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

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Online Appendix A: Additional Details About the Experimental Design

Language as a Racial Signal

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

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

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

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

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

First Names as a Racial Signal

Using first names is a natural way to signal minority status in audit-correspondence studies. This approach is evident and easy for gender (for names that are gender-specific and well-known), but signalling race by name is more complicated. For race, names are used to signal African-American status (e.g., Bertrand and Mullainathan, 2004), Arab, Muslim, or Middle Eastern descent (e.g., Rooth, 2010), Turkish or Moroccan descent (e.g., Baert and De Pauw, 2014), and Asian, Roma, Ashkenazi Jewish, African, Indian, and Pakistani descent, among others (Booth, Leigh, and Varganova, 2012; Fershtman and Gneezy, 2001; McGinnity and Lunn, 2011; Oreopoulos, 2011), and caste (e.g., Siddique, 2011). Using names as a signal improves external validity since names are required. However, first names can signal socioeconomic status in some cases, which some argue (Fryer and Levitt 2004) is the case in studies such as Bertrand and Mullainathan (2004).

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

Last Names as a Racial Signal

For those who identify as AIAN only, AIAN-specific last names are not common, but they are also not unusual. From our Census data, there are 268 last names where at least 80% of those with that name identified as AIAN only. Further, 5.5% of individuals who identified as

AIAN only have one of these 268 last names.[1] A broader list of names where at least 30% of those with the name identified as AIAN only has 660 names, and 11.0% of those who identified as AIAN only have one of these 660 names.

To determine feasible last names, we first extracted a list of 268 last names that met the criteria where at least 80% of the people with those last names identified as AIAN alone. We then narrowed this list to 12 AIAN-specific last names that had at least 0.2 people per 100,000 with that last name. Finally, we selected four last names from this list where we could identify the tribal group (Navajo): Begay (5.96 people per 100,000, 94.98% identified as AIAN alone), Yazzie (5.16, 96.10%), Benally (1.87, 95.99%), and Tsosie (1.80, 96.23%).[2]

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

Another issue with using last names as a signal of race is that they are a weaker signal for women since they may take the last name from her spouse. This is especially an issue given the

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

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

increase in interracial marriages after the 1970s (Fryer 2007). Thus, if discrimination against Native American women occurs less than for men, using the last name as the only signal, then this suggests that discrimination is weaker for women, that this is a weaker signal of race for women, or both. In contrast, using Native Hawaiian first names as the only signal may present a different set of implications. A Native Hawaiian first name and a non-Native Hawaiian last name (although Native Hawaiian last names appear uncommon) may imply that the applicant is multi-racial or it may separately or additionally imply interracial marriage for female applicants. However, we do not find discrimination regardless of gender or the signal used.

Rural Towns as Controls for Indian Reservation Upbringing

As discussed in the main paper, we occasionally had white applicants having attended a high school in a small city in order to control for the fact that employers may prefer applicants who grew up in the local area, or may not like applicants from rural areas, which could affect the interpretation of the Indian reservation signal. Online Appendix Table A2 presents the rural towns that correspond to each city that we apply for jobs in, and that correspond to each reservation. We aimed to select rural towns that were approximately the same distance away as the reservation.

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

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

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

Occupations

Online Appendix Tables A3 and A4 present the full versions of Tables 2 and 3 from the paper.

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

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

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

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

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

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

Phone Numbers and Email Addresses

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

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

Working with Research Assistants on Data Collection

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

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

Sending Out Applications

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

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

Matching Responses to Jobs and Applications

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

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

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

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

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

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

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

Online Appendix B: Pre-Analysis Plan and Power Analysis

Pre-Analysis Plan

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

In this experiment, there is only one outcome – callbacks – so there is little to no risk of a typical data mining issue where a researcher can select a subset of outcome variables that show statistically significant results (Olken 2015). We did, however, pre-specify a few things. First, we specified how we could code callbacks by including ambiguous responses with callbacks (e.g., "We reviewed your application, and we have some questions for you."), as done in previous work (e.g., Neumark, Burn, and Button, 2019). We also chose to pre-specify some control variables and models to avoid less pivotal possibilities of data mining, such as choosing resume control variables or models specifically to affect the results. This sort of decision of which control variables or model to use, and how that could lead to p-hacking or data mining, is not unique to our study by any means. While it is not common to pre-specify these, it has been done before with some benefit (e.g., Neumark, 2001) and we wanted to be upfront about decisions that we knew made the most sense to take beforehand. In this pre-analysis plan we sought to commit to approaches to prevent possibilities of data mining or p-hacking whenever we could while also not tying our hands too much in ways that would negatively affect our ability to conduct this research later (see Olken, 2015, p. 71 and Lahey and Beasley, 2018, for some useful discussion of the costs of pre-analysis plans.) In retrospect, we believe that we struck a good balance, but

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

we did pre-specify a few things that we really should not have (e.g., probit models instead of linear probability models), but this did force us to be transparent about our deviations from our pre-analysis plan and justify those deviations.

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

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

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

Notes: See the notes to Table 6. Regressions include the "Regular Controls" and occupation and city fixed effects from Table 6 (Column (2)). Column (1) uses the first 8,422 observations per our power analysis calculation. Column (2) uses all observations. Significantly different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*).

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

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

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

Notes: N=13,516. See the notes to Table 6. Both regressions include the full controls (Column (3) of Table 6) and city and occupation fixed effects. Column (2) includes the added company indicator variables, which removes the separate effects of reservation job and rural job since it controls for each possible company that could be listed for those. Significantly different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*).

The second minor deviation is in the statistical model that we used to run regressions. We initially specified using a probit, but we later learned that it is problematic to interpret

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

interactions in a probit model (Ai and Norton 2003). For this reason, we switched to presenting the main results from a linear probability model. However, our results are similar using a probit (see Appendix Tables D6 through D9).

