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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 experiment to measure the discrimination in job hiring faced by Indigenous Peoples in the United States (Native Americans, Alaska Natives, and Native Hawaiians). We created realistic resumes of men and women of about age 30 applying for common entry-level jobs (retail sales, kitchen staff, server, janitor, and security) in 11 cities. We sent employers resumes that either signaled that the applicant was Indigenous or white, with all other resume features the same on average. We compared interview response rates by race to measure hiring discrimination. We further signaled that some of the Native American applicants grew up on an Indian reservation to determine if this increases discrimination. Our preliminary results, based on 9,066 of our expected 13,400 applications, do not show any discrimination. These results are robust to several specifications, in different subsamples (e.g., by city, occupation, gender, signal type) and robustness checks. These preliminary results suggest that the substantial economic disadvantages faced by Indigenous Peoples are attributable to factors other than discrimination, such as education and the negative legacy of colonialism.

JEL Codes: J15, J24, J61, J7

Keywords: Indigenous People, employment discrimination, Native American, American Indian, Alaska Native, Native Hawaiian, Indian reservations, resume study

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Introduction

According to the 2010 Census, 5.2 million people identified as American Indian or Alaska Native (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 rapid growth (Hixson, Hepler, & Kim, 2012).

This growing population faces severe economic challenges and disadvantages. In the United States, 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). In 2015 the mean unemployment duration for those who identify as AIAN only was 24.0 weeks, relative to 17.1 weeks for those who identify as white only[1]. Poverty rates among those who identify as AIAN only are nearly double the rates of those in the general population (26.6% versus 14.7%) (U.S. Census Bureau, 2015). These racial gaps in economic outcomes are less stark for NHPIs, as they have the highest employment-to-population ratio (62.8%), although this reflects a stronger economy in Hawaii[2]. 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) as are mean unemployment durations (24.0 versus 17.1 weeks).

[1] These and the following NHIP mean unemployment duration estimates are based on the authors' calculations using Current Population Survey data for all months in 2015.
[2] In November 2016 Hawaii had the fifth lowest unemployment rate. See https://www.bls.gov/web/laus/laumstrk.htm (accessed Dec. 30, 2016).

These gaps in economic outcomes are attributable to several factors, such as differences in education, geography, and especially the intergenerational legacy of colonialism[3]. Another possible explanation for these racial gaps, especially for unemployment rates and unemployment durations, is discrimination. Anecdotal evidence suggests that Indigenous Peoples face employment discrimination[4,5], but we are only aware of one peer-reviewed study that investigated this in the United States.[6] Hurst (1997) decomposed the AIAN-White earnings gap using an Oaxaca-Blinder decomposition and attributed most of the gap in earnings to differences in characteristics rather than wage structures, suggesting that discrimination may not be a significant factor, although evidence from these decomposition studies is hard to interpret (discussed below).

From a policy perspective, quantifying this discrimination is essential. If there is little discrimination, this suggests that the gap in economic outcomes between Indigenous Peoples and whites is primarily caused by factors other than race, such as differences in education, upbringing, or the negative impacts of colonialism. This suggests that investing in education and poverty reduction would be more useful, as these factors are behind the gaps. However, if there is significant discrimination, then this suggests that supply-side policy measures, such as education[7]

[3] There is research quantifying the lingering negative impacts of colonialism, such as forced relocation, assimilation through boarding schools, and the slaughter of the bison on the Great Plains (Feir 2016a; 2016b; Feir, Gillezeau, & Jones (2017).
[4] See, e.g., https://www.justice.gov/opa/pr/justice-department-sues-south-dakota-state-agency-discrimination-against-native-american-job (accessed May. 1, 2016)
[5] 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, & Robert Wood Johnson Foundation, 2017).
[6] Research on discrimination against Indigenous people is somewhat more common for Canada (e.g., Feir, 2013) and Australia (e.g., Booth, Leigh, and Varganova, 2012). There are many discrimination studies using US data, but they are all on other disadvantaged groups. See Neumark (2016) 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).
[7] For example, the U.S. Department of Education's 2016 Indian Demonstration Project Awards seeks to "...improve the education opportunities and achievement of preschool, elementary, and secondary school Indian children by developing, testing, and demonstrating effective services and programs." See http://www2.ed.gov/about/offices/list/oese/oie/index.html (accessed October 21, 2016).

or skills training, 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 discrimination in hiring against Indigenous Peoples, we conducted a field experiment—more specifically, a resume correspondence study—sending job applicants to job openings. These job applicants are identical on average but are either signaled to be white or Indigenous (Native Americans, Alaska Natives, or Native Hawaiians). Our general approach follows previous studies of this nature (e.g., Bertrand and Mullainathan 2004; Lahey 2008; Tilcsik 2011; Neumark, Burn, and Button forthcoming) by estimating hiring discrimination by comparing interview offer rates for these applicants.

There is no straightforward way to signal Indigenous status. Given this, we carefully construct three different signals of Indigenous status. First, we use a language signal, where the Indigenous applicant mentions that she is a native speaker of an Indigenous language in the skills section of the resume. Second, we follow Tilcsik (2011) and Ameri et al. (forthcoming) and mention minority status in the context of a volunteer experience. In our case, this is listing a volunteer position as a youth mentor with Big Brothers Big Sisters, working with youth "… in my Native American/Alaska Native/Native Hawaiian community." Third, we follow numerous other studies (e.g., Bertrand and Mullainathan 2004) and, in limited cases where it is appropriate, use names to signal Indigenous status for Native Hawaiians (first names) and Native Americans of Navajo origin (last names).

We selected this field experiment to measure discrimination because it would provide the most accurate estimate of discrimination by controlling for all differences other than race. Economists often try to estimate discrimination using survey data. They attempt to explain the gap between advantaged and disadvantaged groups as a function of many factors, such as education or career choices, using an Oaxaca-Blinder decomposition (Oaxaca and Ransom 1994; Hurst 1997). The goal is to make the two groups comparable in as many ways as possible so that the unexplained gap is an estimate of discrimination. However, these studies cannot control for all factors that would affect these gaps, making it difficult to interpret the unexplained gap as discrimination. For this reason, social scientists prefer field experiments that create equivalent job applicants by construction (Neumark, 2016).[8]

One issue with the resume-correspondence design is that it is difficult to measure discrimination when the advantaged and disadvantaged groups differ in some fundamental way, such that they cannot be made identical (e.g., age). In this context, the difference is that some Native Americans grew up on Indian reservations. We consider identical Native American applicants both with and without an upbringing on an Indian reservation for two reasons. First, our discrimination estimates are more population representative if they include both types of applicants. As of 2015, there were 326 federal and state recognized American Indian reservations (U.S. Census Bureau, 2015). Overall, 22% of AIANs have lived in American Indian or Alaska Native Statistical Areas (U.S. Census Bureau, 2011).

Second, quantifying if discrimination differs for Native Americans from Indian Reservations has important policy implications. Native Americans from Indian Reservations frequently migrate off reservations (e.g., Snipp 1997, Pickering, 2000). However, they may face a difficult time transitioning if they face discrimination based on their upbringing on an Indian

[8] That said, these audit studies only measure discrimination for specific labor markets for particular types of applicants during a defined period, so while the evidence avoids more potential sources of bias, these studies cannot comment on discrimination across the entire labor market. We discuss this later when we interpret our results.

Reservation. This may be possible as Indian Reservations have higher levels of poverty[9] and worse schools (e.g., DeVoe, Darling-Churchill, & Snyder, 2008), so firms may statistically discriminate against these applicants.[10] We explore how employers perceive Native Americans from Indian Reservations by creating Native American applicants that either grew up on an Indian reservation (attended a reservation high school and possibly had some work experience on the reservation) or grew up in the local city.

Our preliminary results, based on 9,044 out of our expected 13,400 applicants, show no discrimination. This is contrary to the vast majority of discrimination studies that find discrimination against the minority group (see Neumark, 2016). We find no discrimination regardless of how we cut the data (by occupation, city, gender, race, Indian Reservation upbringing, or by how we signal Indigenous status). Our results also hold up under several robustness checks, including correcting for the variance of unobservables, following Neumark (2012). These results suggest that the significant gaps in employment outcomes between Indigenous Peoples and whites are due to factors other than discrimination, such as education and poverty.

Signaling Race and Indian Reservation Upbringing

Indigenous people in the United States are not a homogenous group, but instead, they belong to numerous different tribal groups (566, as of January 2016[11]). Because of this, there are almost no signals of race that apply to all Indigenous people. Racial signals must be carefully chosen to be appropriate for each tribal group. Signals may also achieve different results. For

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

[10] When breaking the results of the survey above of 342 Native American adults into those living in majority Native areas, this subset believed that they had personally experienced job hiring discrimination 54% of the time versus 22% of the time for Native Americans living in non-majority Native areas (Blendon et al., 2017).

[11] See http://www.loc.gov/catdir/cpso/biaind.pdf (accessed October 30, 2016).

example, names that are more externally valid could also be a weak signal of race or could signal socio-economic status (e.g., Fryer & Levitt, 2004). On the other hand, disclosing race through work or volunteer experience (e.g., Tilcsik, 2011; Ameri et al., forthcoming) may be a strong signal but may be less externally valid.

We used three possible ways to signal that the job applicant is Indigenous: volunteer experience, languages spoken, and names. Not all these signals are appropriate for each Indigenous group. We present our matching of possible signals to Indigenous groups in Table 1. Below, we explain how the previous literature used these types of signals, how we determined which signals were appropriate for each Indigenous group, and how we constructed each signal in this study. We then discuss how these signals are assigned to resumes, providing examples in the Online Appendix.

