.0161	0.0098
Janitors and building cleaners 31-201X (#38)	0.75%	0.40%	1.03%	0.0217	0.0127

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.

[35] 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.).
[36] 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.

Job Histories

We modeled our resume design and descriptions off 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.[37] 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.[38] Since kitchen staff jobs are very heterogeneous, covering 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).[39]

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.[40] For last names, we randomly assigned one of the last names used in Neumark, Burn, and Button (2019) who used names from Social Security Administration tabulations of popular last names by birth year.

[37] 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.
[38] 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.
[39] 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.
[40] See https://www.ssa.gov/oact/babynames/#andht=1 (accessed May 20, 2016). We borrowed the list of names from Neumark, Burn, and Button (2019).

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 (2019) and Neumark et al. (forthcoming).[41] We assigned each of our applicants a unique email address and one of 88 different phone numbers.[42]

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.

[41] These addresses were selected carefully to ensure that they did not signal a race or ethnicity other than white, did not signal a particular age (e.g., no senior living complexes), 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.

[42] 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.

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 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.

Sample Size

In our pre-analysis plan,[43] 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

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

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).

Identifying Job Ads

We identified viable jobs to apply for using a common job-posting website.[44] 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,[45] 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.[46] 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

[44] We discuss the process that our research assistants followed in detail in Online Appendix A.
[45] 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.
[46] See https://www.thebalance.com/writing-a-letter-of-application-for-employment-2061570 (viewed August 20, 2016).

through a subjective process, we follow others (e.g., Neumark, Burn, and Button, 2019) and treat only positive and ambiguous responses as callbacks, but our results are robust to using 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:[47]

[47] 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.

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

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[48] (the default for all our analysis), and (3) full controls, which includes additional controls[49] on top of the regular controls.

Following Neumark, Burn, and Button (2019), 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, Gelbach, and Miller 2011). The difficulty with clustering on the job is that we cannot match all

[48] 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.

[49] The additional controls included in full controls are graduation year (we randomize between two years), the start month of the oldest job (job 3), the gap (in months) between job 3 and job 2, the gap between job 2 and 1, the duration of the volunteer experience (in months), and indicator variables for the naming structure for the resume, the version of the e-mail script, the formatting of the e-mail, the structure of the subject line in the e-mail, the opening greeting in the e-mail, the structure of the e-mail, the structure of the e-mail signature, the domain of the e-mail address, the voicemail greeting.

responses perfectly to job ads, leading to a restricted sample.[50] 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).[51] 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.[52]

[50] 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. In all, there were only 33 responses that we were unable to match.

[51] This test treats the observations as independent. Our regression analyses that follow clustered our standard errors so as not to assume independence.

[52] The callback rate for white applicants in Anchorage was 24.8%, and this was much lower for whites in the entire sample (19.8%).

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).

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 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.

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 plus the graduation year (we randomize between two years), the start month of the oldest job (job 3), the gap (in months) between job 3 and job 2, the gap between job 2 and 1, the duration of the volunteer experience (in months), and indicator variables for the naming structure for the resume, the version of the e-mail script, the formatting of the e-mail, the structure of the subject line in the e-mail, the opening greeting in the e-mail, the structure of the e-mail, the structure of the e-mail signature, the domain of the e-mail address, the voicemail greeting.

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.[53] For security we see a 1.1 percentage point higher callback rate for Indigenous applicants, but this is again statistically insignificant.

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 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, 2019; Neumark et al. forthcoming), 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%).

[53] These results, available upon request, are similar if Native Americans, Native Hawaiians, and Alaska Natives are analyzed separately.

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.

Effects by City

Table 9 shows results by city. Again, there are largely no differential results.[54] 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.[55]

[54] 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.

[55] 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.

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)).

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:

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

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,[56] and *Gaelic* is an indicator variable for having the Irish Gaelic control language.[57]

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.

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.

[56] 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.

[57] 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.[58]

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%).[59] 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, 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

[58] 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.

[59] 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.

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,[60] 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.

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

[60] 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.

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%).[61] [62] 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).[63]

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 (2018) and Neumark (2018).[64] 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.[65] Of course, our results are not precise enough to rule out discrimination

[61] 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.

[62] 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, that they would be higher for the names survey (see footnote above).

[63] 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.).

[64] The studies with more job applications than us are: Neumark, Burn, and Button, (2019); 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.

[65] 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.

in every circumstance. For example, we cannot rule out discrimination in small cities or town, such as Billings and Sioux Falls, and other comparisons that involves small cells (e.g., Navajo last name signal) are underpowered.