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

Power Analysis and Sample Size

A vital aspect of this plan was to conduct a power analysis based on previous studies to determine how many observations would be necessary to detect meaningful differences in callback rates between major resume types. Based on previous studies, we saw differences of about three percentage points in the interview request rate to be likely,[7] and we wanted to be able to detect a difference of at least this magnitude between white and Indigenous applicants. Based on our calculations, we anticipated needing to apply to 4,211 jobs (8,422 applicants) to detect differences in callback rates between white and Indigenous applicants of at least three percentage points.[8] We ultimately decided to collect more data than this to be able to have a higher power,[9]

[7] Bertrand and Mullainathan (2004) had approximately 5,000 observations for four types of applicants, differences in callback rates of 0.03 as statistically significant (their standard errors were 0.01). Neumark, Burn, and Button (2019), which shares some similarities to this study regarding resume construction, had 40,223 observations for eight types and were able to detect similar differences of 0.027, with standard errors of 0.006. Using a restricted sample of just men in sales (5,348 observations), they were able to detect differences of 0.038 (standard error of 0.020) between groups. Lahey (2008) was able to detect even smaller differences (0.016) as statistically significant, with almost 5,000 observations (split between Sarasota area and Boston area, analyzed separately) and two groups (young and old).

[8] In Neumark, Burn, and Button (2019), the average interview rate for younger (white) applicants in retail sales was 24.79%, or 24.28% for security, and 32.08% for janitors. Since we use similar resumes for these applications and a similar application process as in this study, we see a weighted combination of these rates (25.43%, weighted by the

detect differences smaller than three percentage points,[10] or to detect other mediators of discrimination (e.g., reservation upbringing, city demographics, gender, occupation) with enough precision.

number of job ads in that study) as a reasonable approximation to the interview rate we will receive for our white applicants. To detect a three percentage point difference using an exact Fisher two-tailed test requires 3,239 observations per group, given the common values of $\alpha = 0.05$, and $\beta = 0.8$ (Faul, Erdfelder, Lang, and Buchner, 2007). However, this calculation does not take into consideration the inter-correlation between clusters (ICC) that occurs when applications are sent in sets to employers. The process to adjust the sample size given this is outlined in Lahey and Beasley (2016). Using a more liberal (higher) estimate of the inter-correlation of 0.3, this suggests that if employers are sent two applicants for each job ad, then the required sample size is 1.3 times the earlier estimate (4,211 jobs).

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

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

Online Appendix C: Correcting for the Variance of Unobservables Using the Neumark (2012) Correction

Introduction and Theoretical Model

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

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

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

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

If X^{II}_N and X^{II}_W are normally distributed with means of zero and standard deviations of σ^{II}_N and

σ^{II}_{W}, respectively, then the interview offer probability is:

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

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

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

Quality Features

Any resume or applicant feature that shifts the quality of the resume in the eyes of the employer can be used in the Neumark (2012) correction. Of course, one can randomly add quality features using resume randomization tools (Lahey and Beasley, 2018, 2009) and then let the data “speak” about what features, according to the employer, boost quality (Lahey and Beasley, 2018). However, we feel that it is essential to incorporate some quality features beforehand that are believed to affect callback rates, with the goal to ensure that there is enough variation in applicant quality in order for this correction to be sufficiently powered. This is crucial since the Neumark (2012) correction requires significantly more power than the standard uncorrected analysis.

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

For retail jobs, the occupation-specific skills are knowledge of programs used to monitor inventory (VendPOS, AmberPOS, and Lightspeed), the ability to learn new programs, and experience with Microsoft Office applications. For janitor, this is a certificate in using particular machines and a certification in janitorial and cleaning sciences. For security, this is CPR and First Aid and stating that they are licensed in their state. For server, this is CPR, First Aid, and experience with point-of-service (POS) software used in food service. For kitchen staff, this is CPR, First Aid, and a certificate or training in food safety. An example of some of these skills are shown in the resume examples later in this appendix, and additional resumes are available upon request.

Of course, not all added quality features will have a positive effect,[11] and some other randomly added features (e.g., certain employers, template styles) might have positive or negative effects. Neumark (2012) shows the iterative process to select from among the resume features to be used in the Neumark (2012) correction. This mirrors the process outlined in Lahey and Beasley (2018) for letting the data "speak" about which features actually affect callback rates.

Online Appendix Table C1 presents the results of the Heteroskedastic Probit estimation, which uses the Neumark correction to deal with the "Heckman-Siegelman" critique, where there could be bias due to differences in the variance of unobservables by group. We find no evidence of bias in our main results due to this. The estimated variances of unobservables are nearly equal for white and Indigenous applicants for the combined analysis (all occupations) and each occupation separately.[12] Thus, our lack of estimated discrimination is robust to this critique.

[11] For example, Spanish, a college degree, and the occupation-specific skills often boosted interview rates in Neumark, Burn, and Button (forthcoming), while adding typos to the resume (missing periods or commas), volunteer experience, and employee of the month awards did not have positive effects, sometimes having negative ones. Lahey and Beasley (2018) also discuss a similar issue for typos. These differential results by quality element prompted us to choose some different quality elements. We also noticed that typos are less common on resumes themselves but are more common in the emails that job applicants send to submit their resumes, which prompted us to try using typos in the cover letter rather than on the resume.

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

Online Appendix Table C1 – Heteroskedastic Probit Estimates

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

Notes: See Neumark (2012) and Neumark, Burn, and Button (forthcoming) for a discussion of this methodology. See also the notes in Table 6. Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). Regressions include the "Regular Controls" and occupation and city fixed effects from Table 6 (Column (2)). All higher-quality resumes randomly receive all but one of the following quality features: fluency in Spanish as a second language, a more detailed cover letter, the lack of typos in the cover letter, and two occupation-specific skills. The occupation-specific skills for retail included knowledge of programs used to monitor inventory (VendPOS, AmberPOS, and Lightspeed) and experience with Microsoft Office applications; janitor included a certificate in using particular machines and a certification in janitorial and cleaning sciences; security included CPR and First Aid and stating that they are licensed in their state; server included CPR and First Aid and experience with point-of-service (POS) software used in food service; kitchen staff included CPR and First Aid and a certificate or training in food safety.

Online Appendix D: Additional Results and Additional Robustness Checks

Probit vs Linear Probability Model

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

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

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

Notes: N = 13,516. See the notes to Table 6. Regressions include the "Regular Controls" and occupation and city fixed effects from Table 6 (Column (2)). Column (3) presents the main results from Table 6 (Column (2).)