Volunteer Experience as a Racial Signal

Volunteer or work experience has been used before to signal minority status. It was used to study discrimination against gays and lesbians. Sexual orientation was signaled through volunteer or work experience with a lesbian, gay, bisexual, and transgender (LGBT) or gay or lesbian group (see, e.g., Tilcsik, 2011). Another compelling case of using volunteer experience to signal minority status is Ameri et al. (forthcoming), who add volunteer experience as an accountant at a fictional New Jersey disability group (for Asperger's syndrome or spinal cord injury). We follow a similar approach by using volunteer experience to signal race. We use volunteer experience as a youth mentor with the Big Brothers and Big Sisters of America to signal race. When we use this as a racial signal, we phrased similarly to: “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.”

This experience could be valuable to employers, independent of the racial signal, so this must be controlled. All applicants, regardless of race or signals used, included one volunteer experience on their resume. The white applicant sent to the same employer gets one of two volunteer experiences, randomly chosen: either being a youth mentor with the local Boys and Girls Club or being a volunteer who sorts food at a local food bank. While these experiences are likely of similar quality, it is still possible that employers perceive Big Brothers Big Sisters as a better or worse experience than the Boys and Girls club or the food bank. To deal with this, the Big Brothers Big Sisters experience is randomly assigned to one of the resumes and has similar phrasing, but without the mention of race,[12] for applicant pairs where the Indigenous applicant does not use the volunteer signal. Thus, we can separately identify the effect of the Big Brothers Big Sisters experience. The Online Appendix includes examples of our resumes and presents examples of how these signals appear on the resumes.

Language as a Racial Signal

Surprisingly, we found few typical audit-correspondence studies of discrimination that used language as a signal (one example may be Oreopolous, 2011, to some extent). However, one 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. The American Community Survey codes 169 Indigenous languages, plus Hawaiian and Hawaiian Pidgin. While most Indigenous people primarily speak English, Indigenous-specific languages are somewhat common: 26.8% of AIANs spoke a language other than English at home in 2014,

[12] We phrased this as: "I mentor youth in my community. I work with youth on social skills, academics, and community engagement."

compared to 21.2% nationally (U.S. Census Bureau, 2015). For those who identify as NHPI only, 30.3% of those who identify as NHPI and were born in the US speak a language other than English at home (U.S. Census Bureau, 2014). Since it is rare for individuals without Indigenous ancestry to speak an Indigenous language at home, this makes for a strong racial signal.

We assigned languages to some, but not all, of the tribal groups. To determine which languages are spoken by which tribal groups, we used two approaches. The first was to determine the languages historically spoken by the tribe. The second was to determine which Indigenous languages are 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. Online Appendix Table 1 presents the languages that we selected for each American Indian tribal group and the proportion of individuals who report speaking this language at home and live on the associated reservations. We did not assign a language for individuals from the Osage or Blackfeet tribes since Indigenous language use by this tribe is very low (less than 1% for Osage) or sufficiently uncommon (less than 10% for Blackfeet).

Some issues must be considered in using and interpreting this signal. Having the ability to speak an Indigenous language may be viewed positively by employers, either because the language could be used on the job (but this is rare) or because it is a signal of ability. 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, individuals who learn a second language at home are seen as more productive for other reasons (e.g., they were raised by more active parents). 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 both languages are spoken natively. Thus, it is unclear if this language signal will be seen positively or negatively by employers, net of its effect as a race signal.

To investigate this to some extent, we add a "control" language to 10% of the white resumes. We add the Irish Gaelic language, which is an uncommon language in the United States and one that is unlikely to signal that the applicant might have worse English skills since English is nearly universal in Ireland and the United Kingdom.

First Name as a Racial Signal (Native Hawaiian)

We signal race through first names for Native Hawaiians. We queried the United States Social Security Administration's "Popular Names by State" database for the state of Hawaii.[13] Among the top 100 names for boys born in 1985-1987 (corresponding to around age 30) we use three Native Hawaiian names: Kekoa, Ikaika, and Keoni. Among the top 100 names for girls, we use Maile.[14] We confirmed these names as Native Hawaiian through various sources[15]. When using the first name as a racial signal, we randomly assign one of these names randomly, conditional on gender. We did not use first names to signal race for Alaska Natives or Native Americans because there is little information on first names for these populations. For example, there is no Census or Social Security Administration tabulation of first names by race (Tzioumis, 2015) and the

[13] See https://www.ssa.gov/cgi-bin/namesbystate.cgi (accessed November 8, 2016).

[14] Malia also appeared on this list for girls, but we avoid using this name in case it sends a different signal given that this is the name of the President Obama's daughter. We also did not use Alana since it is also a name of Irish origin. We opted not to use Leilani as there was some evidence that this name is common for those who are not Native Hawaiian as well.

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

information online about Indigenous-specific first names is spotty. Furthermore, no Alaska Native-specific names appear in the Social Security database in Alaska for the years 1985-1987.

Last Name as a Racial Signal (Native American)

While there is limited information on first names by race, there is some data for last names by race. To find Indigenous-specific last names, we use tabulations from the 2000 Census of the racial composition of each last name, for last names that occurred at least 100 times.[16] 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 identify as a particular race. Unfortunately, these data do not include the proportion of AIAN in combination (there is just a "two or more races" category). These data also do not include the proportion who are NHPI, and any Native Hawaiian last names that occur for those who identify as "Asian and Pacific Islander only" are not very common.[17] Thus we are unable to use last names to signal that applicants are Native Hawaiian.

Since AIAN-specific last names are not unusual, we consider them as one of the racial signals that we use in some cases. To determine appropriate last names, we first created a list of 12 AIAN-specific last names that had at least 0.2 people per 100,000 with that last name. We extracted this short list from a more extensive list of 268 last names where at least 80% of the people with those last names identified as AIAN only[18]. Information on the tribal affiliation of most of these names was sparse. Information on Ancestry.com[19], for example, only allowed us to identify four of the as being tribe or language specific, in this case, Navajo. These were Begay (5.96 people per 100,000, 94.98% AIAN only), Yazzie (5.16, 96.10%), Benally (1.87, 95.99%)

[16] See http://www2.census.gov/topics/genealogy/2000surnames/names.zip (accessed June 25, 2016).
[17] For example, one of the more popular Native Hawaiian last names, Kealoha, only had 911 occurrences.
[18] See Online Appendix for more details.
[19] See, e.g., http://www.ancestry.com/name-origin?surname=begay (accessed October 30, 2016).

and Tsosie (1.80, 96.23%). We were also able to confirm from other sources that these four names were Navajo, but it was not possible to verify the origin of the other names with enough certainty.[20]

We also considered the possibility of assigning some Native American last names that were not tribe-specific, but that signal Native American status in perhaps a stereotypical way. However, these names were rare. 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 more stereotypical, they were not sufficiently frequent enough, even in sum, for their use to speak to the experiences of the vast majority of Native American peoples.

Assigning Racial Signals

Table 1 summarizes which of the signals we used for each tribal or Indigenous group. Since we selected the signals for each group such that they are all compatible with each other, it is possible to assign more than one signal. Assigning more than one signal allows us to determine if discrimination increases when we send more than one signal. This is important to quantify as assigning signals of Indian reservation upbringing (described below) may unintentionally strengthen the racial signal.

We randomly added signals as follows. For Navajo and Native Hawaiian applicants, where all 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, where language and volunteer are possible, this is:

[20] Our primary source was http://tribalemployee.blogspot.com/2013/03/navajo-last-names.html (accessed June 25, 2016) which lists several Navajo last names and their meanings. While this list identified other names on our list as being Navajo, we could not sufficiently corroborate this with other sources. We also found many other sources through a web search that confirmed that Begay, Yazzie, Benally, and Tsosie were Navajo.

Language only (40%), Volunteer only (40%), and both (20%). For Osage and Blackfeet, only volunteer is possible.

Indian Reservation Upbringing

We assigned half of the Native American applicants (50% probability) to have grown up on an Indian reservation rather than in the urban center. We signaled this through having graduated from a high school on an Indian reservation, rather than a local high school, and sometimes with the first job after high school being on an Indian reservation instead of in the local city. This reservation signal is only possible for tribes with an associated Indian reservation. We considered seven Indian reservations: Navajo Nation (Navajo tribe), Fort Apache (Apache), San Carlos (Apache), Blackfeet Nation (Blackfoot), Tohono O'odham (Tohono O'odham), Pine Ridge (Oglala Lakota), and Osage Nation (Osage). These fall within the top ten most populous reservations (Norris et al., 2012).

To assign non-reservation high schools, we randomly assigned one of two to four that are local to the city, collected for Neumark, Burn, and Button (forthcoming) and Neumark, Burn, Button, and Chehras (2017).[21] We compiled a list of high schools on Indian reservations and selected one to three high schools per reservation, depending on availability. We specifically chose high schools with names that were a stronger signal that the high school was located on an Indian reservation to ensure that this signal was not weak. We also specify the location of the high school as "City, Reservation Name, State."

For half of the Indigenous applicants with a reservation upbringing, we have 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 have a local job. In addition to possibly strengthening the

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

reservation signal, the 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 if this has any independent effect beyond the location of the high school (reservation or local). Based on resumes posted on Indeed.com, where the resume poster had work experience on an Indian reservation, a typical job that allows for a non-reservation control is a cashier at a grocery store. For pairs of applicants where we sent Native American applicants, we set Job 3 (see Figure 1) for both resumes to be at a grocery store, with the location either on the reservation or in the local city, and keep the subsequent jobs to be in the targeted occupation. Thus, the only change when we include this reservation job is that the location of Job 3 changes, but not how we describe the job.

