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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 an audit study - a resume correspondence experiment - to measure discrimination in hiring faced by Indigenous Peoples in the United States (Native Americans, Alaska Natives, and Native Hawaiians). We sent employers 13,516 realistic resumes of Indigenous or white applications for common jobs in 11 cities. We signalled Indigenous status in one of four different ways. Interview offer rates do not differ by race, which holds after an extensive battery of robustness checks. We discuss multiple concerns such as the saliency of signals, selection of cities and occupations, and labour market tightness that could affect the results of our audit study and those of others. We also conduct decompositions of wages, unemployment rates, unemployment durations, and employment durations to explore if discrimination might exist in contexts outside our experiment. We conclude by highlighting the essential tests and considerations that are important for future audit studies, regardless of if they find discrimination or not.

JEL Codes:

J15; J7; C93; Indigenous Peoples; employment discrimination; Native American; Alaska Native; Native Hawaiian; Indian reservations; audit study; resume experiment; Gelbach decomposition; Oaxaca decomposition

Introduction

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

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children through Indian boarding schools (Adams, 1995; Feir, 2016b, 2016a). See Nabokov (1999) for a historical summary.

These injustices extend to contemporary racial disparities, which are some of the largest. Among racial and ethnic minorities, American Indians and Alaska Natives (AIANs) have the lowest employment-to-population ratio (54.6%, with 59.9% for whites), the highest unemployment rate (9.9%, with 4.6% for whites) (U.S. Bureau of Labor Statistics, 2016), and they earn significantly less income (median income of $35,060 in 2010, compared to $50,046 for the nation as a whole) (U.S. Census Bureau, 2015). These disparities are less stark for Native Hawaiian and Pacific Islanders (NHPIs) as they have the highest employment-to-population ratio (62.8%); though, this reflects a stronger economy in Hawaii. Even absent this, unemployment rates are still higher for NHPIs relative to whites (5.7%, versus 4.6%) (U.S. Bureau of Labour Statistics 2016). Poverty rates among those who identify as AIAN alone (NHPI) are nearly double (1.5 times) the rates of those in the general population (U.S. Census Bureau, 2015; WHIAAPI, 2010). For NHPIs, these disparities are even more substantial for the 22% of AIANs who reside or used to reside on one of the 326 federal or state Indian reservations (Gitter & Reagan, 2002; Taylor & Kalt, 2005; U.S. Census Bureau, 2015) or in Alaska Native Statistical Areas (U.S. Census Bureau, 2015).[1] These disparities are only becoming more relevant as Indigenous populations grow.[2]

Several factors could contribute to these disparities, such as differences in education, geography (especially in or near Indian reservations), and the intergenerational legacy of colonialism, such as the harmful effects of Indian Residential schools (Adams, 1995; Feir, 2016a, 2016b) and the slaughter of the bison by colonial settlers (Feir et al., 2017).

Another possible explanation is employment discrimination which anecdotal and survey evidence[3] suggest may be occurring, Indigenous Peoples also face negative stereotypes, some of which could contribute to discrimination.[4] Economists typically distinguish between three sources of employment discrimination: taste-based discrimination (Becker, 1957),[5] levels-based statistical discrimination (Arrow, 1973; Phelps, 1972),[6] and variance-based statistical discrimination (Aigner & Cain, 1977)[7] (see Lahey & Oxley (2018) for an excellent discussion). Psychology adds the concept of implicit bias, where discrimination occurs due to unconscious bias (Greenwald & Mahzarin, 1995).[8] Sociologists conceptualize

[1]Native Americans living on tribal lands were 10.1% more likely to live in poverty (Collett, Limb, and Shafer, 2016). We provide some details on the prevalence of Native Americans presently living on or near a reservation in Online Appendix Table I1.

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

[3]In a survey of 342 Native American adults in the United States, 31% believed that they were racially discriminated against when applying for jobs (NPR, Harvard T.H. Chan School of Public Health, & Robert Wood Johnson Foundation, 2017).

[4]Stereotypes, especially in the media, are that Native Americans are “savages” or “noble savages” (McLaurin, 2012; Riverwind, 2007). The stereotypes most closely connected with employment are lazy, less interested in work, less educated or skilled, more reliant on government “handouts” (James, Wolf, Lovato, & Byers, 1994; Riverwind, 2007; Schmidt, 2007; Tan et al., 1997) or more prone to alcoholism (Riverwind, 2007; Tan et al., 1997). For a broader discussion, see James et al. (1994).

[5]Taste-based discrimination, also called animus, is based on prejudice. Taste-based discrimination occurs when employers, employees, or customers get disutility from interacting with the minority group.

[6]Levels-based statistical discrimination occurs when minority groups are assumed to have different average levels of some quality signal. More intuitively, employers assume an average difference between minority and majority groups and use the perceived average difference to infer something about candidates. Often this type of discrimination operates through stereotypes. For example, employers view older workers as being worse with computers but more dependable (Burn, Button, Corella, & Neumark, 2019).

discrimination as being caused by numerous factors that are intrapsychic, organizational, or structural (Pager & Shepherd, 2008). Employment discrimination against Indigenous Peoples could occur from any of these sources.

We are only aware of one peer-reviewed quantitative study that studied employment discrimination against Indigenous Peoples in the United States.[9] Hurst (1997) decomposed the AIAN-white earnings gap using the Oaxaca-Blinder decomposition method. Hurst (1997) found that, while observable factors such as education and geography explain a large part (87%) of the gap, there is “still a substantial unexplained differential in earnings between the various categories of Indians and non-Indians.” (p. 805).

Quantifying employment discrimination against Indigenous Peoples is essential to inform policies to reduce these significant economic disparities. If there is little discrimination, then disparities are primarily caused by factors other than employment discrimination like differences in education, which policymakers may be able to target directly. However, if there is significant discrimination, then this may suggest that supply-side policy measures like education or skills training[10] 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 behaviours or our abilities to act upon them.

To quantify discrimination, we conducted a field experiment of hiring discrimination—a resume correspondence study—sending applications to job openings. Resume correspondence studies are the preferred method of estimating employment discrimination because they can hold all factors other than minority status constant (Bertrand & Duflo, 2017; Gaddis, 2018; Neumark, 2018) which is not the case for decomposition studies that use survey data (e.g., Hurst, 1997).

In our field experiment, job applications are identical on average but are either signalled to be white or Indigenous (Native American, Alaska Native, or Native Hawaiian). Our general approach follows previous studies of this nature (e.g., Baert, Cockx, & Verhaest, 2013; Bertrand & Mullainathan, 2004; Carlsson & Rooth, 2007; Neumark, Burn, & Button, 2019;

[7]Variance-based statistical discrimination occurs when quality signals are less informative (“noisier”) for one group because the variances of the quality signals differ. This differs from levels-based statistical discrimination, where the averages (the “levels”) differ. An example of variance-based statistical discrimination is that employers might have a better sense of what graduating from a particular school means for one group compared to another (Lahey & Oxley, 2018).

[8]Implicit bias, also called implicit social cognition, stems from psychological research (Greenwald, McGhee, & Schwartz, 1998). Implicit bias manifests unconsciously and involuntarily. Individuals are prone to this bias when making quick decisions, such as reviewing resumes for entry-level positions (Bertrand et al., 2005; Rooth, 2010),

[9]Research on discrimination against Indigenous people is somewhat more common for Canada (e.g., Kuhn and Sweetman 2002; Krishna & Ravi 2011; Feir 2013) and Australia (e.g., (Booth et al., 2012; Hunter, 2005). Many discrimination studies focus on the United States, but they are all on other disadvantaged groups. See Neumark (2018) for a review of the experimental studies. There are some qualitative studies of discrimination against Indigenous Peoples in the United States (see, e.g., Fenelon, 1998; Whitbeck, Hoyt, McMorris, Chen, & Stubben, 2001; Whitbeck, McMorris, Hoyt, Stubben, & LaFromboise, 2002)Fenelon, 1998; Whitbeck, Hoyt, McMorris, Chen, & Stubben, 2001; Whitbeck, McMorris, Hoyt, Stubben, & LaFromboise, 2002)but these are often case studies of particular groups.

[10]For example, the Bureau of Indian Affairs’ (BIA) Financial Assistance and Social Services (FASS) program (https://www.benefits.gov/benefits/benefit-details/801), the Native American Vocational and Technical Education Program (NAVTEP) (…/756), the U.S. Department of Labor’s Division of Indian and Native American Program (DINAP) (…/81), the Indian Higher Education Grant Program (…/796), and the U.S. Department of the Interior’s Job Placement and Training Program (…/797) (all accessed June 30, 2018). However, note that discrimination may frustrate the effectiveness of these types of policies less if the discrimination is statistical, rather than taste based.

Pager, 2003) by estimating hiring discrimination by comparing interview offer rates ("callbacks") by race.

Since signalling Indigenous status is not straightforward, we use four different methods. Our most common signal is volunteer experience, where we mention Indigenous status in the description of a volunteer experience, mirroring Tilcsik (2011), Ameri et al. (2018), and Namingit, Blankenau, & Schwab (2017). Our second most common signal is language, through listing an Indigenous language along with English as mother tongues in a language section on the resume. We also occasionally signal Native Hawaiian status using Native Hawaiian first names or Native American status by using last names of Navajo ancestry.

We also quantify whether there is an additional bias against Native Americans from Indian reservations. Employers may have negative perceptions of these reservations, as poverty rates there are higher (Collett, Limb, & Shafer, 2016), incomes are lower (Akee & Taylor, 2014), economic conditions are worse (Akee & Taylor, 2014; Gitter & Reagan, 2002; Taylor & Kalt, 2005), and educational quality can be lower (DeVoe, Darling-Churchill, & Snyder, 2008). If Native Americans from Indian Reservations, who move to urban centres, experience discrimination because of their upbringing, then this makes it more difficult for them to move to these urban centres. Thus, migration is a less useful way for them to seek economic opportunities and possibly escape poverty. Estimating this potential bias against Native Americans who move from Indian reservations to urban centres is increasingly vital given increasing migration over time from Indian Reservations to urban centres (e.g., Snipp 1997, Pickering, 2000).

Our large-scale field experiment, based on 13,516 job applications in 11 cities and five occupations, shows no evidence of discrimination in callbacks against Indigenous Peoples in any of these cities and occupations. We similarly find no additional bias against Native Americans who lived on an Indian Reservation. Our results do not vary by which combination of four signals for Indigenous status we use. Our results hold under a battery of robustness checks which include alternative functional forms and clustering, weighting, alternative callback measures, and correcting for the variance of unobservables (Neumark, 2012).

Our finding of no difference in callbacks is less common compared to the literature (Baert, 2018; Neumark, 2018). Of all resume correspondence studies from 2005 to 2018, 80 (78.4%) show significantly negative discrimination, 17 (16.7%) show no statistical evidence of discrimination, and five (4.6%) show significant preference for the minority group (Baert, 2018). However, publication bias may be a problem, as discrimination experiments with null results are less likely to be published (Zigerell, 2018).

We also conduct a complementary Gelbach (2016) and Oaxaca-Blinder decompositions of disparities in wages, unemployment rates, unemployment durations, and employment durations. We decompose the extent to which differences in observable characteristics explain these economic disparities, and what portion of the disparities remains unexplained, which could suggest discrimination. This provides an alternative measure of discrimination that, while problematic, is broader than our experiment by, for example, covering more cities and occupations, and covering contexts outside of hiring discrimination.