Clustering

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

Dealing with these two possible levels of clustering is not straightforward. Our main results cluster our standard errors on the resume. The difficulty with clustering on the job, however, is that we cannot match all responses perfectly to job ads.[13] However, for the pairs of applications that we can match to jobs, our standard errors are nearly identical regardless of if we cluster on the resume, job, or multi-way cluster on both. We present these results in Online Appendix Table D2.

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

	Cluster on Resume (1)	Cluster on Job (2)	Multi-way Cluster, Resume and Job (3)
Native American	-0.002	-0.002	-0.002
	(0.006)	(0.008)	(0.006)
… x Reservation	-0.003	-0.003	-0.003
	(0.009)	(0.009)	(0.009)
… x Reservation x Reservation Job	-0.005	-0.005	-0.005
	(0.012)	(0.012)	(0.016)
Alaska Native	-0.004	-0.004	-0.004
	(0.014)	(0.024)	(0.013)
Native Hawaiian	-0.007	-0.007	-0.007
	(0.007)	(0.012)	(0.007)
Rural	-0.019	-0.019	-0.019
	(0.010)	(0.010)	(0.011)
… x Rural Job	0.004	0.004	0.004
	(0.014)	(0.014)	(0.012)
Callback Rate for White:		19.8%	

[13] This occurs because we do not have a unique phone number for each applicant. Since we assign multiple applicants the same number, we are sometimes not able to match a voicemail response to a specific job even if we can match it to a specific resume. This can occur because the voicemail is sparse on important details like applicant name or company. In all, there were only 33 responses that we were unable to match to a job. More details on how this is addressed generally can be found in Online Appendix A.

Notes: Regressions include the "Regular Controls" and occupation and city fixed effects from Table 6 (Column (2)). See also the notes to Table 6. N=11,759 since we dropped 1,757 applications that could not be matched to a specific job.

Estimates by Occupation and Indigenous Subgroup

In Online Appendix Table D3, we re-estimate our results from Table 7, which presents results by occupation separately interacted with NA, NH, and AN indicator variables.

Online Appendix Table D3 – Discrimination Estimates by Occupation

Interactions	Estimate	Callback Rate for Whites	N
NA x Retail	0.00264 (0.0143)	17.3%	1,015
NA x Server	0.00841 (0.0143)	16.4%	1,115
NA x Kitchen	-0.00455 (0.0126)	22.2%	1,882
NA x Janitor	-0.00943 (0.0180)	16.8%	633
NA x Security	-0.000542 (0.0228)	27.4%	560
NH x Retail	0.0169 (0.0233)	17.3%	385
NH x Server	-0.0316* (0.0174)	16.4%	221
NH x Kitchen	-0.0235 (0.0211)	22.2%	405
NH x Janitor	0.0167 (0.0231)	16.8%	146
NH x Security	0.0531 (0.0550)	27.4%	80
AN x Retail	-0.0599 (0.0539)	17.3%	63
AN x Server	-0.0861* (0.0455)	16.4%	51
AN x Kitchen	0.0393 (0.0505)	22.2%	108
AN x Janitor	0.0509 (0.0635)	16.8%	47
AN x Security	0.206* (0.118)	27.4%	13

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

Estimates by Occupation and Gender

In Online Appendix Table D4, we re-estimate our results from Table 7, which presents results by occupation, however instead present the results by the interaction of occupation and gender. The estimates show no differential treatment of Indigenous men compared to white men. We find a strong preference for female applicants for server positions, a 6.5 percentage point higher callback rate for white women compared to white men (who have a callback rate of 13.3%). Similarly, and as found in previous work (e.g., Neumark, Burn, and Button, 2019; Neumark et al. 2019), we find a preference for women in retail sales: a 3.7 percentage point higher callback rate for white women compared to white men (who have a callback rate of 16.3%).

Online Appendix Table D4 – Discrimination Estimates by Occupation and Gender

Variable	Estimate
Indigenous	
… x Retail	0.006
	(0.017)
… x Server	-0.002
	(0.016)
… x Kitchen	-0.007
	(0.014)
… x Janitor	0.003
	(0.021)
… x Security	0.011
	(0.022)
Female	
… x Retail	0.037**
	(0.018)
… x Server	0.065***
	(0.017)
… x Kitchen	0.000
	(0.015)
… x Janitor	-0.012
	(0.022)
Indigenous x Female	
… x Retail	-0.003
	(0.025)
… x Server	0.002
	(0.024)
… x Kitchen	0.001
	(0.021)
… x Janitor	-0.008
	(0.031)

Notes: N=13,516. See also the notes to Table 6. For reference, the callback rate for white men is 16.3% in retail, 13.3% in server, 21.5% in kitchen, 17.7% in janitor, and 27.4% in security. Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). Regressions include the "Regular Controls" and occupation and city fixed effects from Table 6 (Column (2)). Note that we did not send female applicants to security jobs.

Effects by City

Online Appendix Table D5 shows results by city. Again, there are mostly no differential results. Callback differences are within two percentage points for all cities except Phoenix (Albuquerque) where Indigenous applicants have a 4.1 percentage point higher (3.7 percentage point lower) callback rate. Only the estimate for Phoenix is statistically significant, but only at the 10% level. We also ran an additional regression, but with additional three-way interactions between NA, Reservation, and city, to see if the effects of reservation upbringing also varied by city. Online Appendix Table D6 presents these results and does not show any differences by city.

Online Appendix Table D5 – Discrimination Estimates by City

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

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

Online Appendix Table D6 – Discrimination Estimates by City with Reservation Signal Interactions

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

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.

Additional Robustness Checks Regarding the Navajo Last Name Signal

Online Appendix Table D7 replicates Table 6, with the main results of Table 6 (column (2)) presented in column (1) of this appendix table. Column (2) of this appendix table conducts a robustness check where resumes with the Navajo last name signal only are dropped from the analysis. This is to deal with the critique that this signal may not have been strong. Dropping this signal does not affect the results. Column (3) instead adds a separate control variable for having a Navajo last name, and this again does not change the results.

Online Appendix Table D8 does a similar robustness check, this time replicating column (1) of Table 8, which is presented as column (1) in this appendix table. In this appendix table, the analysis in column (2) pretends that the Navajo last name signal was not there, so this drops the indicator variable for Navajo last name signal only and ignoring the signal if it appears with other signals (so, for example, a resume with a Navajo last name and the volunteer signal would be counted as volunteer only). This again does not affect the results.