There are two possible threats to our ability to interpret the difference between the Native American resumes with and without the reservation signals as being the penalty that employers place on those from reservations. The first threat is that employers may simply prefer applicants who are more local, because they can better identify the quality of the high school or because locals may know more about the area, its culture, or may fit in better. Another possibility is a preference for those from urban centers over rural areas, such as reservations. We investigate this by randomly assigning to white resumes, in pairs where we send a Native American resume, an upbringing in a small rural town. We add a high school in a small town in 25% of these cases and then in half of these 25% we also assign a Job 3 location in that same small-town area. We specifically choose these small towns to match with each reservation, such that both the reservation and small towns are about an equal distance from the city (see Online Appendix Table 2).

The second threat is that adding the reservation signals to the resume may increase the likelihood that the employer detects that the applicant is Native American. If there is discrimination, and the applicant's race is more detectable with the added reservation signals, then this could explain a portion of the difference between the local (type B) and reservation (C) Native American applicants. We attempt to control for this by sometimes assigning Indigenous applicants to have more than one racial signal, to see if adding, say, language, conditional on volunteer experience, has any additional effect. If it does, then this may suggest that the addition of the reservation signal is having a similar impact, and our estimated effect of reservation upbringing is an upper bound. As shown later, we do not find any effects of adding additional signals, so this is not much of a concern.

Other Resume and Job Application Details

Pre-Analysis Plan

Before putting this experiment into the field, we filed a pre-analysis plan (PEP) and registered it with the American Economic Association's Randomized Control Trial Registry (socialscienceregistry.org)[22]. The goal was to pre-specify any variables, models, sample sizes, or decisions that could feasibly be data mined without tying our hands too much in ways that negatively affect our ability to conduct this research later (see Olken, 2015, p. 71). We discuss this pre-analysis plan in greater detail in the Online Appendix.

Cities

We focused on cities where more Indigenous Peoples live to get estimates of discrimination that better reflects the experiences of the Indigenous population. We applied for jobs in eight of the ten cities with the most people who identify as American Indian or Alaska Native, either alone or in combination (Norris et al., 2012). These are, in order, New York, Los Angeles, Phoenix,

[22] See https://www.socialscienceregistry.org/trials/2299 (accessed December 26, 2017).

Oklahoma City, Anchorage, Albuquerque, Chicago, and Houston.[23] We then add two other cities, Billings and Sioux Falls. While these cities have fewer AIAN individuals, namely because they are smaller cities, they have a greater share of the population that is AIAN. This provides some variation in the proportion of AIAN individuals in each city, with several cities (New York, Los Angeles, Chicago, and Houston) having a small share of the population that is AIAN. Billings and Sioux Falls are also notable because these cities are near a few Indian reservations of interest (e.g., Pine Ridge).

Native Hawaiians and Other Pacific Islanders (NHPI) are categorized as a separate racial category. We apply for jobs, at least in part, using applicants who are NHPI in the following cities: Honolulu, New York, Los Angeles, New York, Chicago, and Houston. Honolulu is added since Honolulu county has the most NHPI individuals, followed by Hawaii County and Los Angeles County (Hixson et al., 2012).

Occupations

Given the constraints on a resume correspondence study, we targeted jobs where there were many posted online that were reasonably entry-level or low-skilled and often allowed applications by email. We were also interested in applying for jobs that are common for applicants of the age we use (age 29-31). We were also interested in these common occupations that skew more towards entry-level or lower-skilled as discrimination is more likely in lower-skilled positions (Helleseter, Kuhn, & Shen, 2014; Kuhn & Shen, 2013), and the impacts of discrimination are likely more significant for lower-skilled applicants who are more likely to face poverty.

To determine how frequent occupations were by race and gender, we used data from all months of the Current Population Survey in 2015. To ensure a reasonable sample size, we used an

[23] We exclude Tulsa (rank of 6) since it is similar to Oklahoma City (rank of 4), and we opted for Houston (rank of 9) instead of San Antonio (rank of 10).

age range of 25 to 35 to correspond to individuals of age 29 to 31 that we use in this study. Tables 2 and 3 contain ranked lists of the most popular occupations for white men and white women (white here being "white only"), AIAN men, AIAN women, NHPI men, and NHPI women (where individuals are deemed AIAN or NHPI if they report that race either in whole or in part).

Unsurprisingly, the common occupations differ by gender. Of the 38 most popular occupations for white men (Table 2) and white women (Table 3), only 13 appear on both lists. For this reason, we compare races separately by gender. There is more overlap for race. 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 (Table 2). This is 27 (23) for AIAN women (NHPI women), compared to the list of 38 for white women (Table 3). Based on this analysis and the jobs that would fit given the constraints of a resume correspondence study, we settled on entry-level jobs in five occupations: retail sales, kitchen staff, server, janitors, and security guards.[24] Since security guard is not common among women, we only used male applicants for security. However, we use applicants of either gender for the other occupations.

Employer job advertisements are not categorized the same way as the Census Bureau classifies occupations. We grouped the highlighted occupations from Tables 2 and 3 into five larger groupings, for which we use common resumes. These are retail sales (corresponding to retail salespersons and cashiers in the Census occupational classification), kitchen staff (cooks, food preparation workers, dishwashers), server (waiters and waitresses), janitors (janitors and building cleaners), and security guards (security guards and gaming surveillance officers).

[24] We note that we could have used other occupations as well. We chose security instead of drivers since we already had the inputs to make these resumes from a previous study (Neumark, Burn, and Button, forthcoming), and we thought that security was interesting given that the relatively higher concentration of Indigenous men in security. We also opted for server and kitchen staff over customer service even though customer service was equally prevalent. While we could have applied for administrative and secretarial positions as in Neumark, Burn, and Button (forthcoming), we decided to avoid doing so since the applications to these jobs in the previous study elicited a large number of spam responses that made data collection less accurate and more time-consuming.

For retail sales, the two separate occupations (retail sales, cashier) that fell under this require slightly different skills and experience. However, job advertisements in retail and for cashiers have a significant amount of overlap, with the main distinguishing feature being how often the worker works at a cashier station relative to other duties (e.g., stocking shelves, helping customers select products). From actual resumes we viewed on Indeed.com, we say that individuals who list experience as a cashier also often have experience in retail sales and vice versa. We followed the same approach as in Neumark, Burn, and Button (forthcoming) to construct the resumes for retail sales, janitors, and security, but we created resumes for kitchen staff and server separately for this study, using the same approach.

For kitchen staff, the jobs posted varied in required duties and experience, but were generally for cooks or entry-level kitchen staff who did food preparation, dishwashing, or worked in a fast-food setting. We created separate resumes for the cook positions (where the applicants had experience as a cook) and for all other positions (where the applicant had experience as a dishwasher or in fast food). In reading the job advertisements, the research assistants decided which type of resumes was more appropriate to send. Thus, we attempted to tailor the resumes to the job positions to improve external validity as, for example, it would be odd for an applicant with significant experience as a cook to apply for a dishwasher position.[25]

Job Histories

We used real resumes posted on Indeed.com as a guide for how to construct the job histories and job descriptions on our resumes. This improved the external validity of our experiment, as our resumes closely matched actual resumes of job seekers. We included three jobs in each job history section, with this work experience being nearly continuous from high school

[25] We pool the cook and entry-level kitchen staff results together into a “kitchen” occupation, but our results are unchanged when we analyze these separately.

graduation to near the present day. We randomly assigned 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 businesses, such as national or regional chains that we have confirmed operated in that location at the time that they were listed. We used data from actual resumes posted on Indeed.com to adapt how we describe these jobs, or similar jobs, on our resumes. For retail sales, janitor, and security, we borrowed the resume information from Neumark, Burn, and Button (forthcoming) and Neumark, Burn, Button, and Chehras (2017). We supplemented this with additional employers in our cities. For server and kitchen staff, we collected this information ourselves using a similar process to that in the above-referenced studies. We randomly generated tenure distribution at these jobs, conditional on all three jobs covering the period from high school graduation to near the present.

For the most recent job, we assigned all applicants within each set sent to a job opening as either all employed (the most recent job end date listed as "Present"), or all unemployed, with 50% probability for each. When applicants are unemployed, the resumes indicated that their last job ended in the month before the job application. During the field experiment, every month we move the ending date of the most recent job forward one month, so that unemployment durations did not lengthen during the experiment. 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.

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 used first names that were common for those of this age based on common baby names taken from Social Security data[26] (borrowed from

[26] See https://www.ssa.gov/oact/babynames/#andht=1 (accessed May 20, 2016).

Neumark, Burn, and Button, forthcoming). For last name, we randomly assigned one of the last names used in Neumark, Burn, and Button (forthcoming) which were taken from Social Security Administration tabulations of popular last names by birth year.

Residential Addresses

Within each set of applications sent in response to an ad, all applications are from different residential addresses, which are randomly assigned. We used addresses from Neumark, Burn, and Button (forthcoming) and Neumark, Burn, Button, and Chehras (2017). These addresses were selected carefully to ensure that they did not signal a race other than white and were not likely to send an unusual signal (positive or negative) about the socio-economic status of the applicant, and weren't too far from the central business district(s) in the metro areas.

Phone Numbers

We purchased online phone numbers for our applicants from the company *Vumber*. These appear the same as regular phone numbers but have the benefit that they do not require any physical phones and all the voicemail recordings get sent to a central account. We gave each phone number a typical and generic voicemail greeting that instructs the caller to leave a detailed message after the tone. When employers called, they did not always leave a message that provides enough information to match them to an exact applicant (let alone job ad). Assigning a unique phone number to every job applicant and job ad would solve this problem, but is too expensive. 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 results in 88 unique phone numbers. With all of these numbers and other matching methods (discussed in the Online Appendix), it was highly unlikely that we could not assign a response to an applicant.