In our preferred decomposition, we find that AIANs have a large, unexplained gap in unemployment rates of 4.3 percentage points and weak evidence of slightly longer unemployment durations. We do not find unexplained gaps in within-occupation wages or in employment durations. For NHPIs, we find an unexplained gap (within occupation) of 4.1% lower wages, a 0.7 percentage point higher unemployment rate, and weak evidence of slightly *shorter* unemployment durations. When controlling for occupational differences, the wage gaps increase for both AIANs and NHPIs, suggesting lower access to higher-paying occupations.

Despite not finding discrimination in callbacks against Indigenous Peoples in our audit field experiment, this does not imply that Indigenous Peoples in the United States do not face employment discrimination at all. We discuss how certain choices for our experiment could have affected our estimates, such as the saliency of our racial signals, our choice of cities and occupations, and the relatively higher labour market tightness during our study. Our decomposition of economic disparities, despite the problems with this approach, also suggests that there could be hiring discrimination outside the context of our audit study. This is evidenced by Indigenous Peoples facing higher unexplained unemployment rates and holding lower-wage occupations.

In addition to this study being the first audit study of discrimination against Indigenous Peoples in North America, our study also provides many methodological contributions to the audit studies literature more broadly. We conduct numerous robustness checks and discuss other considerations that are crucial to the interpretation of audit studies. These include, but are not limited to pre-registering our experiment, using more than one method of signalling minority status, testing how salient signals of minority status and other resume features are, controlling for the variance of unobservables (Neumark, 2012), using population and occupation weighting to generate study-population-representative results, testing and discussing how our results vary by occupation and city, and exploring how our results vary by labour market tightness and economic cycles. These are essential checks and discussions for audit studies more broadly, regardless of whether they find discrimination. We also extend the decomposition literature by studying disparities in economic outcomes that are not usually studied (unemployment rates, unemployment durations, and employment durations) and we are among the first to apply the newer Gelbach decomposition.

Field Experiment Design

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

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

Pre-Analysis Plan

Before putting this experiment into the field, we filed a pre-analysis plan and registered it with the American Economic Association's Randomized Control Trial Registry.[11] The goal was to pre-specify any variables, models, sample sizes, or decisions to prevent data mining or p-hacking while simultaneously avoiding tying our hands too much in ways that would negatively affect our ability to conduct this research later (see Olken 2015 and Lahey & Beasley 2018). The pre-analysis plan is also useful given the apparent publication bias in studies of discrimination, where studies that do not find discrimination are less likely to be p (see Zigerell, 2018). We discuss this pre-analysis plan in greater detail in Online Appendix B.

Signalling Indigenous Status

There is no obvious or perfect way to signal Indigenous status, and different possibilities have strengths and weaknesses. Names are a common way to signal race since names always need to be revealed in the application process. However, names could be a weak signal as we discuss in detail later or could signal socioeconomic status in addition to race (Barlow & Lahey, 2018; Darolia, Koedel, Martorell, Wilson, & Perez-Arce, 2016; Fryer & Levitt, 2004; Gaddis, 2017a, 2017b). On the other hand, disclosing minority status through work or volunteer experience (e.g., Tilcsik 2011; Ameri et al. 2018; Namingit, Blankenau, and Schwab 2017) or through listing an Indigenous language under the skills section of a resume, could be stronger signals but also less externally valid since minority groups may prefer to avoid signalling minority status to avoid potential discrimination. It also may be less common to have these sorts of volunteer experiences or to speak an Indigenous language and list it on a resume. Because there is no clear best option for signalling, as Indigenous Populations are very heterogeneous,[12] we used four possible ways to signal that the job applicant is Indigenous: volunteer experience, languages spoken, first names for

[11]Few audit studies of discrimination were registered at the time that we registered, yet this is standard in other fields. For our registered trial, see https://www.socialscienceregistry.org/trials/2299 (accessed December 26, 2017).

[12]There are Native American 657 tribal nations as of 2017 (National Congress of American Indians, 2017)). Some have associated reservations, but others do not. Some also have specific languages or dialects, some of which are active and some which are not. Naming conventions for first and last names also vary by tribal group and historical factors. For these reasons, not as signals work for each tribal group.

Native Hawaiians, and last names for Native Americans of Navajo ancestry. We discuss these choices in much more detail below.

Our most common signal was using the volunteer experience only, used for 3,029 of the Indigenous resumes, followed by volunteer experience only (1,723), Native Hawaiian last names (475), and then Navajo last names (222). We also sometimes combine racial signals, using two signals (three signals) for 823 (92) resumes. In addition to these racial signals, we also signalled that some Native American applicants grew up on an Indian reservation by listing that they had graduated from a high school on an Indian reservation.

Table 1 presents our matching of possible racial signals to Indigenous groups. We explain these signals and why we assign them in this way in more detail below (with sample resumes in Online Appendix H). Overall, our goal was to ensure that all signals were appropriate for each tribal group so that any signals we use in combination are compatible.[13] Our intent was not to study discrimination for particular tribal groups (e.g., comparing Navajo to Osage), but rather to ensure that our signals were as valid as possible.

Volunteer experience as an Indigenous signal.—Volunteer and work experience have been used before to signal minority status. Tilcsik (2011) and others signal sexual orientation through volunteer experience with a lesbian, gay, bisexual, and transgender (LGBT) group. Ameri et al. (2018) signal disability partly through a relevant volunteer experience as an accountant at a fictional disability group. Namingit, Blankenau, & Schwab (2017) disclose an illness-related gap in employment history partly through a volunteer experience (Cancer survivor's group) on a resume. Relatedly, Baert & Vujić (2016, 2018) find that volunteer experience boosts callbacks, especially for immigrants.

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

A concern with using a volunteer experience to signal race is that ir could be valuable to employers, independent of the racial signal. To control for this, all resumes, regardless of race or signals used list a volunteer experience. For the white resume in a pair where the Indigenous resume has the volunteer signal, the white resume has a volunteer experience either at a local Boys & Girls Club or at a local food bank. For any resume pair where the Indigenous applicant does not signal through volunteer experience, then one resume chosen

[13]The signal combinations we use must be appropriate for each tribal group. For example, we do not want to have someone with a Navajo last name speak a more northern Indigenous language such as Lakota or have grown up on a reservation of a different tribal group (where Navajo names are not common). We also do not want to assign an Indigenous language that has unfortunately become very rare.

at random gets the Big Brothers and Big Sisters volunteer experience without a mention of race, and the other resume gets either Boys & Girls Club or food bank. Thus, we can directly identify the effect of the Big Brothers and Big Sisters volunteer experience, relative to the control volunteer experiences, separately from its use as a racial signal. We find no differences in callback rates by the type of volunteer experience.

Language as an Indigenous signal.—We found few audit studies of discrimination that used language as a signal of minority status (one example may be Oreopoulos, 2011, to some extent). The American Community Survey codes 169 AIAN languages, plus Hawaiian and Hawaiian Pidgin. While most Indigenous people primarily speak English, Indigenous languages are somewhat common: 26.8% of AIANs spoke a language other than English at home in 2014, compared to 21.2% nationally (U.S. Census Bureau, 2015). Among those who identified as NHPI alone and were born in the United States, 30.3% spoke a language other than English at home (U.S. Census Bureau, 2014). Since it is rare for non-Indigenous people to speak an Indigenous language, especially as a native speaker, this makes for a robust racial signal. We thus used Indigenous languages to signal Indigenous status in some cases for most (but not all) of the tribal groups since Indigenous language use varies by tribal group. Table 1 presents the languages that we selected for each Indigenous group, and Online Appendix A presents our analysis of Census data to determine the frequency of each Indigenous language and thus to what extent signalling through language is appropriate.

It is unclear how employers would view this signal.[14] To investigate this, we added the Irish Gaelic language as a control to 10% of the white resumes. Irish Gaelic, like Indigenous languages, is uncommonly used in the United States. It is also one that is unlikely to signal that the applicant might have worse English skills. While this control is imperfect, we find no difference in callback rates between resumes with an Indigenous language or Irish Gaelic or between resumes with Irish Gaelic and no languages listed.

First name as an Indigenous signal (Native Hawaiian only).—We signalled race through first names for some Native Hawaiian applicants only. To determine possible first names, we first considered names within the top 100 baby names from Social Security records for the state of Hawaii in order to get a list of common first names only.[15] We then investigated which of these popular names were Native Hawaiian, using various sources.[16] We settled on three male names: Kekoa, Ikaika, and Keoni, and one female name: Maile. When using the first name as a racial signal, we randomly assigned one of these names,

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

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

[16]http://www.allbabynames.net/, 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 in the last one.

conditional on gender. We did not use first names to signal race for Alaska Natives or Native Americans because there was little information on first names for these populations.[17]

Last name as an Indigenous signal (Native American, Navajo, only).—To find Indigenous-specific last names, we use tabulations from the 2000 Census of the racial composition of each last name.[18] Unfortunately, these data also do not include information on NHPI individuals, so we can only use this data to determine names for AIAN individuals. We used this data and other sources on the ancestry of names to select four names of Navajo origin: Begay, Yazzie, Benally, and Tsosie. These are among the most common last names that are almost exclusively held by individuals who identify as AIAN alone. Online Appendix A provides more details of our process for selecting these names.

We also considered the possibility of assigning some Native American last names that were perhaps stronger signals (e.g., Sittingbull, Whitebear). However, these names are rare[19] and are difficult to assign appropriately to tribal groups. Further, we had concerns that the names signalled stereotypical tropes of Native Americans from popular media (McLaurin, 2012; Tan, Fujioka, & Lucht, 1997).[20] That said, these sort of names would have been a stronger signal of Indigenous status, an issue what may have affected our results as we discuss in detail later.

Assigning racial signals.—Table 1 summarizes which of the signals we used for each tribal or Indigenous group. We allocated Indigenous signals as follows. For Navajo and Native Hawaiian applicants, where three signals were possible, we assigned signals with the following probabilities: name only (30%), language only (25%), volunteer only (25%), name and language (5%), name and volunteer (5%), language and volunteer (5%), and all three (5%). For Alaska Native, Apache, Tohono O'odham, and Oglala Lakota applicants, where language and volunteer were possible, we assigned signals with the following probabilities: language only (40%), volunteer only (40%), and both (20%). For Osage and Blackfeet applicants, only the volunteer signal was possible. Assigning more than one signal allowed us to test whether discrimination increased with higher saliency. Table 8 later in the paper presents how often we use each combination of signals, although the volunteer signal is by far the most common, followed the language signal, and then the names signals.

Indian Reservation Upbringing

We assigned half of the Native American applicants an upbringing on an Indian reservation rather than in the city. We signalled this through having graduated from a high school on an Indian reservation, rather than a local high school. We considered the seven Indian

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

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

[19]For example, "Whiteagle", one of the most frequent, 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.

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

reservations shown in Table 1. These fall within the top ten most populous reservations (Norris et al., 2012). We used one to three high schools per reservation, depending on availability. We specifically chose high schools with names that were a clear signal that the high school was on an Indian reservation. We also specified the location of the high school as “City, Reservation Name, State” to ensure the saliency of this signal. For the white, Native Hawaiian, and Alaska Native resumes, and the other half of the Native American resumes without an Indian reservation upbringing, we assigned one of two to four high schools local to the city (from Neumark, Burn, & Button, 2019, and Neumark, Burn, Button, & Chehras, 2019).