Online Appendix Table D7 – Replicating Table 6, Column (2), Ignoring Navajo Last Name Signals

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

Notes: N=13,516. Column (1) is Column (2) from Table 6. For column (2), any Indigenous resume with the only signal being a Navajo last name signal was recoded as being a non-Indigenous resume. For column (3), Navajo last name signals were added as a separate control variable to the regression in Column (1). Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level.

Online Appendix Table D8 – Replicating Table 8, Ignoring Navajo Last Name Signals

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

Notes: N=13,516. See the notes to Tables 6 and 10. Regressions include the "Regular Controls" and occupation and city fixed effects from Table 6 (Column (2)). Column (1) presents the results from Table 8 for comparison. Column (2) repeats this analysis, pretending that there is no Navajo last name signal. This recodes some the resumes with the last name signal and one other signal as just having that one other signal, and re-codes resumes with the last name signal, volunteer signal, and language signal as "Two Signals." Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*).

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

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

Do Callbacks Capture Hiring Discrimination? Callbacks vs. Explicit Interview Offers

We coded two forms of employer responses: (1) callbacks, and (2) explicit interview offers only. The former is used as the default in many other resume correspondence studies. Callbacks include explicit interview offers but also more ambiguous positive responses (e.g., "I have reviewed your application and have some additional questions for you."). Online Appendix Table D9 compares how our main results from Table 6 change when we use explicit interview offers instead of callbacks. Our results do not vary.

Online Appendix Table D9 – Estimates from Tables 6, D4, D5, and 8, Comparing Results Using Interview Rates Instead of Callback Rates

	Callback (1)	Interview (2)
Panel (a) (Corresponding to Column (2) of Table 6)		
Native American	-0.004 (0.009)	-0.002 (0.008)
… x Reservation	-0.000 (0.012)	0.007 (0.010)
… x Reservation x Reservation Job	0.006 (0.016)	0.001 (0.014)
Alaska Native	0.005 (0.035)	0.010 (0.030)
Native Hawaiian	-0.003 (0.013)	-0.001 (0.011)
Panel (b) (Corresponding to Table D4)		
Indigenous		
… x Retail	0.006 (0.017)	0.013 (0.015)
… x Server	-0.002 (0.016)	0.008 (0.015)
… x Kitchen	-0.007 (0.014)	0.007 (0.013)
… x Janitor	0.003 (0.021)	0.009 (0.018)
… x Security	0.011 (0.022)	-0.005 (0.018)
… x Female x Retail	-0.003 (0.025)	-0.018 (0.022)
… x Female x Server	0.002 (0.024)	-0.007 (0.022)
… x Female x Kitchen	0.001 (0.021)	-0.011 (0.018)
… x Female x Janitor	-0.008 (0.031)	-0.023 (0.024)
Panel (c) (Corresponding to Table D5)		
Indigenous		
… x Phoenix	0.041 (0.023)	0.032 (0.019)
… x Chicago	-0.009 (0.018)	-0.013 (0.014)
… x Los Angeles (NA)	-0.001 (0.014)	0.006 (0.011)
… x Los Angeles (NH)	-0.014 (0.019)	-0.016 (0.015)
… x Alaska (AN)	0.005 (0.035)	0.010 (0.030)
… x Honolulu (NH)	0.002 (0.019)	0.005 (0.015)
… x Billings	0.012 (0.062)	-0.024 (0.054)
… x Albuquerque	-0.037 (0.029)	-0.036 (0.027)
… x New York City	-0.011 (0.011)	-0.002 (0.010)
… x Oklahoma City	0.018 (0.033)	0.001 (0.028)
… x Sioux Falls	-0.004 (0.078)	0.023 (0.073)
… x Houston	-0.002 (0.024)	0.005 (0.020)
Panel (d) (Corresponding to Column (1) of Table 8)		
Indigenous		
… x Volunteer	-0.006 (0.010)	0.000 (0.008)
… x Language	0.006 (0.010)	0.009 (0.009)
… x First Name (Native Hawaiian)	-0.017 (0.018)	-0.023 (0.015)
… x Last Name (Navajo)	-0.007 (0.026)	-0.011 (0.025)
Two Signals	0.003 (0.015)	0.004 (0.013)
Three Signals	0.038 (0.037)	0.033 (0.034)

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

Population and Occupation Weighting

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

Online Appendix Table D10 describes how we created population weights. We first used population counts for AIANs and NHPIs from Norris, Vines, and Hoeffel (2012) and Hixson, Hepler, and Kim (2012), respectively. We used two different population estimates: AIAN (NHPI) alone or AIAN (NHPI) alone or in combination ("in comb"). We constructed population weights by dividing the number of jobs applied to, by city, and by the AIAN or NHPI population in each city, and then normalizing such that a value of one meant no relative weight (neither up nor down) is applied to that city.[14] Weights greater than (less than) one meant that our number of observations for that city was lower (higher) relative to the Indigenous population compared to other cities, and thus the observations for that city needed to be up-weighted (down-weighted).

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

This table indicates that, as expected, we over-sampled Chicago and Houston, large cities with a small proportion of Indigenous Peoples, and under-sampled Honolulu, Anchorage, and other cities with a higher proportion of Indigenous Peoples.

Online Appendix Table D11 presents our construction of occupation weights. To construct these weights, we used all months of the 2015 Current Population Survey (CPS) to estimate the proportion of those aged 25 to 35 who were employed in each occupation. To match the narrower occupational coding in the CPS to our broader occupations (retail, kitchen, server, janitor, and security), we add up occupation counts for each CPS occupation that matched our broader occupations.[15] Online Appendix Tables A3 and A4 present most of the occupation frequencies for these narrower occupations. These occupation weights suggest that relative to the nationally-representative employment estimates in the CPS, we oversampled server and security and under-sampled retail.

Online Appendix Table D12 presents our occupation-by-population weights. We calculated these by multiplying the occupation and population weights together. These weights have a high range, from 0.11 (Chicago, servers, using "in combination") to 5.20 (Honolulu, retail, "in combination").