Email Addresses

We also needed email addresses for our respondents. Because some of the common email providers have Terms of Service agreements that do not permit the creation of email addresses for fictitious persons, and because we wanted complete control of the email addresses, we purchased our own domain names and used them to create email addresses. We renewed the domain names used in Neumark, Burn, and Button (forthcoming) so that we could use email addresses with different domain names for the applications in each set that we sent out. With our domains, we can create unlimited email addresses, so the email addresses we use are unique to each applicant.

Grouping Resumes to Send to Job Ads

After creating the final resumes, we combined them into pairs that go out in response to each job for which we apply. If we choose to apply to a job with Native American applicants, we send the one white applicant (type A) and either a Native American applicant without (B) or with (C) the reservation signal, with 50% probability each. We show this in Figure 1. The type of Indigenous applicants we send depends on the city in which we apply. Table 4 presents our allocations. Anchorage jobs only get Indigenous applicants that are Alaska Natives, and Honolulu only gets Native Hawaiian applicants. Billings, Oklahoma City, and Sioux Falls only get Native American applicants who are Blackfoot, Osage, and Oglala Lakota, respectively.

We matched Albuquerque and Phoenix with more than one Native American tribal group due to their proximity to several notable Indian reservations. For Albuquerque, Native American applicants are chosen with 60% probability to be Navajo and with 40% probability to be Apache. For Phoenix, Native American applicants are selected with 40% probability each to be Tohono O'odham and Navajo, and with 20% probability to be Apache.

We sent Native American applicants of all the chosen tribes to job ads in Chicago, New

York, and Houston. For these cities, we randomly selected the tribal group as follows: Navajo (25%), or Apache, Tohono O'odham, Osage, Blackfoot, or Oglala Lakota (15% each). Since the NHPI population is relatively high in Los Angeles[27] (not in any other mainland city used in the study), we apply with white (A) and Native Hawaiian (B) applicants for 25% of the jobs and with (A) and Native American (B or C) applicants for the remaining 75% (with the same probabilities as above for each tribal group). It is also unlikely for Alaska Natives to live outside of Alaska, so we only send Alaska Native applicants to jobs in Anchorage.

Other resume characteristics were randomly assigned without replacement, such that all resumes have something slightly different to ensure that the resumes do not appear too similar. The characteristics that are randomized in this way are 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 that is listed. This randomization without replacement helps ensure that none of the resumes look too similar, such that employers would not assume that they were somehow related. For example, it might be odd for two resumes received by an employer to have the same resume formatting, the same language describing skills, or the same volunteer experience. All other resume characteristics are randomized with replacement.

Collecting Data

Identifying Job Ads

We identified viable jobs using a common job-posting website.[28] The primary requirements for the jobs were that they must be entry level (e.g., not managers or supervisors), fit the correct

[27], Los Angeles County has the largest population of NHPIs outside of Hawaii (62,487 alone or in combination) (Hixson, Helper, and Kim, 2012).

[28] We discuss the process that our research assistants followed in detail in our Online Appendix.

job description (e.g., retail sales, but not wholesale or merchandizing), did not require in-person applications or inquiries by phone, and did not require the applicant to go to an external website to apply.[29] We ignored job ads that specifically required additional documents that we did not prepare (e.g., head shots, salary history), required skills[30] or education that our resumes did not have.

Sample Size

A vital aspect of this plan was to conduct a power analysis based on previous studies to determine how many observations would be necessary to detect meaningful differences in callback rates between major resume types. Based on previous studies, we saw differences of about three percentage points in the interview request rate to be likely[31], and we wanted to be able to detect a difference of at least this magnitude between white and Indigenous applicants. Based on our calculations, we anticipated needing to apply to 4,211 jobs (8,422 applicants) to detect differences in callback rates between white and Indigenous applicants of at least three percentage points.[32]

[29] Large companies often contract out with external human resources firms to recruit candidates. Other companies such as Walmart, Best Buy, and Target will only accept applications on their websites (Neumark, Burn, and Button, forthcoming).

[30] We also ignored job ads that required a quality element 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 the Online Appendix for more detail.

[31] Bertrand and Mullainathan (2004) had approximately 5,000 observations for four types of applicants, differences in callback rates of 0.03 as statistically significant (their standard errors were 0.01). Neumark, Burn, and Button (forthcoming), which shares some similarities to this study regarding resume construction, had 40,223 observations for eight types and were able to detect similar differences of 0.027, with standard errors of 0.006. Using a restricted sample of just men in sales (5,348 observations), they were able to detect differences of 0.038 (standard error of 0.020) between groups. Lahey (2008) was able to detect even smaller differences (0.016) as statistically significant, with almost 5,000 observations (split between Sarasota area and Boston area, analyzed separately) and two groups (young and old).

[32] In Neumark, Burn, and Button (forthcoming), the average interview rate for younger (white) applicants in retail sales was 24.79%, or 24.28% for security, and 32.08% for janitors. Since we use similar resumes for these applications and a similar application process as in this study, we see a weighted combination of these rates (25.43%, weighted by the number of job ads in that study) as a reasonable approximation to the interview rate we will receive for our white applicants. To detect a three percentage point difference using an exact Fisher two-tailed test requires 3,239 observations per group, given the common values of $\alpha = 0.05$, and $\beta = 0.8$ (Faul, Erdfelder, Lang, and Buchner, 2007). However, this calculation does not take into consideration the inter-correlation between clusters (ICC) that occurs when applications are sent in sets to employers. The process to adjust the sample size given this is outlined in Lahey and Beasley (2016). Using a more liberal (higher) estimate of the inter-correlation of 0.3, this

We ultimately decided to collect more data than this to be able to have a higher power[33], detect differences smaller than three percentage points[34], or to detect other mediators of discrimination (e.g., reservation upbringing, city demographics, gender, occupation) with enough precision.

Emailing Applications / Cover Letters

In our email responses ("cover letters") to the job posting, each application within a set uses a different subject line, opening, body, closing, and signature order. We based some of these scripts on examples and advice articles by job search experts.[35] Differentiating our cover letter scripts further ensures that applicants from the same set are not perceived as related by the employer. We assumed that cover letters, which we pasted into email body, satisfied employers' requests to include a cover letter (although this request was rare). This is not a strong assumption since we write these emails to be very similar to cover letters, and it is common practice for the cover letter to be sent as the body of the email when an application is submitted via email, at least for entry-level positions.

Coding Employer Responses

We coded each employer response as an unambiguous positive response (e.g. "Please call to schedule an interview"), an ambiguous response (e.g. "Please call us back as we have a few questions for you"), or an unambiguous negative response (e.g. "Thank you for your application, but we have filled the position"). To avoid having to classify the ambiguous responses through a subjective process, we treat them all as callbacks, and we code the negative responses the same as no callbacks.

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suggests that if employers are sent two applicants for each job ad, then the required sample size is 1.3 times the earlier estimate (4,211 jobs).

[33] With a power level of 0.9, the required number of observations becomes 23,703.

[34] To detect differences of at least two percentage points, we need 40,351 observations.

[35] See https://www.thebalance.com/writing-a-letter-of-application-for-employment-2061570 (viewed August 20, 2016).

Methodology

Callback Rates by Indigenous Status and Indian Reservation Upbringing

We start our analysis by analyzing callback rates by race, without any regression controls, as it is common to present the raw data first. For this analysis, we compute raw callback rates by race and use an exact Fisher test (two-sided) to test if callback differences are statistically significantly different by race. First, we pool all Indigenous groups together to test for a difference between white and Indigenous applicants. Then we compare Native American, Alaska Native, and Native Hawaiians separately.

We then move to a probit model and control for other resume features to improve precision and to test sensitivity of the results to the inclusion of control variables. We also investigate if discrimination against Native Americans differs if they have an upbringing on an Indian Reservation. Our probit regression is:

$$\begin{aligned} Callback_i = \ & \Phi[\beta_0 + \beta_1 NA_i + \beta_2 Reservation_i + \beta_3 Reservation\,Job_i + \beta_4 AN_i \\ & + \beta_5 NH_i + \beta_6 Rural_i + \beta_7 Rural\,Job_i + Controls_i \beta_8 + \varepsilon_i)] \end{aligned} \quad [3]$$

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 growing up in a rural area (added to some of the white applicants), and *Rural Job* is an indicator variable for growing up in a rural area and having the oldest job listed as being in that rural area. White is the excluded racial category, so all estimates reflect callback differences relative to white applicants.

Controls is a vector of resume controls. We consider three versions: (1) no resume controls (to match the raw tabulations), (2) regular controls (the default), and (3) full controls, which includes additional controls on top of the regular controls. 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. The additional controls included in full controls are graduation year (we randomize between two years), resume naming style, e-mail script version, e-mail format, e-mail subject, e-mail opening line, e-mail body, e-mail signature format, e-mail domain, voicemail greeting, oldest job (job 3) start month, gap (in months) between job 3 and job 2, gap between job 2 and 1, and duration of volunteer experience (in months).

Following Neumark, Burn, and Button (forthcoming), we present all analysis using clustering on 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 on the resume (Cameron, Gelbach, & Miller, 2011). The difficulty with clustering on job is that we cannot match all responses perfectly to job ads. However, we will show estimates with clustering on job and multi-way clustering on job and resume once we complete coding all the job observations. As in Neumark, Burn, and Button (forthcoming), we expect similar results, and if anything, standard errors could also be slightly smaller.