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

Employers may prefer local or non-rural applicants, which challenges our ability to identify differential treatment by Indian Reservation upbringing. We investigate this by randomly assigning a rural upbringing to white resumes in pairs where we sent a Native American resume. We added a high school in a small town to 25% of these white resumes, and then in half of these, we also assigned a Job 3 location in that same rural town, mirroring the reservation job.[21] Adding reservation signals may also increase the likelihood that the employer detects that the applicant is Native American. We attempted to control for this by sometimes assigning Indigenous applicants to have more than one racial signal to see if this affects results (it does not).

Cities

We focused on cities where more Indigenous Peoples live to get more population-representative estimates of discrimination. We applied for jobs in eight of the ten cities with the most people who identify as AIAN (Norris et al., 2012). These are, in decreasing order of AIAN population: New York, Los Angeles, Phoenix, Oklahoma City, Anchorage, Albuquerque, Chicago, and Houston.[22] We then added two additional smaller cities with a higher proportion who are AIAN: Billings and Sioux Falls. These cities give us additional variation in the proportion of the population that is Indigenous, which lends power to testing whether discrimination varies by the size of the minority population, as discussed in

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

[22]We excluded cities from within states already represented. Those excluded were Tulsa (rank of 6) and San Antonio (rank of 10).

sociology,[23] psychology,[24] and tested in some audit studies (e.g., Giulietti, Tonin, & Vlassopoulos, 2019; Hanson & Hawley, 2011). Billings and Sioux Falls are also noteworthy because these cities increase the geographical coverage of our experiment and are near a few Indian reservations of interest (e.g., Pine Ridge) (Pickering, 2000).

To study discrimination against Native Hawaiians, we applied to jobs in Honolulu, the city with the most Native Hawaiians. We also applied for some jobs in Los Angeles with Native Hawaiian applicants, as Los Angeles is the most popular mainland city for Native Hawaiians to live in (Hixson, Hepler, & Kim, 2012). While we cover more cities than most prior resume correspondence studies, we cannot study discrimination in all contexts. For example, it is often untenable in resume correspondence studies to identify and apply for jobs in small cities or rural areas, which in turn makes it difficult to study discrimination in these contexts. We discuss these and related issues and their implications in detail later.

Occupations

We chose common occupational categories where there were many jobs posted online that usually allowed applications by email and were common for applicants of about age 30. Tables 2 and 3 show the popularity of our selected occupations by race and sex for those ages 25 to 35, based on the Current Population Survey (CPS). These are sorted and ranked based on the per cent of whites of that sex who are in the occupation. Online Appendix A presents more detailed tables and additional information on how we selected occupations.

We settled on five broad occupations with high ranks: retail sales, kitchen staff, server, janitors, and security guards.[25] We used male and female applicants for all occupations except security guard as women infrequently hold that occupation.

Education

All applicants had a high school diploma only. We focused on this group for a few reasons. First, it is much less common for Indigenous Peoples to have a post-secondary education.[26] Second, advanced degrees are usually not required in our selected occupations, but high school education almost always is. Third, we wanted to focus on less-educated individuals who might be closer to the margins of poverty.

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[23]This literature started with Blalock (1967)'s "power threat hypothesis", which posited that as the size of the minority group (the "out-group") increases discrimination, violence, and social control against the minority group to preserve the balance of power in favour of the majority group (the "in group"). This theory was investigated and critiqued by Tolnay, Beck, & Massey (1989) and others (e.g., Fossett & Kiecolt, 1989; Parker, Stults, & Rice, 2005; Semyonov et al., 2004; Taylor, 1998).

[24]Allport (1954) proposed the contact hypothesis, which argued that discrimination could sometimes be reduced by contact with the minority group. Much research has built on or critiqued this theory since then (see, e.g., Dovidio, Glick, & Rudman, 2005). The contact hypothesis is suggestive that as the population of the minority group increases, discrimination decreases, which is generally the opposite conclusion of the "power threat" and similar hypotheses from sociology.

[25]If we had conditioned on a high school diploma when generating the frequency ranking of occupations, our chosen occupations would have been even more popular. We also note that other occupations that we did not select were also feasible. We chose security instead of drivers since driver jobs are commonly moving to companies like Uber and Lyft and because we already had the inputs to make security resumes from a previous study (Neumark, Burn, and Button, 2019). We opted for server and kitchen staff over customer service because customer service has some overlap with retail sales, which we had already included. While we could have applied for administrative and secretarial positions as women as in Neumark, Burn, and Button (2019), we decided to avoid doing so since the applications to those jobs in that study elicited many spam responses that made data collection less accurate and more time-consuming.

[26]According to data from the Current Population Survey, 35.3% of those who identify as non-Hispanic white only and have a college degree (Associate's degree or greater), while this is only 15.8% (19.9%) for those who identify as AIAN alone (NHPI alone) (see Online Appendix Table G1 for additional statistics).

Job Histories

We modelled our job histories and descriptions off of publicly posted resumes from a popular job search website. We randomly assigned three jobs with matching descriptions from a list of twelve possible jobs per city-occupation combination. The employer and job title came from real resumes or from active businesses. We randomly generated job tenure distributions, conditional on all three jobs spanning high school graduation to near the present.[27] All applicants within each pair were either both employed with 25% probability or both unemployed (as of the month before the job application) with 75% probability. Since kitchen staff jobs are very heterogeneous, covering experienced cooks down to entry-level dishwashers, we created separate resumes for cooks and more entry-level positions (e.g., food preparation, fast food, dishwasher).[28]

Age and Names

We set the age of all applicants to be approximately 29 to 31, via a high school graduation year of 2004 or 2005. We used common first and last names for age 30 from Neumark, Burn, & Button (2019), who got these names from Social Security tabulations of popular names by age.

Residential Addresses, Phone Numbers, and Email Addresses

Within each pair of applications sent to a job, both were from different residential addresses, taken from Neumark, Burn & Button (2019) and Neumark, Burn, Button, & Chehras (2019). We assigned each applicant a unique email address and one of 88 phone numbers.[29]

Collecting Data

Pairing Applicants to Apply to Jobs

After creating the final resumes, we combined them into pairs to apply to each job (see Figure 1). Each pair always had one white and one Indigenous applicant. Table 4 presents how we match Indigenous tribal groups to cities. This was to have Indigenous applicants, for those cities with a high proportion Indigenous, that reflect the Indigenous groups in the area. To ensure that the resumes were sufficiently differentiated, all other resume characteristics were randomized with replacement except the following: first and last names, resume template styles, addresses, email address domain, employers listed in the job history, exact phrasing describing skills or jobs on the resume or cover letter, and the specific volunteer experience.

[27]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.

[28]While we pool all these kitchen staff jobs together in our analysis, our results are the same if we analyse cook jobs separately from the others. These results are available upon request.

[29]We purchased enough phone numbers to assign numbers to bins of job applicants defined by city, race, and occupation (janitor and security shared numbers). With all these numbers and other matching methods (further discussed in Online Appendix A), it was highly unlikely that we could not assign a response to an applicant.

Sample Size

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

Identifying Job Ads

We identified viable jobs to apply for using a popular job-posting website (see Online Appendix B for more details). The jobs had to fit our occupational categories, be non-supervisor roles, and not require in-person applications, inquiries by phone, or application through an external website. We ignored job ads that required documents that we did not prepare (e.g., headshots or salary history) or required skills,[30] training, or education that our resumes did not have. We applied for jobs between March 2017 and December 2017.

Emailing Applications

We used a different email subject line, opening, body, closing, and signature order for each application in a pair to ensure that applicants from the same pair were not perceived as related. We based some of these scripts on examples and advice from job search experts.[31] The content of our emails mirrored cover letters, and we followed the standard practice for these jobs of including this content in the body of the email (requests for separate cover letters were rare).

Coding Employer Responses

We coded employer responses as positive (e.g., “Please call to schedule an interview”), ambiguous (e.g., “We reviewed your application and have a few questions”), or negative (e.g., “We have filled the position”). To avoid having to classify the heterogeneous ambiguous responses through a subjective process, we follow others (e.g., Neumark, Burn, & Button, 2019) and treat only positive and ambiguous responses as callbacks, but our results are robust to using strict interview requests only (Online Appendix Table D9).

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[30]We also ignored job ads that required a quality element that was part of the randomized quality features we added to the resumes to correct for the variance of unobservables. See Online Appendix C for more details.
[31]See https://www.thebalance.com/writing-a-letter-of-application-for-employment-2061570 (viewed August 20, 2016).

Data Analysis Methodology

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

We then estimated the following regression:

$$\begin{aligned} Callback_i &= \beta_0 + \beta_1 NA_i + \beta_2 NA_i * Reservation_i \\ &+ \beta_3 NA_i * Reservation_i * Reservation\,Job_i + \beta_4 NA_i + \beta_5 NA_i \\ &+ \alpha_1 Rural_i + \alpha_2 Rural_i * Rural\,Job_i + \mu_o + \mu_c + Controls_i\gamma + \varepsilon_i \end{aligned} \quad [1]$$

where i indexes each application, o indexes the occupation, and c indexes the city. *NA* is an indicator variable for being Native American, *AN* is an indicator variable for being Alaska Native, *NH* is an indicator variable for being Native Hawaiian, *Reservation* is an indicator variable for being a Native American applicant who grew up on an Indian Reservation, *Reservation Job* is an indicator variable for being a Native American applicant who grew up on an Indian Reservation and their oldest job listed on the resume (first job out of high school) was on the reservation, *Rural* is an indicator variable for being a white applicant who grew up in a rural area, and *Rural Job* is an indicator variable for being a white applicant who grew up in a rural town and their oldest job was in the rural town. White is the excluded racial category, so all estimates reflect callback differences relative to white applicants. μ_o and μ_c are occupation and city fixed effects, respectively. The city fixed effects are important to account for how we sent different types of Indigenous resumes by city (see Table 4). *Controls* is a vector of resume controls. We used three versions: (1) no resume controls, (2) regular controls[32] (the default for all our analysis), and (3) full controls, which includes additional controls[33] on top of the regular controls.

Following Neumark, Burn, & Button (2019), we cluster our standard errors on the resume. There may also be random influences at the level of the job ad, which would suggest clustering on the job, or two-way clustering on the job and the resume simultaneously (Cameron, Gelbach, & Miller, 2011). Our results are the same regardless of how we cluster (Online Appendix D).

After conducting this primary analysis, we then conduct regressions to analyse callback rates for Indigenous Peoples, compared to whites, separately by occupation, occupation and gender, by city, and by the Indigenous signal(s) we used. In these and all subsequent analysis, we use the regular controls and include occupation and city fixed effects.

[32]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), gender, resume sending order, and volunteer experience.

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

Results

Effects by Race and Indian Reservation Upbringing

Table 5 presents the raw callback rates by race. The callback rates were nearly identical for whites and Indigenous Peoples at 19.8% and 20.1%, respectively. By subgroup, the callback rates were 19.6% for Native Americans, 21.3% for Native Hawaiians, and 25.5% for Alaska Natives. However, these estimates do not account for clustering, and, more importantly, they do not control for city-specific callback rates, which is important since callback rates in general are higher in Honolulu and especially in Anchorage (24.8% versus 19.8%).

Table 6 presents the estimates from Equation [1]. The regression without controls (column (1)) shows again that Alaska Natives have a higher callback rate. However, adding city fixed effects (column (2)) removes this difference. In the regression with regular controls and occupation and city fixed, our preferred and default specification in column (2), Native American applicants (without a reservation upbringing) have only a 0.4 percentage point lower callback rate, but this is not statistically significant. Alaska Natives (Native Hawaiians) have a 0.5 percentage point higher (0.3 percentage point lower) callback rate, but this is again not statistically significant.