Finally, in Online Appendix Table D13, we present our main results (replicating Table 6, column (2)) under different types of weighting (Indigenous population in the city, occupational

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

popularity, and both). Our results are unchanged regardless of how we weight (or do not weight) the results.[16]

Online Appendix Table D10 – Construction of Population Regression Weights

Panel (a): Cities with Native American and Alaska Native Applicants								
	Total	AIAN alone or in combination		AIAN alone		Jobs Applied	Population Weight	
City	Population	%	Count	%	Count		In Comb.	Alone
New York	8,175,133	1.4%	111,749	0.7%	57,512	2,756	0.85	0.85
Los Angeles	3,792,621	1.4%	54,236	0.7%	28,215	1,866	0.61	0.62
Phoenix	1,445,632	3.0%	43,724	2.2%	32,366	1,530	0.60	0.86
Oklahoma City	579,999	6.3%	36,572	3.5%	20,533	614	1.25	1.36
Anchorage	291,826	12.4%	36,062	7.9%	23,130	564	1.34	1.67
Albuquerque	545,852	6.0%	32,571	4.6%	25,087	700	0.97	1.46
Chicago	2,695,598	1.0%	26,933	0.5%	13,337	1,466	0.38	0.37
Houston	2,099,451	1.2%	25,521	0.7%	14,997	1,106	0.48	0.55
Sioux Falls	153,888	3.6%	5,540	2.7%	4,155	154	0.75	1.10
Billings	104,170	6.0%	6,251	4.4%	4,584	212	0.62	0.88
National	308,745,538	1.7%	5,220,579	0.9%	2,932,248	10,968		
Panel (b): Cities with Native Hawaiian Applicants								
	Total	NHPI alone or in combination		NHPI alone			Population Weight	
City	Population	%	Count	%	Count	Jobs Applied	In Comb.	Alone
Honolulu	953,207	24.5%	233,637	9.5%	90,878	2,020	2.42	1.84
Los Angeles	3,792,621	0.6%	20,924	0.3%	10,079	508	0.86	0.81
National	308,745,538	0.4%	1,225,195	0.2%	540,013	2,290		

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

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

Online Appendix Table D11 – Construction of Occupation Regression Weights

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

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

Online Appendix Table D12 – Construction of Occupation-by-Population Regression Weights

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

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

Online Appendix Table D13 – Robustness of the Estimates in Table 6, Column (2), to Different Weights

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

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

Robustness to the Proportion Hispanic in each Occupation and City.

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

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

Hispanic), AIAN (alone or in combination, independent of Hispanic ancestry), or Hispanic (independent of race).

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

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

Online Appendix Table D14 – Demographics of Each Occupational Grouping, by City and Gender

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

Notes: This analysis is calculated using Current Population Survey data from IPUMS-CPS (Flood et al., 2018). Bolded numbers indicate when the % Hispanic > % white. White only includes those who only report white as a race and do not report being Hispanic. Hispanic includes those who report being Hispanic, regardless of race. AIAN includes those who report being AIAN alone or in part, regardless of if they report being Hispanic or report another race as well. The occupational groupings correspond to the following occupational codes: retail sales (retail salespersons; cashiers; counter and rental clerks; sales representatives, services, all other; and sales and related workers, all others, in the Census occupational classification), kitchen staff (cooks; food preparation workers; dishwashers; combined food preparation and serving workers, including fast food; counter attendants, cafeteria, food concession, and coffee shops; food servers, non-restaurant; and dining room and cafeteria attendants and bartender helpers), server (waiters and waitresses; bartenders; and hosts and hostesses, restaurant, lounge, and coffee shop), janitors (janitors and building cleaners and grounds maintenance workers), and security guards (security guards and gaming surveillance officers).

Timing and Labour Market Tightness of Our Experiment Relative to Other Audit Field Experiments

Online Appendix Table D15 presents a table showing how the timing and labour market tightness of our experiment compared to that of other experiments. The table presents when each study from Neumark (2018) and Baert (2018) were in the field, according to the paper (as best we could determine), and what the unemployment rate range was during this time. We then calculate a percentile range for this unemployment rate from the study, which informs where the study fits in the distribution of unemployment rates, from January 1948 to October 2018.

This table shows that our study was during a time with lower unemployment rates (16th to 24th percentile of the seasonally-adjusted rate from 1948 to 2018). This percentile range of our unemployment rates overlaps with the ranges of Pager (2003) (23rd to 56th percentile) and Kleykamp (2009) (21st to 35th), both which find statistically significant effects, although their signals of minority status may have been stronger (e.g., criminal records). The unemployment rates during our study were not as extreme as over a third of the other studies which occurred during the Great Recession, where unemployment rates reached record highs.[17]

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

[17] Of the 21 studies, eight have a percentile range that includes at least the 90th percentile of unemployment rates, if not higher (Jacquemet and Yannelis 2012; Bailey, Wallace, and Wright 2013; Wright et al. 2013; Decker et al. 2015; Nunley et al. 2015; Gaddis 2015; Hipes et al. 2016; Farber, Silverman, and von Wachter 2017). We argue that many of these studies are just case studies of the Great Recession and may not tell us about discrimination in general.

Online Appendix Table D15 – Comparison of the Timing of Our Study with Others in the US

Study	Timing	Unemployment Rates During Timing	Percentile Range
This Paper	March to December 2017	4.1-4.5	16th-24th
Agan and Starr (2018)	Jan, Feb, May, June 2015	5.3-5.7	42nd-55th
Ameri et al. (2018)	June to August 2013	7.2-7.5	80th-85th
Bailey, Wallace, and Wright (2013)	March to May 2010	9.6-9.9	**97th-99th**
Bendick, Jackson, and Romero (1997)	March to June 1993	7.0-7.1	77th-79th
Bendick et al. (1999)	March 1995 to March 1996	5.4-5.8	44th-59th
Bertrand and Mullainathan (2004)	July 2001 to May 2002	4.6-5.8	25th-59th
Darolia et al. (2016)	May 2013 to May 2014	6.3-7.5	69th-86th
Decker et al. (2015)	June to August 2011, June to August 2012*	8.1-9.1	**90th-96th**
Farber, Silverman, and von Wachter (2017)	March to May 2012, July to September 2012	7.8-8.2	**89th-91st**
Gaddis (2015)	March to August 2011	9.0-9.1	**95th-96th**
Hipes et al. (2016)	June 2011 to May 2012	8.2-9.1	**91st-96th**
Jacquemet and Yannelis (2012)	August 2009 to February 2010	9.6-10.0	**97th-99th**
Kleykamp (2009)	Year of 2007*	4.4-5.0	21st-35th
Lahey (2008)	February 2002 to February 2003	5.7-6.0	55th-65th
Mishel (2016)	March, April, May 2014*	6.3-6.7	69th-74th
Neumark et al. (2019)	January to June 2015	5.3-5.7	41st-56th
Nunley et al. (2015)	January to July 2013	7.3-8.0	**82nd-91st**
Pager (2003)	June to December 2001	4.5-5.7	23rd-56th
Tilcsik (2011)	Year of 2005*	4.9-5.4	30th-45th
Widner and Chicoine (2011)	February and March 2008*	4.9-5.1	30th-37th
Wright et al. (2013)	July to October 2009	9.5-10.0	**96th-99th**