Callback Rates by Occupation and Gender

Next, we analyze callback rates separately by occupations as follows:

$$
\begin{aligned}
Callback_i = \ \Phi[\beta_0 &+ \beta_1 Indigenous_i \times Retail_i + \beta_2 Indigenous_i \times Server_i \\
&+ \beta_3 Indigenous_i \times Kitchen_i + \beta_4 Indigenous_i \times Janitor_i \\
&+ \beta_5 Indigenous_i \times Security_i + Controls_i \beta_6 + \varepsilon_i)]
\end{aligned} \tag{4}
$$

Since gender discrimination differs by occupation, we re-run the regressions above with interactions between *Female* and each occupation, and *Female* and the above *Indigenous* and occupation interactions, as follows:

$$
\begin{aligned}
Callback_i = \ \Phi[\beta_0 &+ \beta_1 Indigenous_i \times Retail_i + \beta_2 Indigenous_i \times Server_i \\
&+ \beta_3 Indigenous_i \times Kitchen_i + \beta_4 Indigenous_i \times Janitor_i \\
&+ \beta_5 Indigenous_i \times Security_i + \beta_6 Female_i \times Retail_i \\
&+ \beta_7 Female_i \times Server_i + \beta_8 Female_i \times Kitchen_i \\
&+ \beta_9 Female_i \times Janitor_i + \beta_{10} Indigenous_i \times Retail_i \times Female_i \\
&+ \beta_{11} Indigenous_i \times Server_i \times Female_i \\
&+ \beta_{12} Indigenous_i \times Kitchen_i \times Female_i \\
&+ \beta_{13} Indigenous_i \times Janitor_i \times Female_i + Controls_i \beta_{14} + \varepsilon_i)]
\end{aligned} \tag{5}
$$

Note that we only send male applicants to security positions, hence the missing interactions between female and security.

Callback Rates by City

We analyze callback rates by city as follows:

$$\begin{aligned} Callback_i = \ \Phi[\beta_0 &+ \beta_1 Indigenous_i \times Albuquerque_i \\ &+ \beta_2 Indigenous_i \times Anchorage_i + \beta_3 Indigenous_i \times Billings_i \\ &+ \beta_4 Indigenous_i \times Chicago_i + \beta_5 Indigenous_i \times Honolulu_i \\ &+ \beta_6 Indigenous_i \times Houston_i + \beta_7 NA_i \times Los\ Angeles_i \\ &+ \beta_8 NH_i \times Los\ Angeles_i + \beta_9 Indigenous_i \times New\ York_i \\ &+ \beta_{10} Indigenous_i \times Oklahoma\ City_i + \beta_{11} Indigenous_i \times Phoenix_i \\ &+ \beta_{12} Indigenous_i \times Sioux\ Falls_i + Controls_i \beta_{13} + \varepsilon_i)] \end{aligned} \quad [6]$$

where this regression is run separately by occupation (retail, server, kitchen staff, janitor, security). Los Angeles appears twice in this regression, as we sent both Native Hawaiian and Native American applicants to positions in those cities. All other cities receive either only Native Hawaiian (Honolulu), Alaska Native (Anchorage), or Native American applicants (all other cities).

Callback Rates by Indigenous Signal Type

We analyze callback rates by Indigenous signal type by splitting Indigenous applicants into mutually-exclusive categories, based on signals, as follows:

$$\begin{aligned} Callback_i = \ \Phi[\beta_0 &+ \beta_1 Volunteer\ Only_i + \beta_2 Language\ Only_i + \beta_3 First\ Name\ Only_i \\ &+ \beta_4 Last\ Name\ Only_i + \beta_5 Two\ Signals_i + \beta_6 Three\ Signals_i \\ &+ \beta_{12} Boys\&Girls_i + \beta_{12} FoodBank_i + \beta_{12} Gaelic_i + Controls_i \beta_{13} + \varepsilon_i)] \end{aligned} \quad [7]$$

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 Navajo applicant with a Navajo last name only, *Two Signals* is an indicator variable for any combinations of two the above four signals, *Three Signals* is an indicator variable for any combination of three signals, *Boys & Girls* is an indicator variable for having the Boys and Girls Club control volunteer experience, and *Food Bank* is an indicator variable for having the food bank control volunteer experience[36], and *Gaelic* is an indicator variable for having the Gaelic control language.[37]

Results

Data Sample for Current Draft

We have complete data collecting, and we have sent a total of 13,400 job applicants to 6,675 job openings. However, in this early, preliminary draft, we only present 9,066 job applicants for 4,533 jobs since these are the only observations for which we have had the opportunity to code responses thus far. The remaining jobs will be coded for their responses and included in our final analysis.

Estimates by Race and Indian Reservation Upbringing

Table 5 presents the raw callback rates by race. The callback rates are nearly identical for whites and Indigenous Peoples at 29.8% and 30.2%, respectively. By subgroup, the callback rates are 29.4% for Native Americans, 38.7% for Native Hawaiians, and 36.5% for Alaska Natives. Exact Fisher tests (two-sided) find that Native Hawaiians have a statistically significantly higher callback rate compared to both whites and Native Americans (both at 1% level). However, these estimates do not control for city-specific callback rates, and these results could be explained by higher callback rates for all applicants in Honolulu or Los Angeles[38].

[36] 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.

[37] 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 is not the case.

[38] There are two possible explanations. First, applicants to jobs in Anchorage and Honolulu had all three jobs in the same occupation, while applicants in other cities had the oldest job (Job 3, first one out of high school) at a grocery store (either local to the city, on an Indian reservation, or in a rural town). This job may be less relevant, leading to

In Table 6 we estimate probit regressions, following Equation [3], to estimate callback differences by race. The results without controls (column (1)) mirror the raw differences discussed above, with Native Hawaiians having a statistically significantly higher callback rate. However, adding the regular controls (column (2)), which includes city fixed effects, causes the estimate to become insignificant. In the regression with regular controls, Native American applicants (without a reservation upbringing) have an identical callback rate (with a standard error of 1.5 percentage points). Native Hawaiians have a callback rate that is 0.5 percentage points lower (again, insignificant). Alaska Natives have a callback rate that is 4.5 percentage points lower (but statistically insignificant). This is a substantial callback difference for Alaska Natives but is very imprecisely estimated (the standard error is 6.0 percentage points), reflecting the minimal number of observations (we only have 104 Alaska Native observations thus far). Therefore, these probit estimates show no evidence of discrimination, although there is likely not enough power to detect for differences between white and Native Hawaiian, and white and Alaska Native specifically. These zero estimates do not change in any meaningful way when moving from regular to full controls (column (3)).

In Table 6, we also explore if discrimination against Native Americans is more significant if they have an upbringing on an Indian Reservation. The estimates are not significant and are near zero. However, the estimated effect of having a rural background (added as a control to some of the white applicants) is negative and marginally significant in the regression without controls, but adding controls also removes this significance.

higher callback rates for all applicants in Anchorage and Honolulu. Second, there may just be higher callbacks in those two cities otherwise (see, e.g., footnote 2).

Estimates by Occupation and Gender

Table 7 presents the results by occupation (Equation [4]). For all occupations except Janitor, the callback rates are nearly identical for Indigenous and white applicants. For janitor, however, we see a 3.3 percentage point lower callback rate for Indigenous applicants, but this is insignificant with a standard error of 4.8 percentage points. This estimate is very imprecise given that it is only based on 357 observations thus far.

Table 8 presents results by occupation and gender (Equation [5]). The estimates show no preference for Indigenous men over white men. We do find a preference for female applicants for server and retail positions, which is not surprising. For kitchen staff, there is no evidence of a gender preference, but for janitor we find a large imprecise penalty faced by women- a 9.4 percentage point lower callback rate- but this is insignificant (standard error of 5.7). The Indigenous interactions with female suggest that perhaps the benefit for women in retail is only attributed to white women, as the coefficient on this Indigenous x Female interaction is -5.4 percentage points, which is nearly the benefit for women in general (5.8). However, this interaction is not statistically significant.

Estimates by City

Table 9 shows results by city (Equation [6]). Again, there are no differential results by city. The estimates are a bit more negative for smaller cities (which have a more substantial Indigenous concentration), but the pattern is not entirely consistent. For some cities (Anchorage, Sioux Falls, Billings), the number of applicants is small, so there is likely not enough power to detect differences between all the cities.

Estimates by Indigenous Signal

We explore if our results differ based on the three ways we signal Indigenous status (volunteer, language, name). These results, based on Equation [7], are presented in Table 10. The estimates do not show that the results differ by signal. For Indigenous applicants who have the volunteer signal only, the callback rate is 2.7 percentage points lower, but this is statistically insignificant (standard error of 1.7). The estimates on the control 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.

For Indigenous applicants who have the language signal only, the callback rate is nearly the same (0.3 percentage points lower). Similarly, the control for the Indigenous language (Gaelic) is also insignificant, and the penalty of having Gaelic is actually larger, a 2.2 percentage point lower callback rate, but this is not statistically significant. This is further evidence that there is no discrimination against Indigenous Peoples when this signal is used.

The estimates with two or three signals are also insignificant (but imprecise, especially for three signals). These insignificant estimates suggest that there is no evidence to support that having multiple signals affects the callback rate (if anything, it increases the callback rate, contrary to what we expected). This is further evidence that there is no discrimination, as there is no evidence that the magnitude of the signal negatively affects the callback rate.

Discussion and Next Steps

Though preliminary, these results strongly suggest a lack of discrimination against Indigenous Peoples. Also, we do not find discrimination against Native American applicants from Indian Reservations. While these results are preliminary, we have enough data according to our power analysis to detect callback differences of at least three percentage points. By the time our data coding is complete, we will have about 13,400 applicants instead of our current 9,066 applicants.