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

Effects by Occupation and Gender

Table 7 presents discrimination estimates by occupation from a similar regression to Equation [1], but pooling Indigenous groups into one indicator variable, and interacting this with each occupation. The callback rates are nearly identical in all occupations.[34]

In Online Appendix Table D4, we present estimates by occupation and gender. These show no racial or intersectional discrimination. We find a strong preference for female applicants for server (retail) positions: a 6.5 (3.7) percentage point higher callback rate for white women compared to white men (who have a callback rate of 13.3% [16.3%]).

Effects by City

Online Appendix Table D5 shows results by city. Callback differences are within two percentage points for all cities except Phoenix (Albuquerque) where Indigenous applicants have a 4.1 percentage point higher (3.7 percentage point lower) callback rate. Only the estimate for Phoenix is statistically significant, at the 10% level only.[35]

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[34]These results, available upon request, are similar for Native Americans, Native Hawaiians, and Alaska Natives analysed separately.
[35]Online Appendix Table D6 extends this analysis to include interactions between *NA*, *Reservation*, and *city*, and does not find any differential results by city.

Estimates by Indigenous Signal Type

We then explore if our results differed by how we signalled Indigenous status, as follows:

$$\begin{aligned} Callback_i = {} & \beta_0 + \beta_1 Volunteer\,Only_i + \beta_2 Language\,Only_i \\ & + \beta_3 NH\,First\,Name\,Only_i + \beta_4 Navajo\,Last\,Name\,Only_i \\ & + \beta_5 Two\,Signals_i + \beta_6 Three\,Signals_i + \alpha_1 Boys\&Girls_i \\ & + \alpha_2 Food\,Bank_i + \alpha_3 Gaelic_i + \mu_o + \mu_c + Controls_i\gamma + \varepsilon \end{aligned} \quad [2]$$

where *Volunteer Only* is an indicator variable for being an Indigenous applicant with the volunteer (Big Brothers and Big Sisters) signal only, *Language Only* is an indicator variable for being an Indigenous applicant with the language signal only, *NH First Name Only* is an indicator variable for being a Native Hawaiian applicant with the first name signal only, *Navajo Last Name Only* is an indicator variable for being a Native American applicant of Navajo ancestry with a Navajo last name only,[36] *Two (Three) Signals* is an indicator variable for any combinations of two (three) signals, *Boys & Girls* is an indicator variable for having the Boys & Girls Club control volunteer experience, *Food Bank* is an indicator variable for having the food bank control volunteer experience,[37] and *Gaelic* is an indicator variable for having the Irish Gaelic control language.

We also extended Equation [2] to split *Language Only* into Native American and Alaska Native versus Native Hawaiian and to split *Two Signals* into all possible combinations. This more saturated model allows us to compare these estimates for more granular signals that match our signal saliency survey, described later.

Table 8 presents the estimates by signal type, from Equation [2] (in column (1)), with the more saturated version in column (2). Here we discuss the results from column (1), but the results in column (2) are similar. Across all variables and columns, the results are never statistically significant and do not suggest any differences by signal.

For Indigenous applicants with the volunteer (language) signal only, the callback rate is 0.6 percentage points lower (higher), but this is statistically insignificant with a standard error of 1.0. The estimates on the controls for volunteer and language experiences are also statistically insignificant, which suggests that regardless of which control volunteer experience, or if the Irish Gaelic control is used, there is no difference in callback rates. Results are also similar for our name signals. For Native Hawaiian first name (Navajo last name) as the only signal, the estimate is a 1.7 (0.7) percentage point lower callback rate, again statistically insignificant.

The estimates with two or three signals are positive but again statistically insignificant. These estimates are imprecise, however, for three signals, given that most resumes had only one or two signals. Thus, there is no evidence to support that having multiple signals decreases the callback rate. The fact that there is no difference in callback rates by Indian

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

[37]The excluded category is the Big Brothers and 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.

reservation upbringing is further evidence that our discrimination estimates do not vary by signal type or by saliency.

Robustness Checks

Here we present a summary of two major robustness checks: weighting our estimates and the effects of correcting for possible differences in the variance of unobservables. In Online Appendices C and D, we present additional details and results for these robustness checks, and we also conduct other robustness checks such as using a probit instead of a linear probability model, alternative methods of clustering, and using interview offers instead of callbacks.

Robustness to Population and Occupation Weighting

We attempted to apply for all eligible job openings that met our criteria in each city and occupation. What would generate more population-representative estimates for Indigenous Peoples would be to weight the estimates by the population distribution of Indigenous Peoples across these cities (Hanson, Hawley, Martin, & Liu, 2016; Neumark, Burn, Button, & Chehras, 2019). Similarly, we can weight by the popularity of occupations according to the CPS data in case our sample by occupation differs from the national data. We can also weight by both. In Online Appendix D, we discuss how we construct these weights, and we present our main results, from Table 6, under different types of weighting. Our results are unaffected by how we weight the data.

Correcting for the Variance of Unobservables using the Neumark (2012) Correction

Audit and correspondence studies, especially resume-correspondence studies like ours, could face the “Heckman-Siegelman critique” (Heckman, 1998; Heckman and Siegelman, 1993). This critique holds that while these studies control for average differences in observable characteristics (information included in the job application), discrimination estimates can still be biased, in either direction, through differences in the variance of unobservable characteristics - which relates to variance-based statistical discrimination (Aigner & Cain, 1977). Neumark (2012) shows how this can occur using a model of hiring decisions, and Neumark and Rich (2016) show that about half of the resume-correspondence studies were biased because of this issue. In Online Appendix C we discuss this in more detail, including with a formal model, and test for this bias.

To summarize, we correct for this possible bias by randomly adding quality features to the applications. As discussed in Neumark (2012) and Online Appendix C, these quality features shift the probability of a callback, allowing us to identify to what extent differences in the variance of unobservables between white and Indigenous applicants lead to bias. We find no evidence of bias in our main results due to the variance of unobservables issue and the estimated variances of unobservables by race are nearly for white and Indigenous peoples.

Implications of our Experimental Design

Here we discuss numerous choices in our experimental design that could have impacted our results: the saliency of our signals, our choice of occupations and cities, our choice of job board, our use of callbacks to measure hiring discrimination, and our sample size. These broader discussions bring attention to the limitations of our experiment and others.

Implications of the Saliency of our Indigenous Signals

A key question in any audit study is whether the tested subjects detected and correctly interpreted the signal(s) of minority status. Usually, this is just assumed. We are only aware of a few audit studies that carefully test for saliency and interpretation of their signals (Kroft, Notowidigdo, & Lange, 2013; Lahey & Oxley, 2018). If the signal is only detected sometimes, then results are attenuated towards zero. If the signal is interpreted differently than intended, then the results may not reflect what the experimenters expect to test (Barlow & Lahey, 2018; Darolia et al., 2016; Fryer & Levitt, 2004; Gaddis, 2017a, 2017b; Ghoshal, 2019).

We use four different signals in our study (volunteer experience, language, Native Hawaiian first name, and Navajo last name). Despite our results not differing by signal type, or when more than one signal is used (Table 8), it still may be the case that each signal has different saliency. To investigate this, we fielded two surveys, both described in more detail in Online Appendix E (“resume survey”) and Online Appendix F (“names survey”).

First, we fielded the resume survey, a survey similar to Kroft, Notowidigdo, and Lange (2013). Specifically, we asked individuals on MTurk to read a resume from our study and to consider the candidate for a job position in the relevant occupation. We then asked the subjects to recall characteristics of the applicant (race or ethnicity, languages spoken, age, education, employment status). We included surveys showing resumes without signals (white) or with some combination of signals for either Native American or Native Hawaiian applicants. We included respondents from both a national sample and an Arizona and New Mexico only sample for the Navajo resumes since we primarily send Navajo resumes to positions in Albuquerque and Phoenix.

Table 9 summarizes our main results which combine both samples, with additional results and details in Online Appendix E. To summarize, the white resumes (no signals) are identified as white 86.8% of the time. However, resumes with a Native American (Native Hawaiian) signal were detected as AIAN (NHPI) at rates between 18.8% to 74.2% (26.4% to 82.0%). More specifically, the Navajo last name only signal is very weak (18.8%) compared to the language signal only (32.4%) or the volunteer signal only (37.2%), which are stronger, but still not strong. Saliency is significantly higher when using more than one signal, ranging from 58.0% for Navajo last name and volunteer experience to 74.2% for Navajo last name and Navajo language listed. In the Arizona and New Mexico sample only, saliency was significantly higher, ranging from 58.3% (Navajo last name only) to 76.7% (Navajo last name and Navajo language).

However, the saliency estimates presented in Table 9 are lower bounds for a few reasons. First, the saliency for the Native American resumes may have been higher, namely, for those with the volunteer signal, had we listed “Native American” as a race option, rather than the more “official” Census category of “American Indian or Alaska Native”. Some individuals may not have equated “Native American”, as stated in the volunteer signal, as “American Indian” in the survey question. Second, we add an Indian reservation upbringing to half of the Native American resumes, which further strengthens the racial signal, but our estimates above are for resumes without this additional signal. Third, most Native Hawaiian resumes were sent to jobs in Honolulu, where saliency of would be higher. However, we were unable to perform an oversample of Hawaii, so our saliency estimates for Native Hawaiian names and signals are significantly bias downwards, reflecting more-so how Native Hawaiians are perceived on the mainland.

To compare the saliency of our different types and combinations of signals for Indigenous Status to the discrimination estimates when using these signals, we match the estimates in Table 8, column (2), with their corresponding estimates from Table 9 and plot them in Figure 2 below. Figure 2 shows no clear relationship between saliency and discrimination as the relationship is very flat, or perhaps slightly upward sloping. Thus, this suggests that while our saliency was often (but not always) somewhat low, saliency does not seem to impact our results.

Since the saliency of the Navajo last name signal was the lowest, we also conducted three robustness checks where we: (1) recoded those with Navajo last names as the only signal as “white” and re-estimated Table 6, column (2); (2) controlled for resumes with the Navajo last name only with a separate indicator variable and re-estimated Table 6, column (2); and (3) re-estimated the results in Table 8, column (1), but recoded the signals as if the Navajo last name signal did not exist. As shown in Online Appendix Tables D7 and D8, these tests again do not change our results.

Implications of the Saliency and Perception of our Indigenous Name Signals

Some recent audit study literature discusses or tests how names signal race, ethnicity, and socioeconomic status, finding that individual names may not signal what researchers assume and names can drive results in unexpected ways (Barlow & Lahey, 2018; Darolia et al., 2016; Gaddis, 2017b, 2017a; Ghoshal, 2019). We tested the specific names we used to signal Indigenous status through our resume survey, discussed earlier. We also fielded a second survey on MTurk specifically on our Navajo last names, similar to how Gaddis (2017a, 2017b) and Ghoshal (2019) test names. We simply showed those surveyed a name (e.g., Daniel Begay, Emily Adams) and asked them to indicate the race of the individual (in addition to asking them about other perceptions). We present more details and full results from both surveys in Online Appendix E (resume survey) and Online Appendix F (names survey).