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[66] to the applications. As discussed in Neumark (2012) and Online Appendix C, these quality features 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

[66] 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 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.

combined analysis (all occupations) and each occupation separately.[67] 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, 2019; Booth, Leigh, and Varganova 2012). There are many reasons to believe that callbacks capture a significant share of hiring discrimination. 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, 2019). 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% to 90% of discrimination occurs at the callback stage (Bendick, Brown, and Wall 1999).[68]

On the other hand, the share of discrimination that occurs after the callback phase (at or after the interview) could vary by the minority group and by the saliency of the minority status on the resume. Where minority status is more salient (e.g., gender, commonly-understood race or ethnicity-specific names), then discrimination at this stage is more possible. In sum, we argue that we are capturing a stage where a significant proportion of hiring discrimination (if it exists) usually

[67] 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.
[68] See discussion of International Labor Organization (ILO) studies of ethnic discrimination in Riach and Rich (2002).

occurs, but, like other studies, we cannot claim to capture all of hiring discrimination. Future work on discrimination beyond the callback stage would be useful.[69]

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.[70] 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, 2019) also apply for janitor and security jobs and find some evidence of

[69] For example, a referee shared with us that there is anecdotal evidence that some Native Americans have experienced discrimination at the interview stage depending on their skin color or how "white" they appear.

[70] 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.[71] 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,[72] 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.[73] However, discrimination does not appear to vary by occupation (Tables 7 and 8), suggesting that this concern

[71] Some studies other than Neumark, Burn, and Button (2016, 2019) 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.

[72] 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.

[73] 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).

did not affect our results. A related issue is that typing could vary by city based on the size of the Hispanic population, as certain jobs may be typed as more or less "Hispanic".[74] 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.[75]

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).[76] This percentile range of our 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, although their

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[74] We thank Randall Akee, and others, for raising this helpful point.

[75] These results are available upon request.

[76] 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.

signals of minority status may have been stronger (e.g., criminal records). 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.[77]

While better economic conditions at the time of our study could have made our discrimination estimates smaller, it is not yet clear from the literature to what extent economic cycles affect discrimination in callbacks. We do argue that more work needs to be done to determine how economic cycles affect discrimination, especially considering many studies being case studies of the Great Recession, which may not reflect normal economic times.

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; Krishna and Ravi 2011; Kuhn and Sweetman 2002) by also decomposing gaps in

[77] 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.

unemployment rates and unemployment durations, given that these are more directly related to the callback discrimination we estimate in our field experiment.[78]

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,[79] 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%).[80] 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 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.

[78] 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.

[79] 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).

[80] For comparison, prior studies have found the unexplained gap in log earnings to be 13% for single-ancestry Native Americans and 26.5% for single-ancestry Alaska Natives (Hurst, 1997), and among Aboriginal Peoples in Canada, the unexplained gaps was 11 to 16% in 2005(Feir, 2013).

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.

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.[81] 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 2017). Thus, uncontrolled differences other than discrimination could explain these unexplained gaps. The most relevant uncontrolled difference would be differences in reservation wages.

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

[81] 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.

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.[82] 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), Feir (2013), Kuhn and Sweetman (2002), or Krishna and Ravi (2011), 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 jobs as retail salespersons, servers, kitchen staff, janitors, or security guards show a lack of discrimination at the callback stage, in net, against Indigenous Peoples. We also do not find bias

[82] Specifically, we restricted our sample to individuals in our age range who are high school graduates in the occupations and states that we tested.

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 could be affected by several factors, such as better economic conditions at the time of the experiment (but this is rather uncertain), the ways in which we signal Indigenous status, the saliency of our signals of Indigenous status, or a lack of statistical power in a few circumstances. 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.

We learn quite a bit about audit study methodology from this paper. First, it is unclear to what extent macroeconomic cycles affect discrimination, and thus future work on this topic would help us both understand the mechanisms of discrimination and help us interpret the results of previous work and this paper. Second, we learn from this paper and other recent work (Gaddis 2017a, 2017b; Barlow and Lahey 2018) that it is crucial to test for how salient the signal of minority status is, and to test for how the signal is perceived. Audit studies may not be capturing what the researchers think it is. Third, we propose several additional robustness checks that should be considered, such as weighting. While our lack of results prompted us to dig further into if our results were a true “zero”, we believe these robustness checks are important to conduct regardless of the result.

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, forthcoming), 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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