Our results are robust in several ways, including robust to controls, robust to the Neumark (2012) correction for the variance of unobservables, and robust to the signals of Indigenous status (volunteer, language, name). The results are also insignificant regardless of whether the results are estimated separately by city, occupation, or gender.

Several robustness checks remain. These include clustering our standard errors by job or multi-way by job and resume, estimating results measuring callbacks as unambiguous positive responses only, and weighting the regression results by the Indigenous population and/or by the popularity of each occupation.

For population weighting, our estimates are currently unweighted, which means that they oversample populous cities such as Chicago, Houston, New York, and Los Angeles, which have more jobs but have a lower proportion of Indigenous Peoples. On the other hand, the other cities have a greater proportion of Indigenous Peoples, but fewer jobs. Thus, to create discrimination estimates that are population-representative for Indigenous Peoples, we must down-weight observations from big cities and up-weight observations from smaller cities. To do this, we will use population estimates from Hixson, Heplyer, and Kim (2012) and DeVoe, Darling-Churchill, and Snyder (2008).

Similarly, we can weight by the popularity of occupations according to the CPS data (Tables 2 and 3). While the number of posted job ads likely corresponds to these amounts, we may not have sampled job ads proportionately. For example, we assigned research assistants different occupation or city combinations, and some occupations may have been over- or under-sampled.

Thus, we will also re-weight our estimates using occupation weights constructed from the CPS data. We will also re-weight using both the city population and the occupation weights. Since neither occupation nor city interactions yield statistically significant estimates (Tables 7 and 9), we do not expect this re-weighting to affect the results.

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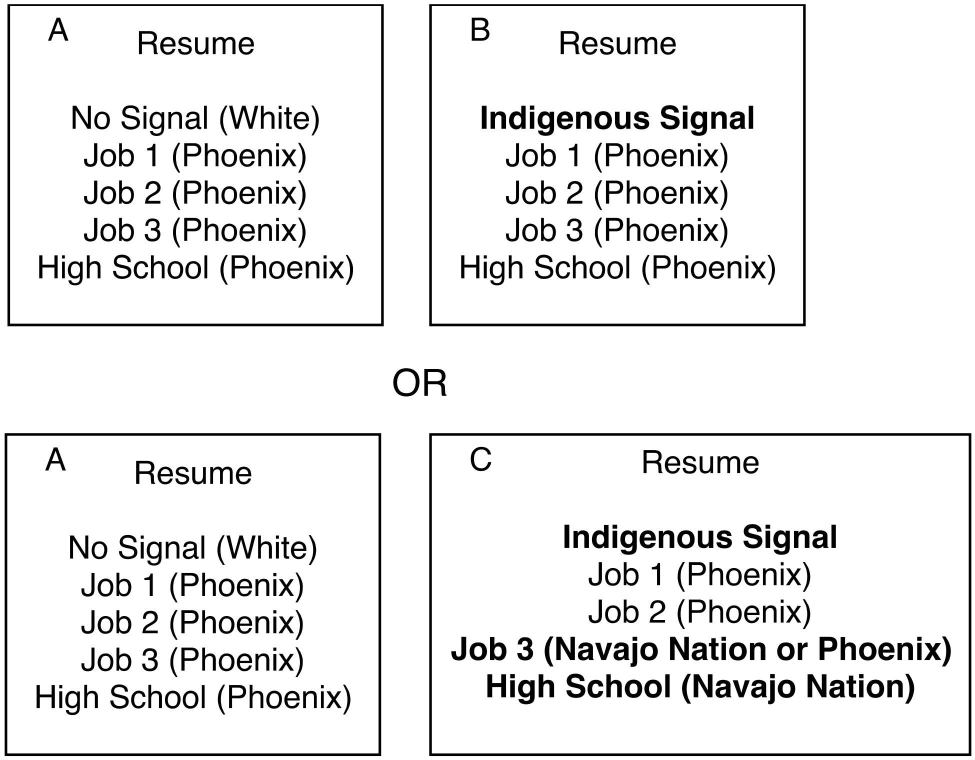
Figure 1 – Example of Pairs of Applicants for Jobs in Phoenix with Navajo Applicants



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

Table 1 – Summary of Possible Racial Signals by Indigenous Group

Indigenous Group	Signals of Indigenous Status: Volunteer Experience	Language	First Name	Last Name	Indian Reservation Possible (Type C)
Navajo	X	X (Navajo)		X	X
Apache	X	X (Apache)			X
Blackfeet	X				X
Tohono O'odham	X	X (Pima)			X
Oglala Lakota	X	X (Lakota)			X
Osage	X				X
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 1).

Table 2 – Selected Occupations of Men Aged 25-35, by Race

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

Notes: Data come from all months of the 2015 Current Population Survey. Estimates are weighted using population weights. Occupations are ranked based on the decreasing share of white men that have this occupation out of all white men.

Table 3 – Selected Occupations of Women Aged 25-35, by Race

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

Notes: See the notes to Table 2. Occupations are ranked based on the decreasing share of white women that have this occupation out of all white women.

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: A, B, and C refer to the major resumes types presented in Figure 1, where A is always a white applicant, B is always an Indigenous application who grew up in the urban center, and C is always a Native American applicant who grew up on an Indian reservation.

Table 5 – Mean Callback Differences by Indigenous Status

Callback:	No	Yes	Total
White	3,184 (70.2%)	1,349 (29.8%)	4,533
Indigenous	3,166 (69.8%)	1,367 (30.2%)	4,533
Native American	2,689 (71.6%)	1,069 (28.4%)	3,758
Native Hawaiian	411 (61.3%)	260 (38.7%)	671
Alaska Native	66 (63.5%)	38 (36.5%)	104
Total	6,350 (70.0%)	2,716 (30.0%)	9,066
Test of independence (p-value):	**White**	**N.A.**	**N.H.**
White	…	…	…
Native American	0.198	…	…
Native Hawaiian	0.000	0.000	…
Alaska Native	0.159	0.079	0.745
White vs. Indigenous = 0.000			

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

Table 6 – Discrimination Estimates by Race and Indian Reservation Upbringing, with and without Controls

	No Controls (1)	Regular Controls (2)	Full Controls (3)
Native American	-0.022	0.000	-0.001
	(0.015)	(0.015)	(0.015)
… x Reservation	-0.006	-0.009	-0.008
	(0.022)	(0.020)	(0.020)
… x Reservation x Reservation Job	0.007	-0.003	-0.001
	(0.032)	(0.028)	(0.028)
Alaska Native	0.057	-0.045	-0.052
	(0.051)	(0.060)	(0.060)
Native Hawaiian	0.079***	-0.005	-0.007
	(0.026)	(0.023)	(0.023)
Rural	-0.052*	-0.004	-0.006
	(0.026)	(0.023)	(0.023)
… x Rural Job	0.012	-0.023	-0.022
	(0.038)	(0.031)	(0.031)
Callback Rate for White:		29.76%	

Notes: Marginal effects are reported, computed as the discrete change in the probability associated with the dummy variable, evaluating other variables at their means. 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 resumes quality features (Spanish, no typos in the cover letter, better cover letter, and two occupation-specific skills), occupation, gender, resume sending order, volunteer experience, and city. The full controls include the regular controls and graduation year, resume naming style, e-mail script version, e-mail format, e-mail subject, e-mail opening line, e-mail body, e-mail signature format, e-mail domain, voicemail greeting, oldest job (Job 3) start month, gap (in months) between Job 3 and Job 2, gap between Job 2 and 1, indicator variables for each company used on the resume, and duration of volunteer experience (in months).

Table 7 – Discrimination Estimates by Occupation

Indigenous	Estimate	Callback Rate for Whites	N
… x Retail	-0.004	23.80%	2,042
	(0.022)		
… x Server	0.004	24.93%	2,214
	(0.022)		
… x Kitchen	0.000	33.15%	3,710
	(0.018)		
… x Janitor	-0.033	34.27%	356
	(0.048)		
… x Security	-0.009	41.40%	744
	(0.033)		

Notes: N=9,066. 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.

Table 8 – Discrimination Estimates by Occupation and Gender

	Indigenous	Female	Indigenous x Female	Callback Rate for White Men
… x Retail	0.022	0.058*	-0.054	22.73%
	(0.031)	(0.033)	(0.039)	
… x Server	-0.009	0.111***	0.025	19.92%
	(0.033)	(0.032)	(0.044)	
… x Kitchen	0.003	-0.013	-0.007	32.94%
	(0.023)	(0.021)	(0.032)	
… x Janitor	-0.004	-0.094	-0.053	41.18%
	(0.075)	(0.057)	(0.089)	
… x Security	-0.010	…	…	41.40%
	(0.033)			

Notes: Note that we did not send female applicants to security jobs. N=9,066. 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.

Table 9 – Discrimination Estimates by City

Indigenous	Estimate	N
… x Albuquerque	-0.051	310
	(0.050)	
… x Anchorage (AK Native)	-0.046	208
	(0.060)	
… x Billings	-0.047	92
	(0.090)	
… x Chicago	-0.005	1,288
	(0.028)	
… x Honolulu (Native HI)	-0.002	908
	(0.030)	
… x Houston	-0.008	646
	(0.041)	
… x Los Angeles (Native Am.)	0.002	1,642
	(0.023)	
… x Los Angeles (Native HI)	-0.006	432
	(0.037)	
… x New York	-0.015	1,946
	(0.025)	
… x Oklahoma City	0.010	330
	(0.057)	
… x Phoenix	0.035	1,178
	(0.029)	
… x Sioux Falls	-0.019	80
	(0.097)	

Notes: N=9,066. 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.