To summarize the names survey, out of the four Navajo last names, saliency was highest for Tsosie (47.1% nationally thought this person was AIAN and 70.0% in Arizona and New Mexico only), followed by Yazzie (12.5%, 28.6%), Begay (10.0%, 35.0%), and Benally (5.7%, 15.0%).[38] We also learn from both our surveys that individuals perceive Indigenous

Peoples to be more likely to have been born outside the United States - an odd result, but one seen in other research using the Native Implicit Association Test.[39] Given that our Navajo names are less salient, it highlights the trade-offs between saliency and representativeness (see the discussion on page 14) in using our chosen names and more "stereotypical" names (e.g., Whitebear).

Implications of the Saliency of Other Resume Features

Following Kroft, Notowidigdo, and Lange (2013), we measured the saliency of other frequently-used signals: gender, age, education, employment status, employment duration, and whether a second language was listed on the resume. More details are in Online Appendix E.

Across all tested resumes, survey respondents correctly recalled gender from our gender-specific names 71.4% of the time, highest completed education 86.4% of the time, employment status 68.3% of the time, and correctly recalled whether there was a second language on the resume 75.3% of the time. The mean age in years (or duration of the last job held in years) minus actual was −1.60 years (−0.90 years), with a standard deviation of 4.69 years (3.15 years).

These results suggest that one should never assume that signals, even ones like employment status, will always be detected. Researchers should test the saliency of their signals in all contexts and discuss how this affects their results. The fact that not all signals are always detected suggests that the estimates in most audit field experiments are lower bounds.

Implications of our Choice of Occupations

We chose common occupations for those age 30. These positions do skew more low-skilled or lower-experience relative to some other possible occupations, although this is a broader concern facing resume-correspondence studies (Baert, 2018; Neumark, 2018). A key question is if there is discrimination in occupations that we do not study. We discuss arguments on both sides. First, there are arguments that our chosen occupations are likely to have *more* discrimination than others, so our chosen occupations did not give us our "no differences" result. Second, there are arguments that our occupations would have less discrimination, suggesting that perhaps our "no differences" result is a function of our occupations and is not more broadly generalizable. After this discussion, we leave it to the reader to decide the extent to which our occupations drove our result.

An argument against the claim that our occupations drove our result is the fact that previous audit studies found discrimination in these specific occupations, albeit for different minority groups. Numerous studies used retail sales, server, and kitchen staff positions and found

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

[39]See https://implicit.harvard.edu/implicit/ (accessed February 22, 2020). In the names survey, those with white names are seen as having been born in the United States 92.1% of the time in the national sample (96.0% of the time in the Arizona and New Mexico sample), relative to 64.8% for those with Navajo last names (72.3% in the Arizona and New Mexico sample.).

discrimination (Baert, 2018; Neumark, 2018). Neumark, Burn, & Button (2016, 2019) also apply for janitor and security jobs and find some evidence of discrimination.

Another argument against the claim that our occupations drove our result is that Helleseter, Kuhn, & Shen (2014) and Kuhn & Shen (2013) find that discrimination is more likely in low-skilled occupations than in higher-skilled jobs. Our occupations are lower-skilled, suggesting that higher-skilled occupations could be even less likely to have discrimination.

However, there are arguments that our chosen occupations are ones where discrimination is less likely to occur, such that we may have found discrimination had we chosen other occupations. First, our occupations may be "typed" to be for Indigenous Peoples or are otherwise more "friendly" to Indigenous Peoples. Sociology research suggests that individuals sometimes "type" jobs as being more suitable for individuals of certain races or genders (Kaufman, 2002). While we found no research on this typing for Indigenous Peoples, we do not think that Indigenous Peoples are "typed" into retail sales or server positions. In these occupations, there is a significant amount of customer interaction such that customer taste-based discrimination, if it exists, may cause a preference for white employees. Typing, however, may be relevant for kitchen staff, janitor, and security jobs. For kitchen staff, there is the potential notion that people of colour are more likely to be "back of the house" (kitchen) than "front of the house" (servers, hosts, bartenders) staff, and this manifests in the CPS data (Tables 2 and 3). However, discrimination does not appear to vary by the five occupations we study (Tables 7 and Online Appendix Tables D3 and D4), suggesting that this concern likely does not affect our results.

A related issue is that typing could vary by city based on the size of the Hispanic population, as certain jobs may be typed as more or less "Hispanic".[40] In Online Appendix Table D14, we show estimates by the relative size of the Hispanic population in each occupation-city-gender combination. We used this information to re-estimate our main results (Table 6, Column (2)) excluding occupation-city-gender combinations where Hispanics outnumber whites. Our results, available upon request, are unchanged.

Implications of our Choice of Jobs Within our Occupations

There is also the related question of whether the jobs we applied to, within our occupations, were ones where discrimination does not occur. Kuhn & Shen (2013) find that almost a third of the variation in age and gender discriminatory job ads in China occurs between firms in the same occupation, which suggests that a non-representative sample of jobs from an occupation could drive a particular result in an audit study.

It is difficult to know whether our jobs are representative of all those within an occupation. While we use a popular job board, especially for these occupations, no job board is representative. Our job board skews towards smaller firms since larger firms either post job ads elsewhere or require submissions through their website, regardless of posting site. We argue that these smaller firms could be even more likely to discriminate because they are less likely to have Human Resources departments and are less likely to be covered by Title VII

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

of the Civil Rights Act (which applies to firms with at least 15 employees). However, it is unclear whether our selection of jobs is representative of our occupations, a broader concern facing all audit studies.

Implications of our Choice of Cities

Our selection of 11 cities focuses on those with the most Indigenous Peoples to get more population-representative estimates. Despite covering more cities relative to most resume correspondence studies, discrimination may vary geographically in ways we cannot account for.

A related concern is that where Indigenous Peoples live could be endogenous to the geographical distribution of discrimination.[41] Suppose, as many have suggested to us, that Indigenous Peoples face more discrimination in rural areas relative to in urban areas, especially rural areas near reservations. We cannot capture this because we do not include rural areas.

We attempted to account somewhat for these differences in geography by including Billings and Sioux Falls, which are cities with a much smaller population in more rural states. However, our analysis of these small cities is underpowered as it was challenging to collect observations (e.g., few eligible job postings), a common issue in studies with small cities (see, e.g., Neumark, Burn, Button, & Chehras, 2019).[42] Even then, these smaller cities might not represent rural areas enough. Given this, we cannot comment on whether discrimination differs in urban versus rural contexts. Similarly, we cannot speak to discrimination on a reservation.

Our selection of cities also does not allow us to explore whether discrimination differs in cities that neither have large Indigenous populations nor have a high percentage Indigenous. Numerous cities (e.g., Salt Lake City, Indianapolis, Atlanta) fit into this broad category. It is unclear whether discrimination in these cities differs from the larger cities in our experiment (New York, Los Angeles, Chicago, and Houston), although we believe this to be unlikely.

A trade-off we faced in our selection of cities was balancing population-representativeness with increasing the variation in our chosen cities to test for moderators of discrimination. While our selection of cities incorporates significant variation in the proportion AIAN,[43] we could have instead swapped out some of our chosen cities (e.g., Houston) with other cities that, while having a lower number of Indigenous Peoples, have a higher proportion. This would have improved our ability to test how discrimination varies by the proportion Indigenous, at the cost of population representativeness. These trade-offs are essential for researchers to consider.

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[41]For example, many Native Americans moved from urban areas to reservations during the Great Recession, which provides some evidence for this effect if discrimination increased during this time (Feir & Gillezeau, 2018).

[42]It may not even be possible to apply for jobs in some rural areas as many popular job boards, such as the one we used, have job boards by city, so rural areas may not be covered, or online job boards are simply not used in these areas.

[43]Out of all 282 cities with a population greater than 100,000, the proportion AIAN in our cities ranges from one the lowest values, at 1.4% AIAN alone or in combination in New York, to the highest, at 12.4% in Anchorage. We also include four of the cities within the top ten highest proportion of the population that is AIAN alone or in combination (Anchorage, 12.4%, 1st; Oklahoma City, 6.3%, 4th; Billings, 6.0%, 5th; Albuquerque, 6.0%, 6th) plus and Sioux Falls (3.6%, 13th) (Norris et al., 2012).

Implications of the Timing of the Study and Labour Market Tightness

Discrimination could occur more often when economic conditions are worse (Baert, Cockx, Gheyle, & Vandamme, 2015; Johnston & Lordan, 2016; Kroft et al., 2013; Neumark & Button, 2014) although there is also evidence that discrimination decreases when economic conditions are worse (Carlsson, Fumarco, & Rooth, 2018). Therefore, resume-correspondence studies could generate larger (smaller) discrimination estimates during a downturn (a boom) in labour markets. We investigate this in two ways. First, we compare the timing of our study to all other employment audit studies conducted in the United States listed in Baert (2018) or Neumark (2018)'s reviews of the literature. This comparison informs whether discrimination estimates are a function of national economic cycles. Second, we follow the approaches of Kroft, Notowidigdo, & Lange (2013), Baert et al. (2015), and Carlsson et al. (2018) and explore how cross-sectional differences in labour market tightness by city and occupation relate to our discrimination estimates.

To see how discrimination estimates from different studies in the United States varied based on national economic cycles, we created Online Appendix Table D15, which presents the timing of data collection in each study and matches this with the national, seasonally adjusted unemployment rates during that time. This table shows that our study was during a time (March to December 2017) with lower unemployment rates of 4.1 to 4.4 per cent (16th to 24th percentile of the seasonally adjusted rate from 1948 to 2018,). This percentile range with the ranges faced by Pager (2003) (23rd to 56th percentile) and Kleykamp (2009) (21st to 35th), both which find statistically significant effects, although under different contexts. The unemployment rates during our study were not as extreme as over a third of the other studies which occurred during the Great Recession, where unemployment rates reached record highs. This comparison of previous studies and the national unemployment rates they faced does not provide clear evidence that lower unemployment rates drove our results, but it does not rule this out.

Another way to investigate whether discrimination depends on labour market tightness is to use cross-sectional labour market tightness; that is, seeing whether discrimination varies by the tightness of labour markets by city and occupation. We follow Kroft, Notowidigdo, & Lange (2013) and construct two variables that measure labour market tightness. First, we estimate the unemployment rate for each city and occupation combination using data from the CPS.[44] Second, we use the callback rate by occupation and city for our white applicants as a measure of tightness. Online Appendix D presents the details of our methodology and Online Appendix Tables D17 and D18 present out results. We do not find that this cross-sectional tightness of occupation and city affects our results, regardless of which measure of tightness we use.

[44]For employed (unemployed) individuals, occupation refers to their current (most recent) job with the most hours. We use data from January 2010 to December 2019.

Do Callbacks Capture Hiring Discrimination?

There is the recurring question of whether callbacks capture hiring discrimination. Many others discuss this issue (e.g., Booth, Leigh, & Varganova, 2012; Cahuc, Carcillo, Minea, & Valfort, 2019; Neumark, Burn, & Button, 2019). There are again arguments on both sides.

There are many reasons that callbacks capture a significant share of hiring discrimination. At the callback stage, is it far less likely that discrimination can be detected or enforced, relative to later when company personnel systems may have more detailed records of applicants (Neumark, Burn, & Button, 2019). At this callback stage, employers are also more likely to make quick decisions and fall victim to implicit bias (Bertrand, Mullainathan, & Chugh, 2005; Rooth, 2010). Audit studies that have actors go to interviews show that 75% to 90% of hiring discrimination occurs at the callback stage (Bendick, Brown, & Wall, 1999; Riach & Rich, 2002).