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

Online Appendix Table D16 – Unemployment Rates by Occupation-City Combinations

State * Occupation	Unemployment Rate	White Callback	State * Occupation	Unemployment Rate	White Callback
AK * Janitor	11.9%	61.7%	MT * Security	5.0%	66.7%
AK * Kitchen	10.4%	75.0%	MT * Server	8.4%	72.7%
AK * Retail	9.6%	85.7%	NM * Janitor	8.2%	61.8%
AK * Security	5.5%	61.5%	NM * Kitchen	8.7%	64.4%
AK * Server	9.2%	64.7%	NM * Retail	8.9%	68.4%
AZ * Janitor	6.7%	83.2%	NM * Security	7.2%	65.0%
AZ * Kitchen	10.6%	71.6%	NM * Server	6.8%	47.4%
AZ * Retail	9.0%	84.4%	NY * Janitor	8.8%	69.4%
AZ * Security	7.8%	78.6%	NY * Kitchen	8.1%	71.1%
AZ * Server	6.9%	78.2%	NY * Retail	9.0%	61.3%
CA * Janitor	7.4%	90.7%	NY * Security	9.7%	72.0%
CA * Kitchen	7.1%	72.2%	NY * Server	7.3%	82.1%
CA * Retail	12.3%	52.5%	OK * Janitor	9.2%	84.2%
CA * Security	12.2%	98.6%	OK * Kitchen	9.2%	54.3%
CA * Server	9.8%	60.4%	OK * Retail	6.6%	68.8%
HI * Janitor	6.7%	59.5%	OK * Security	8.3%	88.2%
HI * Kitchen	7.4%	64.8%	OK * Server	7.1%	81.4%
HI * Retail	5.7%	94.2%	SD * Janitor	7.9%	81.8%
HI * Security	8.3%	57.4%	SD * Kitchen	5.9%	74.1%
HI * Server	6.0%	69.7%	SD * Retail	4.6%	84.2%
IL * Janitor	10.9%	69.7%	SD * Security	2.5%	50.0%
IL * Kitchen	8.2%	79.8%	SD * Server	4.5%	72.2%
IL * Retail	7.7%	85.2%	TX * Janitor	6.3%	68.5%
IL * Security	11.1%	65.7%	TX * Kitchen	7.2%	83.2%
IL * Server	8.8%	82.1%	TX * Retail	7.9%	72.5%
MT * Janitor	11.4%	77.8%	TX * Security	7.2%	88.0%
MT * Kitchen	7.3%	75.0%	TX * Server	9.8%	61.9%
MT * Retail	5.5%	37.5%			

Notes: Data from IPUMS-CPS monthly data from 2010-2017 (Flood et al., 2018). Means are weighted using the provided population survey weights.

Cross-Sectional Analysis of Labour Market Tightness

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.[18] Second, we use the callback rate by occupation and city for our white applicants as a measure of tightness. We estimate the following regression:

$$\begin{aligned} Callback_i = \ & \beta_0 + \beta_1 Indigenous_i + \beta_2 Tightness_{co} + \beta_3 Indigenous_i * Tightness_{co} \\ & + Controls_i \gamma + \varepsilon_i \end{aligned} \quad [3]$$

where i indexes the individual applicant, c indexes the city, and o indexes the occupation. $Tightness_{co}$ is one of the two tightness measures above, calculated for each city and occupation combination. We demean $Tightness_{co}$ so that β_1 is interpreted as discrimination against Indigenous applicants at the mean of $Tightness_{co}$. We express both our tightness measures, the unemployment rate and the callback rate for whites, as percentages so that a one-unit increase in $Tightness_{co}$ represents an increase in the unemployment rate or the callback rate of white by one percentage point.

This regression, compared to Equation [1], does not include city and occupation fixed effects. Excluding these allows us to leverage between city and between occupation variation in labour market tightness, instead of just within-city variation across occupations.[19] Since

[18] 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.
[19] Results are, however, similar if we do include these fixed effects, as shown in Online Appendix Tables D17 and D18.

$Tightness_{co}$ is determined at the occupation and city level, we two-way cluster our standard errors on occupation by city and also on resume, which was our default in the earlier analysis.

β_2 indicates how the callback rate for whites varies by tightness and our coefficient of interest. For the measure of tightness using the unemployment rate, we expect that as the unemployment rate increases (tightness decreases), callback rates will decrease (so $\beta_2 < 0$). For the measure of tightness using the callback rate for white, we instead expect $\beta_2 > 0$. β_3 Indicates if the discrimination estimate varies by tightness. If discrimination decreases when the unemployment rate (callback rate for white) increases, then $\beta_3 > 0$ ($\beta_3 < 0$).

Online Appendix Table D17 – Callback Estimates by Labour Market Tightness

	Labour Market Tightness Measure:	
	Unemployment Rate (1)	Callback Rate for White (2)
Indigenous	-0.000452	-0.000510
	(0.00476)	(0.00450)
Tightness	-0.000649	0.00224
	(0.000666)	(0.00453)
Indigenous x Tightness	0.0202	0.00217
	(0.0307)	(0.00249)
Number of occupation-city clusters:	55	55
Number of resume clusters:	3,072	3,072
N	13,244	13,244

Notes: We calculate unemployment rates by occupation for Anchorage using data from the entire state of Alaska. Regressions use the "Regular Controls" from Table 6 (Column (2)) but do not include city and occupation fixed effects. Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level. We two-way cluster our standard errors on occupation-by-city and by resume.