Table 10 – Discrimination Estimates by Signal Type

Indigenous	Estimate	N
… x Volunteer Only	-0.027	5,244
	(0.017)	
… x Language Only	-0.003	3,316
	(0.016)	
… x First Name (Native Hawaiian) Only	-0.060	618
	(0.060)	
… x Last Name (Navajo) Only	0.073	498
	(0.066)	
… x Two Signals	0.041	896
	(0.029)	
… x Three Signals	0.017	114
	(0.059)	
Boys & Girls Club (Volunteer Control)	-0.012	2,218
	(0.015)	
Food Bank (Volunteer Control)	-0.012	2,315
	(0.014)	
Irish Gaelic (Language Control)	-0.022	587
	(0.020)	

Notes: N=9,066. 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. The excluded volunteer control is Big Brothers Big Sisters without the racial signal.

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

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Contents:

Pre-Analysis Plan

Before putting this experiment into the field, we filed a pre-analysis plan (PEP) and registered it with the American Economic Association's Randomized Control Trial Registry (socialscienceregistry.org)[1]. The goal was to pre-specify any variables, models, sample sizes, or decisions that could feasibly be data mined. In this experiment, there is really only one outcome – callbacks – so there is little to no risk of a typical data mining issue where a researcher can select a subset of outcome variables that show statistical significant results (Olken, 2015). However, we chose to pre-specify some controls variables and models to avoid less risky possibilities of data mining, such as choosing which resume control variables to include in the regressions. This sort of decision of which control variables to use is not unique to our study, and while it is not common to do this, it has been done before with some benefit (e.g., Neumark, 2001). Thus, we sought to file this pre-analysis plan to guard against risks of some data mining while also not typing our hands too much in ways that negatively affect our ability to conduct this research later (see Olken, 2015, p. 71 for some useful discussion of the costs of pre-analysis plans).

In this plan, we pre-specified the way callbacks would be coded, the primary probit models and tabulations that would be conducted, and the main control variables that would be used in the regressions. We also committed to use a particular sample size, in additional to using all our data, for our main results to mitigate concerns of data mining if our sample size exceeded the minimum sample size required based on the power analysis.

We largely adhered to the core of the pre-analysis plan, but made a few minor deviations. The first deviation is in our full controls (see Table 6, column (3)), in which we planned to include

[1] https://www.socialscienceregistry.org/trials/2299

indicator variables for each company used on the resume in our vector of full controls[2]. Given the number of company signals, the use of company controls heavily saturated the model and resulted in some highly sensitive and imprecise estimates. Removing the company controls stabilized the estimates. Importantly, aside from one table, we only report estimates based on our regular control vector, which never included the company control.

Another deviation concerns our use of multiple Indigenous signals. In our pre-analysis plan, we anticipated reporting each set of interactions separately (e.g., two-signal and three-signal combinations of the Volunteer, Language, and Name (first or last) signals). However, doing so fractionated the sample too much that we needed to pool the multiple signal terms into two variables that captured the ascending degrees of Indigenous-signaling (e.g., with the *Two Signals* and *Three Signals* indicators). Even with pooling in this way, we do not find statistically significant estimates with estimates on either variable.

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

Additional Resume Construction Details

First Names as a Racial Signal

Using first names is a natural way to signal minority status in AC studies. This approach is obvious and near perfect for gender, but signaling race by name is more complex. For race, names are used to signal African-American status (e.g., Bertrand and Mullainathan, 2004), Arab, Muslim, or Middle Eastern descent (e.g, Rooth, 2010), Turkish or Moroccan descent (e.g., Baert and De Pauw, 2014), and Asian, Roma, Ashkenazi Jewish, African, Indian, and Pakistani descent, among others (Booth, Leigh, and Varganova, 2012; Fershtman and Gneezy, 2001; McGinnity and Lunn, 2011; Oreopoulos, 2011), and caste (e.g., Siddique, 2011). Using names as a signal improves external validity since signaling minority status other ways (e.g., volunteer experiences) is less common, while names must be included. However, first names can signal socio-economic status in some cases, which some argue (Fryer and Levitt, 2004) is the case in studies such as Bertrand and Mullainathan (2004).

Last Names as a Racial Signal

For those who identify as AIAN only, AIAN-specific last names are not common, but they are also not unusual. There are 268 last names where at least 80% of those with that name identify as AIAN only. Further, 5.5% of individuals who identify as AIAN only have one of these 268 last names[3]. A broader list of names, where at least 30% of those with the name identify as AIAN only, has 660 names, and 11.0% of those who identify as AIAN only have one of these 660 names.

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

There are costs and benefits to this last name signal. Last names have the benefit of being a natural signal, since one cannot realistically put a different last name on the resume, but one could refuse to disclose relevant experience or skills that signal Indigenous status (e.g., the volunteer or language signals, discussed earlier) or applicants may re-phrase the experience in attempts to obscure racial signals. However, it may be less likely that employers understand that these are Native American last names, relative to, say, understanding African-American first names.[4] This makes this last name signal weaker.

Another issue with using last names as a signal of race is that they are a weaker signal for women since the last name may be taken from her spouse. This is especially an issue given the increase in interracial marriages after the 1970s (Fryer 2007). Thus, if discrimination against Native American women occurs less than for men, using last name as the only signal, then this suggests that discrimination is weaker for women and/or that this is a weaker signal of race for women. In contrast, using Native Hawaiian first names only as a signal may present a different set of implications: a Native Hawaiian first name and a non-Native Hawaiian last name (although Native Hawaiian last names appear uncommon) may imply applicant multi-raciality, it may separately or additionally imply interracial marriage for female applicants, or it may simply imply that a non-Native Hawaiian was given a reasonably prevalent and popular Hawaiian name.

[4] We plan to conduct some surveys both of students and of a more population-representative sample (e.g., via Qualtrics) to gauge perceptions of these names.

Additional Information on Data Collection

Working with Research Assistants

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

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

Sending Out Applications

Once research assistants determined that a job was eligible to apply to, they entered information about the job into a spreadsheet. They entered the job ID number (unique to each posting), the day and city in which the job was posted, the occupation, the email address that the applications should be sent to, the subject line to be used (if the employer requests a particular subject line, otherwise we randomize subject lines that are realistic), and if the employer requests a resume in Microsoft Word format rather than PDF (by default we send resumes as PDF documents). We then used Python and SQL code created by Nanneh Chehras to email these job applications automatically, with a delay of a few hours between emails to the same employer. The code was run at least twice per week ideally on set days (e.g., Monday and Thursday).

Each day was randomly assigned a different set of resumes in terms of skill levels, employed or unemployed, and the gender of the applicants, as these factors are set to be the same within resume sets. Within each set the order that the applications sent out was randomized. To distinguish further the resumes in each set, we will name the computer files slightly differently. One resume in the set will be named “FirstLastResume,” where First and Last are replaced with the applicant’s first and last names, another resume was “ResumeFirstLast,” while the final resume will be “FirstLast.” This naming convention was randomly assigned.

Matching Responses to Jobs and Applications

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

Matching responses to exact applications and job advertisements is easier if the response from the employer is through email. If the email from the employer is sent as a reply to the original application email (which is sent to the employer through an email relay system), then the email response will contain a unique ID number for the job ad. Each ID number provides a one-to-one match to a job ad. However, if firms respond directly to the individual (by typing in the email address rather than hitting reply), then we will not observe this job ID. In this case we use other information from the email, such the company name or type, job ad title, and location. While our email addresses are not perfectly unique[5], we also look through records of which applications used which email addresses, and for which job ads, to narrow down the likely matches.

Voicemail responses convey less information which made matching more difficult, but usually possible. Based on how we assigned phone numbers by bin, we always knew the city and Indigenous status of the applicant that the voicemail response was for, and we almost always knew the occupation (janitor and security get the same phone numbers). We then used information in the voicemail message itself to try to match to an exact applicant or job advertisement. We assigned first and last names such that the combination of phone number (bin) and first or last name gives us the unique job applicant (except in a few cases for janitor or security, which fall into the same bin). This was useful way to improve matching since employers almost always mention the first or last name of the applicant they are calling.

However, a particular applicant can apply to multiple jobs (since we assign each applicant to a particular day of the month). Given this, additional information was required to make a match to a specific job advertisement. This was often disclosed via the phone number of the employer and in the content of their voicemail message (e.g., they mention the employer by name). When matching to a job ad is not possible, we matched it to the applicant level.[6]

[5] A few email addresses get randomly repeated based on the randomization process to generates names and email address. So there may be more than one unique applicant with the same or similar name that uses the same email address, but this only occurs a few times. Also, since we assign each day to be a different set of applicants, an applicant with a particular email may apply to multiple jobs in one day.

[6] For a handful of voicemail responses, we didn't have enough information to even match it to the applicant.

Neumark (2012) Correction for the Variance of Unobservables

Applicant Quality and the Variance of Unobservables

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

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

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

$$\begin{gathered} T(X^{I*}, X^{II}_{N}) \mid (N = 1) = 1 \; if \; \beta_1 X^{I*} + X^{II}_{N} + \gamma N > c \\ T(X^{I*}, X^{II}_{W}) \mid (N = 0) = 1 \; if \; \beta_1 X^{I*} + X^{II}_{W} > c \end{gathered} \qquad \text{[A1]}$$

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

$$\Phi[(\beta_1 X^{I*} + \gamma N - c)/\sigma_N^{II}]\ if\ N = 1$$
$$\Phi[(\beta_1 X^{I*} - c)/\sigma_W^{II}]\ if\ N = 0. \quad [A2]$$

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

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

Quality Features

Any resume or applicant feature that shifts the quality of the resume, in the eyes of the employer, can be used in the Neumark (2012) correction. Of course, one can randomly add quality features using resume randomization tools (Lahey and Beasley, 2016, 2009) and then let the data "speak" about what features, according to the employer, boost quality (Lahey and Beasley, 2016).