However, recent work by Cahuc et al. (2019) argues that callbacks can fail to capture hiring discrimination when interviewing costs are low. Cahuc et al. (2019) uses a model of hiring and interviewing behaviour to show that as the cost of interviewing decreases, callbacks become less able to capture hiring discrimination as discrimination shifts to after the interview stage. Since we study the case where Cahuc et al. (2019) argue that interviewing costs are high - private-sector jobs in competitive industries - we believe that the hiring costs are high such that their critique does not apply much to our study. This explanation also does not explain how our results differ from the other resume studies, almost all of which were for private-sector jobs.

We argue that we capture a stage where a significant proportion of hiring discrimination usually occurs. However, like other studies, we cannot claim to capture all hiring discrimination. Future work on discrimination beyond the callback stage would be useful.[45]

The Effect of Statistical Power on our Results

A possible reason generally for a lack of statistically significant results is low power, but this is not a problem we face for our main results. As discussed earlier, we have significantly more observations than our power analysis required, and we have the seventh-largest sample size relative to the other 113 resume-correspondence studies of hiring discrimination summarized in Baert (2018) and Neumark (2018).[46] Our standard errors, in many cases, are also precise enough to rule out meaningful amounts of discrimination in our main results, suggesting that even if there is differential treatment, it is uncommon.[47] Of course, our results are not precise enough to rule out discrimination in every circumstance. For example,

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[45]For example, a referee shared that there is anecdotal evidence that some Native Americans have experienced discrimination at the interview stage depending on their skin colour or how “white” they appear or seem.

[46]The studies with more job applications than us are: Neumark, Burn, & Button (2019); Agan & Starr (2018); López Bóo, Rossi, & Urzúa (2013); Maurer-Fazio (2012); Maurer-Fazio & Lei (2015); and Zhou, Zhang, & Song (2013). Our records of the sample sizes (applications sent, unique jobs) for each study are available upon request.

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

we cannot rule out discrimination in small cities, such as Billings and Sioux Falls, and other comparisons that involve small cells (e.g., Navajo last name signal) are underpowered.

Decomposition of Disparities in Economic Outcomes using Survey Data

Our field experiment shows no evidence of discrimination in callbacks in the cities, occupations, and timeframe we studied. However, a complementary way to study discrimination is through a decomposition of survey data on economic outcomes, such as an Oaxaca-Blinder decomposition (Jann, 2008; Oaxaca & Ransom, 1994; Yun, 2004, 2005) or a more modern Gelbach decomposition (Gelbach, 2016). While a decomposition cannot make both groups on-average identical, making it challenging to isolate discrimination, the approach has some benefits. These include exploring discrimination in broader contexts than an audit field experiment (e.g., other occupations and geographies), providing information about which factors relate to disparities, and allowing for a decomposition of different types of employment-related variables. More specifically, a decomposition allows us to decompose wages, unemployment rates, unemployment durations, and employment durations. The latter is important since our audit field experiment can say nothing about whether Indigenous Peoples face discrimination in firing or employment length.

Data

For data, we used monthly IPUMS-CPS data from 2013 to 2017 (Flood, King, Rodgers, Ruggles, & Warren, 2018) for log weekly wage, unemployment rate, and unemployment duration. Unfortunately, employment duration at the current job is not available as a part of the regular CPS monthly survey. Instead, we use data on employment duration from the IPUMS-CPS Job Tenure Supplement for the years 2014, 2016, and 2018.

Methodology

We conduct a Gelbach decomposition (Gelbach, 2016), a new approach which nests the Oaxaca-Blinder decomposition. The Gelbach decomposition improves on the Oaxaca-Blinder decomposition by allowing for standard error estimates derived from Gelbach's (2016) asymptotic variance formulas. This avoids the common practice of providing Oaxaca-Blinder decomposition results without standard errors. To conduct the Gelbach (2016) decomposition, we use the Stata code "b1×2" (Gelbach, 2014) provided by the author. We decompose gaps in economic outcomes into an explained portion, explained by observable factors such as education, occupation, and geography, and into an unexplained (residual) portion, which could reflect discrimination. We expand on prior wage decomposition studies (e.g., Baldwin & Choe, 2014a, 2014b; Feir, 2013; Gardeazabal & Ugidos, 2004; Hurst, 1997; Krishna & Ravi, 2011; Kruse et al., 2018; Kuhn & Sweetman, 2002) by also decomposing gaps in unemployment rates and unemployment durations, rather than just gaps in wages, given that unemployment rates and durations relate more directly to the callback discrimination we estimate in our resume correspondence experiment.[48] We also extend prior studies by also studying employment durations, which allows us to investigate whether there could be discrimination in firings, a factor that audit field experiments cannot quantify. More details of our methodology are in Online Appendix G.

Results

Tables 10 and 11 present our estimates from a Gelbach (2016) decomposition of log hourly wages, unemployment rates, unemployment duration and employment duration (both in weeks) for AIANs vs whites (Table 10) and NHPIs vs whites (Table 11). In both tables, we compare those who identify as AIAN (NHPI) alone with those who identify as white, non-Hispanic, alone, but our results, available upon request, are similar if we use "alone or in combination" for AIAN and NHPI. Our results are also similar using Oaxaca-Blinder decompositions (Online Appendix G).

To summarize our results in Tables 10 and 11, we find that for AIANs, most of the raw gap in hourly wages (a 14.5% gap) is explained by lower educational levels and lower-paying occupations, leading to a small unexplained gap (1.2%).[49] For NHPIs the raw gap in hourly wages is smaller (8.7%) and is explained by education and occupations but is offset by differences in the state of residence, whereby wages are higher in Hawaii. In net, there is a larger and statistically significant unexplained gap (4.1%). These results suggest the potential for minimal amounts of wage discrimination against AIANs (when controlling for occupations) and the potential for some wage discrimination against NHPIs.

For unemployment rates, the raw gap of a 4.5 percentage point higher unemployment rate for AIANs is almost entirely unexplained (4.3 percentage points unexplained). For NHPI, the raw gap is smaller (1.7 percentage points) but is partially explained (a statistically significant 0.7 percentage points left unexplained).

However, for unemployment durations, the evidence differs for AIANs and NHPIs. Both AIANs and NHPIs have negative raw gaps, suggesting shorter unemployment durations (1.7 weeks shorter for AIANs, 2.9 weeks shorter for NHPIs). After the decomposition, the unexplained gap in unemployment durations is a now a 1.6-week *longer* duration for AIANs. For NHPIs, this gap of shorter unemployment durations is left almost entirely unexplained.

Tables 10 and 11, column (4), present the results for employment duration. Without the decomposition, AIANs (NHPIs) have employment durations that are 124.8 weeks (153.6 weeks) shorter, on average, than non-Hispanic whites, who have a duration of 447 weeks. However, after the decomposition, the unexplained portion is an employment duration that is 17.4 weeks shorter for AIAN and 15.5 weeks shorter for NHPI, but neither of these small unexplained portions is statistically significant. Age differences between non-Hispanic white and Indigenous Peoples explain most of the raw differences in employment duration, as Indigenous Peoples are more likely to be younger, when employment durations are shorter. In summary, this decomposition provides mixed evidence of discrimination. For AIANs, the unemployment results both for unemployment durations and especially unemployment rates

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

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

point consistently towards potential hiring discrimination. However, we do not find unexplained gaps in wages or employment durations, suggesting that discrimination is less likely beyond the hiring stage. We do find that occupations explain a large part of the wage gap, which implies that AIANs are in less well-paying occupations. This could be additional evidence of discrimination to the extent that discrimination affects occupation.

For NHPIs, the evidence of hiring discrimination is more mixed, since the unexplained gap in unemployment rates is slightly higher for NHPIs compared to whites, but the unexplained gap in unemployment durations is shorter for NHPIs. There is again no unexplained gap in employment duration, but now there is an unexplained gap in wages, which increases when we do not control for occupations. This suggests possible wage discrimination.

Comparing our Decomposition to our Experiment

There are two possible explanations for why our decomposition results, namely for AIANs and for unemployment rates and duration, differ from the results of our field experiment. First, there is the standard criticism that unexplained gaps in decompositions are not necessarily evidence of discrimination, due to the inability to control for all factors. These unexplained gaps show the upper-bound of discrimination (*potential* discrimination). The most relevant uncontrolled factor could be reservation wages, as this may affect who stays in the labour market or accepts jobs.

Conversely, it is possible that hiring discrimination does exist and is picked up to some extent by this decomposition, but it is missed entirely by our experiment. As discussed above, our audit field experiment is only a case study: discrimination among five common occupations (retail sales, server, kitchen staff, janitor, and security) in 11 cities for applicants who have only a high school education and are of about age 30 and we cannot entirely rule out that there could be discrimination in other occupations or contexts.

To better understand whether the results from our decompositions reflect potential discrimination outside of our case study, we re-ran our decompositions where we restricted the sample to include only observations that better aligned with the sample in our experiment.[50] Our results, available upon request, are relatively unchanged in these restricted samples, suggesting that contexts outside our field experiment are not driving the potential discrimination we see in the decompositions. We see it is far more likely that the unexplained higher unemployment rates (and durations for AIAN) reflect uncontrolled factors rather than us missing discrimination in our sample. However, a more thorough decomposition and analysis, similar to Hurst (1997), Feir (2013), Kuhn and Sweetman (2002), or Krishna and Ravi (2011), would be helpful to provide additional evidence but is beyond the scope of this paper.

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

Conclusion

Our results from a large-scale field experiment of hiring discrimination where we sent 13,516 job applications of on-average identical applicants who were either Indigenous or white to jobs as retail salespersons, servers, kitchen staff, janitors, or security guards show no difference in callbacks between Indigenous Peoples and whites. We also do not find bias against Native American applicants from Indian reservations. We do not find differences in callbacks even when we estimate separately by our 11 cities, five occupations, or by the intersection of occupations and gender. These results differ from most other resume correspondence studies, which usually find discrimination (78.4% of the time, according to Baert [2018]). However, it appears that experiments that find no discrimination are less likely to be published (Zigerell, 2018)

Our results are robust in several ways, including to how we signal Indigenous status, to functional form assumptions (linear probability model versus probit), to the Neumark (2012) correction for potential bias from the variance of unobservables, to how we weight the regressions, to how we code callbacks, and to how we cluster our standard errors.

However, we discuss how our results could potentially be affected by the relatively better economic conditions at the time of the experiment, the saliency of our signals, the cities and occupations we selected, and the type of employers that posted to the job search website we used. These factors could lead to more positive, or, perhaps more likely, more negative estimates of discrimination if the experiment were in a different context or were done differently. For these reasons, we must be clear that our audit field experiment does not prove that Indigenous Peoples do not face hiring discrimination at all.

We also conduct Gelbach and Oaxaca-Blinder decompositions of disparities in wages, unemployment rates, unemployment durations, and employment durations. We find that AIANs (NHPIs) have an unemployment rate that is 4.3 (0.7) percentage points higher than what can be explained by controlling for observable differences between AIANs (NHPIs) and non-Hispanic whites. There is weak evidence that AIANs (NHPIs) face slightly longer (shorter) unexplained unemployment durations. For AIANs, we do not find an unexplained gap in wages, but we do find one for NHPIs (4.1% lower wages). However, for both groups, this wage gap increases when not controlling for occupation, suggesting that Indigenous Peoples are more likely to work in occupations that pay less. This occupational segregation that negatively affects wages could be due, in part, to discrimination, although it is difficult to know without further study. These results provide some evidence of discrimination in hiring, through the higher unexplained unemployment rates and wage gaps, although these results are subject to the critique that decomposition studies cannot control for all differences between groups.