Online Appendix Table D18 – Callback Estimates by Labour Market Tightness Without State or Occupation Fixed Effects

	Labour Market Tightness Measure:	
	Unemployment Rate (1)	Callback Rate for White (2)
Indigenous	0.000137	-0.0000598
	(0.00484)	(0.00467)
Tightness	-0.000219	-0.00744
	(0.00118)	(0.00785)
Indigenous x Tightness	0.0203	0.00188
	(0.0301)	(0.00252)
Number of occupation-city clusters:	55	55
Number of resume clusters:	3,072	3,072
N	13,244	13,244

Notes: See also the notes to Table 6 and D17. Regressions use the "Regular Controls" from Table 6 (Column (2)) but do not include city and occupation fixed effects. Different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). We two-way cluster our standard errors on occupation-by-city and by resume.

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

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

More Details on the Resume Survey

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

The specific resumes we tested had the following signals of Indigenous status (or no signal):[20]

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

[20] The actual resumes are available upon request.

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

Resume Survey Questions

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

More Detailed Resume Survey Results

Online Appendix Table E1 – Responses to "What is the race or ethnicity of this applicant?" from the Resume Survey, Arizona and New Mexico Only

Resume Type	Distribution of Responses (by Resume Type)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Navajo Last Name	x			x	x		x
Language (Navajo)		x		x		x	x
Volunteer (Native American)			x		x	x	x
Response							
White	23.6%	0%	17.1%	17.5%	21.1%	16.9%	18.0%
American Indian or Alaska Native	**58.3%**	**71.1%**	**73.2%**	**76.7%**	**70.7%**	**78.3%**	**68.5%**
Native Hawaiian or Pacific Islander	5.5%	3.6%	4.9%	2.9%	3.3%	2.4%	6.3%
Hispanic	4.7%	2.4%	3.7%	1.9%	1.6%	2.4%	5.4%
Black	0.8%	0%	1.2%	0%	0%	0%	0%
Other	7.1%	22.9%	0%	1.0%	3.3%	0%	1.8%
N	127	83	82	103	123	83	111

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

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

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

Names Survey Questions

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

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

More Detailed Name Survey Results

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

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

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

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

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

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

Notes: Sample sizes in parenthesis.

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

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

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

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

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

Notes: See the notes to Online Appendix Table F1.

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

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

Notes: See the notes to Online Appendix Table F1.

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

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

Notes: See the notes to Online Appendix Table F1.

Online Appendix G: Decompositions of Disparities in Economics Outcomes using CPS Data

Data Sources and Sample Composition

We used data from the Current Population Survey (CPS) (Flood et al., 2018) to measure the unconditional and conditional gaps in economic outcomes between AIAN, NHPI, and white populations. We study disparities in log hourly wages, unemployment rates, unemployment duration, and employment duration in weeks. We pooled data for the years 2013 to 2017, and we restricted the sample to individuals of age 25 to 64 of any gender. We also estimated results using some restricted samples that more closely match our experiment. These results were similar and are available upon request. Data for employment duration comes from the "Job Tenure" supplements in 2014, 2016, and 2018.

Coding Race

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

Measuring Economic Outcomes

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

Oaxaca-Blinder Decomposition

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

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

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

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

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

The term $\beta_0(X_0 - X_1)'$ is the explained part of the wage differential while the term $(\beta_0 - \beta_1)X'_0$ is the unexplained part of the wage differential. The variables in X_0 and X_1 include:

location (indicator variables for each state), marital status (indicator variables for each type of status including married with or without spouse present, separated, divorced, never married, widowed), occupation (indicator variables for each category, harmonized to 2010 variables), education (indicator variables for each highest grade attained, assuming the lower value if an education range was provided), whether the individual is Hispanic, age and age squared terms, indicators for the number of children, whether the individual is female, experience (calculated as age minus five minus years of education), indicators for month and year combinations, and whether the individual lives in metro or non-metro location.

Gelbach (2016) Decomposition

We conduct a Gelbach decomposition (Gelbach 2016), 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 "b1x2" (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 unemployment 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 are more directly related to the callback discrimination we estimate in our resume correspondence experiment.[21] We also extend prior studies by also studying employment durations, which allows us to investigate if there could be discrimination in firings, something which cannot be studied in an audit field experiment.

Results

We present the more detailed results in Online Appendix Tables G5 through G6, with a summary of these results in the main paper (Tables 10 and 11).

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

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

Outcome Variable	AIAN Alone	AIAN Alone or In Part	NHPI Alone	NHPI Alone or in Part	Non-Hispanic White Alone
Less Than High School Graduate	18.9%	17.1%	13.0%	12.0%	9.3%
High School Graduate or GED	25.2%	23.5%	27.7%	26.3%	23.9%
Some College but no Degree	14.3%	12.1%	14.3%	13.9%	15.2%
Associate's Degree	6.9%	7.1%	6.5%	6.3%	8.4%
Bachelor's Degree	6.1%	7.2%	9.9%	9.2%	17.2%
Master's Degree	2.1%	2.6%	2.7%	2.5%	7.1%
Professional School Degree	0.3%	0.3%	0.4%	0.5%	1.2%
Doctorate Degree	0.4%	0.5%	0.4%	0.4%	1.4%
N	198,260	335,284	72,754	115,190	10,528,129

Notes: Calculated using IPUMS-CPS data from 2010 to 2019 (Flood et al., 2018). Categories were calculated using the "educ" variable, which encodes multiple levels of highest educational attainment.

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

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

Notes: These estimates use data from the outgoing rotation group (ORG) of the IPUMS-CPS monthly data from 2013-2017 (Flood et al., 2018). Statistically significantly different from at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). The mean hourly wage for non-Hispanic whites (the comparison group) is $19.13. The hourly wage was generated using the reported hourly wage for those who are paid hourly and are below the censored limit or the calculated hourly wage from weekly earnings divided by the usual working hours. Controls include indicator variables for state, marital status, occupation, education, number of children, sex, metro status, years of experience, month by year, whether the individual is Hispanic, and age and age squared terms, indicators for month and year combinations.