But we feel that it is important to deliberately include some quality features beforehand that researchers believe are likely to matter to ensure that there is enough variation in applicant quality in order for this correction to work. This is crucial since the Neumark (2012) correction requires significantly more power than the standard, uncorrected, analysis.

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

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

CPR and First Aid and a certificate or training in food safety. Examples of all these skills are shown in the resume examples in later in this appendix.

Of course, not all added quality features will have a positive effect[7], and some other randomly added features (e.g., certain employers, template styles) may have positive or negative effects. Neumark (2012) shows the iterative process used to select from among the resume features the ones that can be used in the Neumark (2012) correction. This mirrors the process outlined in Lahey and Beasley (2016) for letting the data "speak" about which features actually matter.

[7] For example, Spanish, a college degree, and the occupation-specific skills often boosted interview rates in Neumark, Burn, and Button (forthcoming), while adding typos to the resume (missing periods or commas), volunteer experience, and employee of the month awards did not have positive effects, sometimes having negative ones. Lahey and Beasley (2016) also discuss a similar issue for typos. These differential results by quality element prompted us to choose some different quality elements.

Additional Robustness Checks

Clustering

(Forthcoming)

Indigenous Population Weighting

(Forthcoming)

Job Popularity Weighting

(Forthcoming)

Population and Job Popularity Weighting

(Forthcoming)

Re-Estimating Results with "Interview Only" Measure of Callbacks

(Forthcoming)

Sample Resumes and Cover Letters

Sample Resume #1

David Walker

#3 - 27 W Pasadena Ave

Phoenix, AZ 85013

Phone

Email

Work Experience

Sales Associate
Best Buy, Phoenix, AZ
Jan. 2007 - Present
Stocked shelves and displays. Checked for damage and correct pricing. Worked as a cashier and at customer service.

Customer Service Representative
JC Penney, Phoenix, AZ
July 2005 - Jan. 2007
Ensured accuracy in pricing and order information, Investigated sales questions, helped resolve shipping discrepancies, and collaborated with other departments to find resolutions. Processed customer orders/changes and returns according to established department procedure.

Volunteer Experience

Treasurer
Native Americans for Environmental Action, Phoenix, AZ
May 2014 - Present
Managed finances for a local group of Native American environmental activists. Promoted sustainable and ethical use of natural resources on tribal lands.

Education

High School Diploma
Chandler High School 2006

References are available to send.

Sample Cover Letter #1

From: “David Walker” *Email*

To: *Employer Email*

Subject: Application for *Position*

Attachment: DavidWalkerResume.pdf

Dear Hiring Manager,

My name is David Walker and I am contacting you to respond to your recently posted job ad for a *Position*.

I have enclosed my resume. I have experience in retail sales for over 10 year through positions at Best Buy and JC Penney. In these positions I had many responsibilities, such as managing inventory, working as a cashier, and resolving customer concerns. I also manage finances as a treasurer for Native Americans for Environmental Action, where we work to promote sustainable use of our tribal lands.

I am looking forward to hearing from you soon.

Sincerely,

David Walker

Email

Phone

Sample Resume #2

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

Objective	To obtain a position as a sales associate.
Work Experience	**Sales Associate** Costco, Phoenix, AZ *Oct. 2009 - Present* Assist customers as they shop, answering questions and trying to find the merchandise that fits their needs the best. Straighten up merchandise to ensure a professional appearance. Ring up customers at check out.
	Cashier Walmart, Phoenix, AZ *July 2008 - Sept. 2009* Worked as a cashier and in customer service Primary responsibilities were related to working the cash register, but also assisted with stocking shelves. Occasionally, I checked merchandise for damage and incorrect tags.
	Sales Associate Target, Phoenix, AZ *Nov. 2004 - June 2008* Answer customers' questions. Ring up customers at checkout. Handle returns and other customer service responsibilities. Straighten up merchandise to insure a professional appearance at all times.
Volunteering	**Youth Dance Instructor** Phoenix, AZ *Mar. 2014 - Present* Teach a youth Irish Step dance class (ages 10 to 15).
Education	**High School Diploma** North High School, 2004
References	References available upon request.

Sample Cover Letter #2

From: “Christopher Johnson” *Email*

To: *Employer Email*

Subject: Application for *Position*

Attachment: ResumeChristopherJohnson.pdf

Dear Hiring Manager,

My name is Christopher Johnson and I am interested in applying for your position of *Position*.

My resume (attached) provide information on my background and qualifications. To briefly summarize, I have significant experiences in retail sales through positions at Costco, Walmart, and Target. In these positions I worked as a cashier, stocked inventory, and handled returns and customer inquiries. I also volunteer as a youth dance instructor, teaching Irish Step dance.

I am looking forward to hearing from you to arrange a time for an interview.

Thank you kindly for considering my application.

Christopher Johnson

Email

Phone

Sample Resume #3

Jonathan Moore
1004 W Anderson D
Phoenix, AZ 85023
Phone
Email

Work Experience

Retail Associate
CVS, Phoenix, AZ
July 2011 - Present

- **I rang customers up at the cash register.**
- **I helped handle product returns, refunds, and other transactions according to company policies.**

Sales Associate
GAP, Phoenix, AZ
Apr. 2008 - July 2011

- **I assisted customers while they shopped.**
- **I answered customer questions and tried to direct them towards the products that best fit their needs.**
- **I rang customers up at the cash register and handled product returns**

Cashier
Navajo Nation Oil & Gas Stations, Navajo Nation Reservation, NM
Sept. 2005 - Jan. 2008

- **I answered customer questions and tried to direct them towards the products that best fit their needs.**
- **I helped ring customers up at the cash register and handled product returns.**

Volunteer Experience

Mentor
Big Brothers Big Sisters of America, Phoenix, AZ
Feb. 2014 - Present

- **Mentor for Native American youth in my community.**
- **Foster the development of social skills, academics, and an understanding our Native American culture and community.**

Education

High School Diploma
Navajo Pine High School, Navajo Nation Reservation, NM
2005

Sample Cover Letter #3

From: “Jonathan Moore” *Email*

To: *Employer Email*

Subject: *Position* - Jonathan Moore

Attachment: JonathanMoore.pdf

To Whom it May Concern,

My name is Jonathan Moore and I am very interested in your position of *Position*. Please see my attached resume for application to this position. A brief summary of my experience is that I have worked in retail sales for over ten years. I have worked at CVS, GAP, and at Navajo Nation Oil & Gas Stations. I also mentor Native American youth through Big Brothers Big Sisters, teaching them social skills and about our Native American culture.

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

Jonathan Moore

Phone

Email

Appendix Table 1 – Non-English Languages and Indian Reservations

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

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

Appendix Table 2 - Rural City and Reservation Matches for the Rural Control for Indian Reservation Upbringing

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

Notes: Distances between the city and the reservation and the rural town were determined using Google Maps. We present the time to drive between the two locations as this is more informative than strict distance.

Appendix Table 3 – Occupations of Men Aged 25-35, by Race

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

Notes: Data come from all months of the 2015 Current Population Survey. Estimates are weighted using population weights. Occupations are sorted and presented based on the decreasing share of white men that have this occupation out of all white men.

Appendix Table 4 – Occupations of Women Aged 25-35, by Race

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

Notes: See the notes to Appendix Table 3.

Appendix Table 5 – Heteroskedastic Probit Estimates for Callbacks
(Corrects for Potential Biases from Difference in Variance of Unobservables)

	Combined	**Retail**	**Server**	**Kitchen**	**Security**	**Janitor**
	(1)	(3)	(2)	(4)	(5)	(6)
	Common quality features	All quality features	All quality features	All quality features	All quality features	All quality features
A. Probit estimates						
Indigenous (marginal)	0.003	0.002	0.024	0.014	-0.017	-0.086
B. Heteroskedastic probit estimates						
Indigenous (marginal)	0.003	0.001	0.011	0.004	-0.009	-0.030
Overidentification test: ratios of coefficients on skills for Indigenous relative to white are equal (p-value, Wald test)	0.884	0.985	0.998	0.916	0.969	1.000
Standard deviation of unobservables, Indigenous/white	0.988	0.938	1.226	0.918	0.961	0.908
Test: homoscedastic vs. heteroskedastic probit (p-value, Wald test for equal variances)	0.916	0.700	0.305	0.616	0.626	0.924
Indigenous-level (marginal)	0.006 (0.024)	0.016 (0.044)	-0.033 (0.045)	0.017 (0.031)	-0.009 (0.035)	-0.011 (0.175)
Indigenous - variance (marginal)	-0.002 (0.022)	-0.016 (0.041)	0.045 (0.044)	-0.014 (0.027)	-0.004 (0.030)	-0.016 (0.170)
N	9,066	2,042	2,214	3,710	742	356

Notes: See Neumark (2012) and Neumark, Burn, and Button (forthcoming) for a discussion of this methodology. See also 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. Quality features were as follows: retail included knowledge of programs used to monitor inventory (VendPOS, AmberPOS, and Lightspeed) and experience with Microsoft Office applications; janitor included a certificate in using particular machines and a certification in janitorial and cleaning sciences; security included CPR and First Aid and stating that they are licensed in their state; server included CPR and First Aid and experience with point-of-service (POS) software used in food service; kitchen staff included CPR and First Aid and a certificate or training in food safety. All high-skilled resumes randomly receive all but one of these skills: fluency in Spanish as a second language, a more detailed cover letter, the lack of typos in the cover letter (that is, resumes without this "skill" have typos), and two occupation-specific skills.