We hope that readers can learn quite a bit about audit study methodology from this paper. There are three main takeaways. First, it is unclear to what extent macroeconomic cycles and labour market tightness affect our results or the results of other studies. Future work on this topic would help us both understand the mechanisms of discrimination and help us interpret the results of previous work and this paper. Second, we learn from this paper and other recent work (Barlow & Lahey, 2018; Darolia et al., 2016; Gaddis, 2017a, 2017b; Ghoshal, 2019) that it is crucial to test the saliency and perception of signals. Signals may not capture what the researchers think, and signals may not be that salient, even if they are signals that seem obvious like employment status on a resume (this was recalled only about two-thirds of the time in our survey). The fact that signals are not always detected may imply that in most studies, the discrimination estimates are lower bounds. Third, we propose or highlight several additional robustness checks that we hope will become standard, regardless of if the study finds discrimination or not.

We also make some contributions to the methodology of decomposition studies, by being one of the first to use the Gelbach (2016) decomposition, and by exploring economic disparities other than just in wages: unemployment rates, unemployment durations, and employment durations to get a fuller picture of economic disparities, and to better compare the results of a decomposition with a resume correspondence study.

This study is one of the first, and few, to explore quantitatively the extent to which Indigenous Peoples face discrimination. Future work can explore this in many ways. First, our case study cannot rule out hiring discrimination in all circumstances, so future researchers could continue to investigate whether hiring discrimination occurs in other circumstances, such as during recessions, in smaller cities, rural areas, or areas near reservations, or in different occupations.

Second, employment discrimination could occur in wages, firing, and employment duration, all of which our audit field experiment does not capture. Our Gelbach and Oaxaca-Blinder decompositions using Current Population Survey data provide some suggestive evidence of hiring discrimination, discrimination that affects occupational choice, and some potential discrimination in wages. Despite the challenges that these decompositions have at isolating discrimination, they go beyond audit field experiments, which are narrower case studies. More detailed decompositions of the economic disparities faced by Indigenous Peoples, along the lines of Feir (2013) for Canada, but incorporating the additional economic outcome variables we explore, would be helpful.

Third, discrimination can occur more broadly than just in employment, as shown in audit field experiments of discrimination in access to health care (e.g., Kugelmass, 2018; Wisniewski & Walker, 2020), housing (e.g., Ameri, Rogers, Schur, & Kruse, 2018; Hanson et al., 2016; Oh & Yinger, 2015), local government services and taxation (e.g., Giulietti, Tonin, & Vlassopoulos, 2019; Schwegman, 2020), political representation (e.g., Butler and Broockman 2011), and in education (e.g., Francis, De Oliveira, & Dimmitt, 2019). There are a few non-experimental studies that uncover disparities or suggest discrimination against Indigenous Peoples in these other contexts such as in policing (Gorsuch & Rho, 2019), access to credit (Jorgensen & Akee, 2017), housing and institutionalization (Feir & Akee, 2018), in business and economic development (Akee & Jorgensen, 2014), and in health and health care (e.g., Gone & Trimble, 2012; Jones, 2006). However, more research, of which audit field experiments are an essential part, is needed to fully understand to what extent Indigenous Peoples face discrimination more broadly. This would be an important part of a larger research agenda focused on the challenges and opportunities faced by Indigenous

Peoples, a research agenda that is crucial given the shortage of quantitative social science research on Indigenous Peoples (Feir & Hancock, 2016).

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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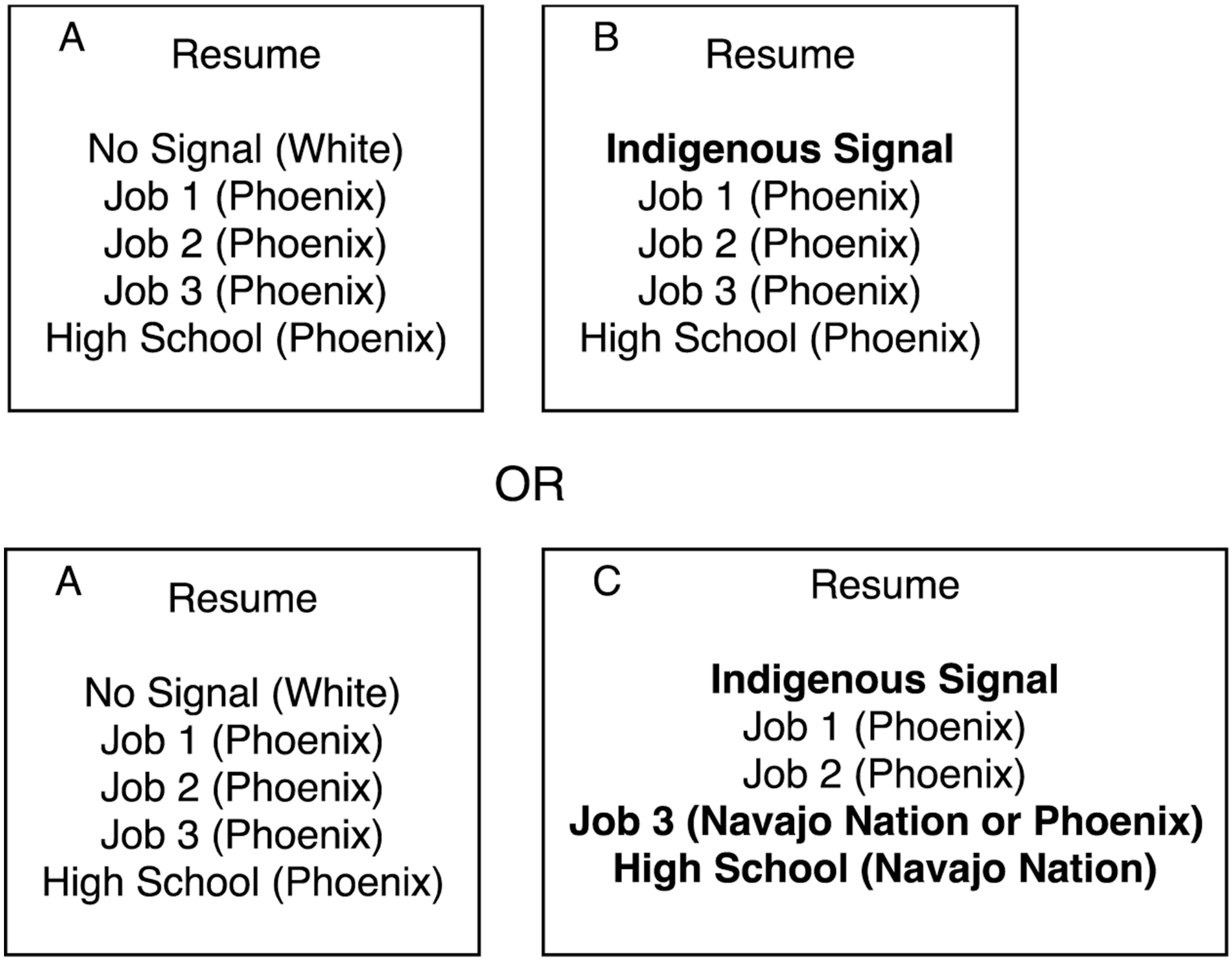

Figure 1 - Example of Pairs of Applicants for Jobs in Phoenix with Navajo Applicants

Notes: We always sent the A–B pair when the Indigenous applicant was Native Hawaiian or Alaska Native. For pairs with a Native American applicant, we used the A–B or A–C pairs with equal probability. Of the A–C pairs, half have Job 3 for resume type C be a job on the Indian reservation while the other half have the equivalent job in the local city as in resume type A.

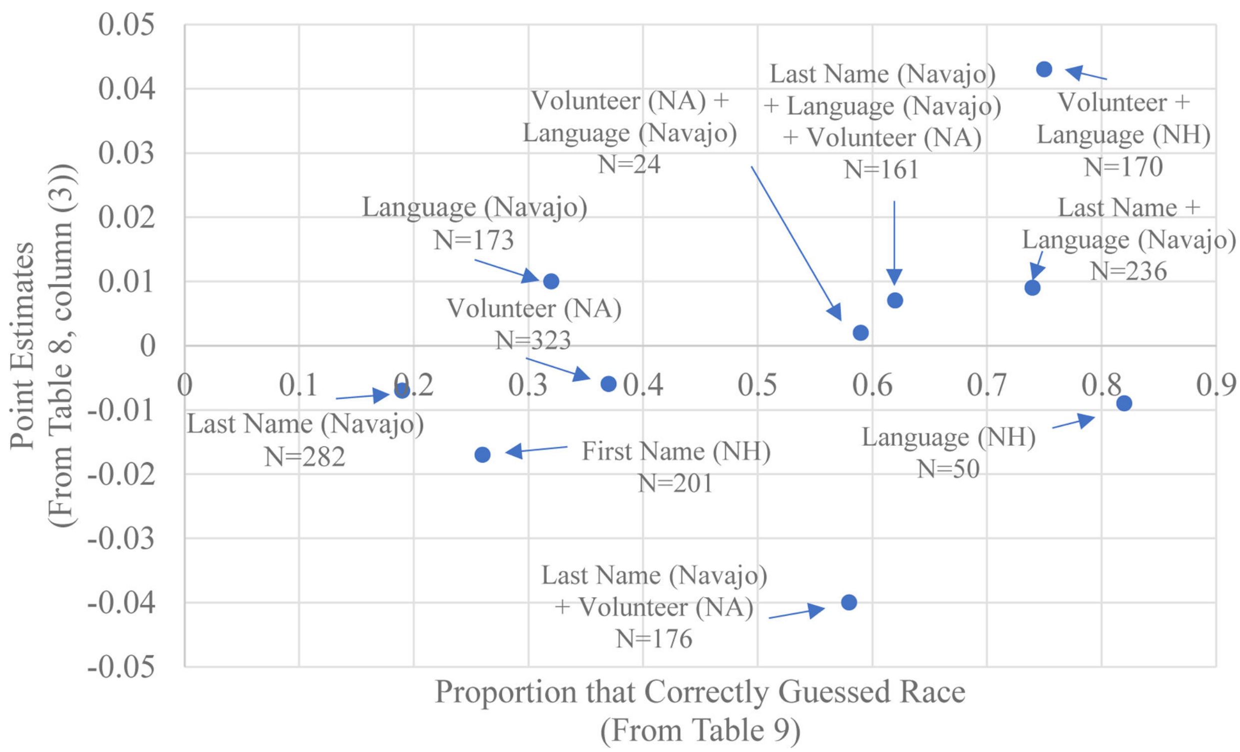

Figure 2 - Signal Saliency vs Discrimination Point Estimates

Note: the size of each dot corresponds to the number of applicants that use that signal in our experiment, as shown in Table 8, column (2), above.

Table 1 -

Summary of Possible Racial Signals by Indigenous Group

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

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

Table 2 -

Frequency of our Selected Occupations for Men, by Race

Occupation (Rank)	Proportion of Entire Race			Ratio to White	
	White	AIAN	NHPI	AIAN	NHPI
Retail salespersons 41–2031 (#5) (*retail sales*)	2.18%	0.83%	0.46%	0.0119	0.0020
Grounds maintenance workers 37–3010 (#6) (*janitor*)	2.06%	2.36%	2.11%	0.0359	0.0097
Cooks 35–2010 (#9) (*kitchen staff*)	1.65%	3.73%	2.51%	0.0707	0.0144
Janitors and building cleaners 31–201X (#10) (*janitor*)	1.49%	1.68%	2.00%	0.0355	0.0128
Waiters and waitresses 35–3031 (#24) (*server*)	0.94%	0.57%	0.08%	0.0189	0.0008
Cashiers 41–2010 (#31) (*retail sales*)	0.84%	1.26%	0.50%	0.0469	0.0056
Security guards and gaming surveillance (#37) (*security*)	0.74%	1.44%	2.74%	0.0614	0.0353

Notes: Data come from all months of the 2015 Current Population Survey. Estimates are weighted using person-level sampling weights. Occupations are ranked based on the decreasing share of white men that have this occupation out of all white men. White corresponds to those who report that they are white only, while AIAN (NHPI) correspond to those who report AIAN (NHPI) either alone or in combination with another race. Our sample includes those aged 25 to 35 only. Ratio to white presents the number of AIAN (NHPI) individuals in the occupation for each white individual. In parentheses is the larger occupation grouping to which this finer CPS occupation belongs. See Online Appendix A and Online Appendix Table A3 for a larger table with additional occupations.

Table 3 -

Frequency of our Selected Occupations for Women, by Race

Occupation (Rank)	Proportion of Entire Race			Ratio to White	
	White	AIAN	NHPI	AIAN	NHPI
Cashiers 41–2010 (#4) (*retail sales*)	2.65%	3.30%	3.25%	0.0503	0.0113
Waiters and waitresses 35–3031 (#5) (*server*)	2.65%	0.80%	0.47%	0.0122	0.0016
Retail salespersons 41–2031 (#8) (*retail sales*)	2.00%	1.94%	1.50%	0.0391	0.0069
Cooks 35–2010 (#27) (*kitchen staff*)	1.00%	1.11%	1.81%	0.0449	0.0167
Bartenders 35–3011 (#34) (*server*)	0.81%	0.32%	0.86%	0.0161	0.0098
Janitors and building cleaners 31–201X (#38) (*janitor*)	0.75%	0.40%	1.03%	0.0217	0.0127

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

Table 4 -

Applicant Types Sent by City

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

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

Table 5 -

Mean Callback Differences by Indigenous Status

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

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

Table 6 -

Callback Estimates by Race and Indian Reservation Upbringing

	No Controls (1)	Regular Controls (2)	Full Controls (3)
Native American (β_1)	−0.011 (0.010)	−0.004 (0.009)	−0.005 (0.009)
… x Reservation (β_2)	0.000 (0.015)	−0.000 (0.012)	−0.000 (0.012)
… x Reservation x Reservation Job (β_3)	0.022 (0.020)	0.006 (0.016)	0.005 (0.016)
Alaska Native (β_4)	0.052** (0.026)	0.005 (0.035)	0.003 (0.035)
Native Hawaiian (β_5)	0.012 (0.013)	−0.003 (0.013)	−0.002 (0.013)
Non-Reservation Rural (α_1)	−0.038** (0.016)	−0.016 (0.013)	−0.015 (0.013)
… x Rural Job (α_2)	0.018 (0.023)	0.002 (0.018)	0.002 (0.018)
Occupation and city fixed effects:	No	Yes	Yes
Callback Rate for White:		19.8%	

Notes: N=13,516. Standard errors are clustered at the resume level. Significantly different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). The regular controls are indicator variables for employment status, added quality features (Spanish, no typos in the cover letter, better cover letter, and two occupation-specific skills), gender, resume sending order, and volunteer experience. The full controls include the regular controls plus the graduation year (we randomize between two years), the start month of the oldest job (job 3), the gap (in months) between job 3 and job 2, the gap between job 2 and 1, the duration of the volunteer experience (in months), and indicator variables for the naming structure for the resume, the version of the e-mail script, the formatting of the e-mail, the structure of the subject line in the e-mail, the opening greeting in the e-mail, the structure of the e-mail, the structure of the e-mail signature, the domain of the e-mail address, the voicemail greeting. We also tested for $\beta_2 = \alpha_2$ ($\beta_3 = \alpha_3$) in column (2) and found that these were not statistically different, with a p-value of 0.2830 (0.9186).

Table 7 -

Discrimination Estimates by Occupation

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

Notes: N=13,516. See the notes to Table 6. Regressions include the “Regular Controls” and occupation and city fixed effects from Table 6 (Column (2)). Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*).

Table 8 -

Discrimination Estimates by Indigenous Signal Type

	(1)	(2)
Volunteer Only (β_1) (n = 3,029)	−0.006 (0.010)	−0.006 (0.010)
Language Only (β_2) (n = 1,723)	0.006 (0.010)	…
Language Only (Native A. or Alaska N.) (n = 1,356)	…	0.010 (0.012)
Language Only (Native Hawaiian) (n = 346)	…	−0.009 (0.021)
First Name (Native Hawaiian) Only (β_3) (n = 475)	−0.017 (0.018)	−0.017 (0.012)
Last Name (Navajo) Only (β_4) (n = 222)	−0.007 (0.026)	−0.007 (0.026)
Two Signals (β_5) (n = 823)	0.003 (0.015)	…
Language + Last Name (n = 78)	…	0.009 (0.033)
Volunteer + Last Name (n = 89)	…	−0.040 (0.030)
Language + First Name (n = 112)	…	−0.011 (0.032)
Volunteer + First Name (n = 117)	…	0.023 (0.041)
Volunteer + Language (Native A. or Alaska N.) (n = 578)	…	0.002 (0.019)
Volunteer + Language (Native Hawaiian) (n = 125)	…	0.043 (0.054)
Three Signals (β_6) (n = 92)	0.038 (0.037)	0.007 (0.062)
Boys & Girls Club (Volunteer Control) (α_1) (n = 3,298)	−0.007 (0.009)	−0.007 (0.009)
Food Bank (Volunteer Control) (α_2) (n = 3,460)	−0.006 (0.009)	−0.005 (0.009)
Irish Gaelic (Language Control) (α_3) (n = 831)	−0.017 (0.013)	−0.016 (0.013)

Notes: N=13,516 for the entire sample and the *n* in each row corresponds to the number of resumes with that feature. See also the notes to Table 6. Regressions include the "Regular Controls" and occupation and city fixed effects from Table 6 (Column (2)). Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*).

Table 9 -

Responses to "What is the race or ethnicity of this applicant?" from the Resume Survey

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

Notes: The sample includes both a national sample (no restriction based on the state of residence) and an oversample of Arizona and New Mexico. Estimates are bolded to highlight the signalled race in each case.

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Table 10 -

Gelbach (2016) Decomposition Estimates for AIAN vs White

	Log Hourly Wage (1)	Unemployment Rates (2)	Unemployment Duration (Weeks) (3)	Employment Duration (Weeks) (4)
Total Difference	−0.145*** (0.008)	0.0454*** (0.0019)	−1.705** (0.800)	−124.8*** (12.4)
Explained	−0.133*** (0.008)	0.0027* (0.0015)	−3.313*** (0.636)	−107.4*** (11.6)
Occupation	−0.072*** (0.005)	0.0127*** (0.0001)	0.495** (0.219)	−28.4*** (3.1)
Education	−0.051*** (0.002)	0.0040*** (0.0015)	0.974*** (0.186)	29.7*** (2.7)
State	0.017*** (0.002)	0.0009*** (0.0002)	−1.086*** (0.208)	−14.5*** (1.8)
Hispanic	−0.014** (0.006)	−0.0195*** (0.0015)	−2.466*** (0.518)	−2.2 (8.5)
Age	−0.010*** (0.003)	−0.0004*** (0.0001)	−2.744*** (0.405)	−94.7*** (9.5)
Married	−0.006*** (0.001)	0.0034*** (0.0001)	0.503*** (0.089)	−5.05*** (1.02)
Gender	0.005*** (0.001)	−0.0000** (0.0000)	0.088** (0.041)	0.5 (0.4)
Metro Status	−0.003*** (0.001)	0.0002** (0.0001)	−0.074 (0.083)	−0.9 (0.8)
Potential Experience	0.001 (0.002)	0.0027 (0.0015)	1.582*** (0.322)	9.2*** (3.0)
Survey Timing	0.001* (0.001)	−0.0004*** (0.0001)	−0.304*** (0.103)	−0.6*** (0.2)
Children	−0.000 (0.000)	0.0001*** (0.0000)	−0.282*** (0.074)	−0.3 (0.4)
Unexplained	−0.012 (0.008)	0.0427*** (0.0023)	1.609* (0.917)	−17.4 (13.0)
White Mean	$19.13	0.0374	30.11	447.0
Observations	239,981	2,186,764	81,543	110,355

Notes: Data from IPUMS-CPS monthly data from 2013–2017 (Flood et al., 2018) (2014, 2016, and 2018 for employment duration data, which comes from the CPS job tenure supplement data). Statistically significantly different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). Standard errors are in parentheses. AIANs include only those who identify as AIAN alone, and white includes those who identify as white alone and also not as Hispanic. Results including AIAN in combination are similar and are presented in Online Appendix Tables G2, G3, and G4. We calculate the hourly wage by using the reported wage for those paid hourly and by dividing weekly earnings by the usual hours worked for those not paid hourly. Estimates are weighted using person-level sampling weights.

Table 11 -

Gelbach (2016) Decomposition Estimates for NHPI vs White

	Log Hourly Wage (1)	Unemployment Rates (2)	Unemployment Duration (Weeks) (3)	Employment Duration (Weeks) (4)
Total Difference	−0.087*** (0.013)	0.0170*** (0.0025)	−2.876* (1.564)	−153.6*** (17.4)
Explained	−0.046*** (0.011)	0.0102*** (0.0015)	0.010 (1.120)	−138.1*** (15.3)
Occupation	−0.053*** (0.007)	0.0090*** (0.0006)	0.068 (0.364)	−36.9*** (4.9)
Education	−0.025*** (0.003)	0.0017* (0.0010)	0.984*** (0.339)	20.1*** (3.0)
State	0.049*** (0.003)	0.0017*** (0.0006)	0.694** (0.335)	−9.5*** (3.1)
Hispanic	−0.010* (0.006)	−0.0050*** (0.0013)	0.731 (0.960)	−0.9 (8.4)
Age	−0.018*** (0.004)	0.0004* (0.0002)	−3.461*** (0.581)	−129.3*** (15.0)
Married	−0.002*** (0.001)	0.0016*** (0.0003)	−0.434*** (0.165)	1.9* (1.1)
Gender	0.005*** (0.002)	−0.0000 (0.0000)	−0.209** (0.085)	0.4 (0.7)
Metro Status	0.008*** (0.001)	−0.0001 (0.0000)	0.260*** (0.088)	−3.0*** (0.7)
Potential Experience	−0.000 (0.003)	0.0010 (0.0010)	1.522*** (0.473)	19.5*** (5.8)
Survey Timing	0.003*** (0.001)	−0.0001 (0.0001)	0.151 (0.224)	0.3 (0.3)
Children	−0.000 (0.000)	0.0001** (0.0000)	−0.295** (0.115)	−0.8 (0.6)
Unexplained	−0.041*** (0.012)	0.0069** (0.0028)	−2.887* (1.550)	−15.5 (17.6)
White Mean	$19.13	0.0374	30.11	447.0
Observations	239,981	2,186,764	81,543	109,453

Notes: See notes to Table 11. Statistically significantly different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). NHPIs include those who identify as NHPI alone, and white includes those who identify as white alone and also not as Hispanic. Results including NHPI in combination are similar and are presented in Online Appendix Tables G2, G3, and G4.