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

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

Notes: These estimates use data from the IPUMS-CPS monthly data from 2013-2017 (Flood et al., 2018). Statistically significantly different from at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). The unemployment rate for non-Hispanic whites (the comparison group) is 0.037. Controls include indicator variables for state, marital status, occupation, education, number of children, sex, metro status, years of experience, month by year, whether the individual is Hispanic, and age and age squared terms, indicators for month and year combinations. The Unemployment outcome is an indicator variable and the Oaxaca model used is a linear probability model.

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

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

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

Online Appendix Table G5 – Oaxaca-Blinder and Gelbach Decomposition Estimate Comparisons Estimates (AIANs vs Whites)

	Log Hourly Wage		Unemployment Rates		Unemployment Duration (Weeks)		Employment Duration (Weeks)	
	Oaxaca	Gelbach	Oaxaca	Gelbach	Oaxaca	Gelbach	Oaxaca	Gelbach
Total Difference	-0.145***	-0.145	0.045***	0.045***	-1.705***	-1.705**	-124.8***	-124.8***
	(0.006)	(0.008)	(0.001)	(0.002)	(0.502)	(0.800)	(12.1)	(12.4)
Explained	-0.133***	-0.113***	0.003***	0.003*	-3.313***	-3.313***	-107.4***	-107.4***
	(0.006)	(0.006)	(0.000)	(0.002)	(0.263)	(0.636)	(11.8)	(11.6)
Occupation	-0.072***	-0.072***	0.013***	0.013***	0.495***	0.495**	-28.4***	-28.4***
	(0.005)	(0.005)	(0.000)	(0.000)	(0.156)	(0.219)	(3.1)	(3.1)
Education	-0.053***	-0.051***	0.007***	0.004***	1.330***	0.974***	29.7***	29.7***
	(0.002)	(0.002)	(0.000)	(0.001)	(0.081)	(0.186)	(2.7)	(2.7)
State	0.017***	0.017	0.001***	0.001***	-1.086***	-1.086***	-14.5***	-14.5***
	(0.001)	(0.002)	(0.000)	(0.000)	(0.081)	(0.208)	(1.8)	(1.8)
Hispanic	-0.014***	-0.014**	-0.019***	-0.019***	-2.466***	-2.466***	-2.2	-2.2
	(0.001)	(0.006)	(0.000)	(0.001)	(0.120)	(0.518)	(8.5)	(8.5)
Age	-0.010***	-0.010***	-0.000***	-0.000***	-2.744***	-2.744***	-94.7***	-94.7***
	(0.001)	(0.003)	(0.000)	(0.000)	(0.173)	(0.405)	(9.5)	(9.5)
Married	-0.006***	-0.006***	0.003***	0.003***	0.503***	0.503***	-5.0***	-5.0***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.080)	(0.089)	(1.0)	(1.0)
Gender	0.005***	0.005***	-0.000**	-0.000**	0.088**	0.088**	0.5	0.5
	(0.001)	(0.001)	(0.000)	(0.000)	(0.041)	(0.041)	(0.4)	(0.4)
Metro Status	-0.003***	-0.003***	0.000***	0.000**	-0.074***	-0.074	-0.9	-0.9
	(0.000)	(0.001)	(0.000)	(0.000)	(0.025)	(0.083)	(0.8)	(0.8)
Experience	0.003**	0.001	-0.001***	0.002	1.226***	1.582***	9.2***	9.2***
	(0.001)	(0.002)	(0.000)	(0.001)	(0.114)	(0.322)	(3.0)	(3.0)
Survey Timing	0.001**	0.001*	-0.000***	-0.000***	-0.304***	-0.304***	-0.6***	-0.6***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.100)	(0.103)	(0.2)	(0.2)
Children	-0.000**	-0.000	0.000***	0.000***	-0.282***	-0.282***	-0.3	-0.3
	(0.000)	(0.000)	(0.000)	(0.000)	(0.035)	(0.074)	(0.4)	(0.4)
Unexplained	-0.012***	-0.015**	0.043***	0.043***	1.609***	1.609*	-17.4	-17.4
	(0.003)	(0.006)	(0.000)	(0.002)	(0.410)	(0.917)	(13.1)	(13.0)
White Mean	$19.13		0.037		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 Tenure supplement data). Statistically significantly different from zero at 1-per cent level (***), 5-per cent level (**) or 10-per cent level (*). Robust standard errors are in parentheses. AIANs include only those who identify as AIAN alone. Results including AIAN in combination are similar and are presented in Online Appendix Tables G2, G3, and G4. Hourly wage is determined as either the hourly wage for those paid hourly, or if not paid hourly, it is estimated by dividing weekly earnings by the usual hours worked. Estimates are weighted using person-level sampling weights.

Online Appendix Table G6 – Oaxaca-Blinder and Gelbach Decomposition Estimate Comparisons (NHPIs vs Whites)

	Log Hourly Wage		Unemployment Rates		Unemployment Duration (Weeks)		Employment Duration (Weeks)	
	Oaxaca	Gelbach	Oaxaca	Gelbach	Oaxaca	Gelbach	Oaxaca	Gelbach
Total Difference	-0.087***	-0.087***	0.017***	0.017***	-2.876**	-2.876*	-153.6***	-153.6***
	(0.012)	(0.013)	(0.001)	(0.002)	(1.383)	(1.564)	(16.6)	(17.4)
Explained	-0.046***	-0.046***	0.010***	0.010***	0.010	0.010	-138.1***	-138.1***
	(0.011)	(0.011)	(0.001)	(0.002)	(0.646)	(1.120)	(15.6)	(15.3)
Occupation	-0.053***	-0.053***	0.009***	0.009***	0.068	0.068	-36.9***	-36.9***
	(0.007)	(0.007)	(0.001)	(0.001)	(0.308)	(0.364)	(4.9)	(4.9)
Education	-0.010*	-0.025***	-0.005***	0.002*	0.731*	0.984***	20.1***	20.1***
	(0.006)	(0.003)	(0.000)	(0.001)	(0.396)	(0.339)	(3.0)	(3.0)
State	-0.026***	0.049***	0.004***	0.002***	0.858***	0.694**	-9.5***	-9.5***
	(0.003)	(0.003)	(0.000)	(0.000)	(0.165)	(0.335)	(3.0)	(3.1)
Hispanic	-0.002***	-0.010*	0.002***	-0.005***	-0.434***	0.731	-0.9	-0.9
	(0.001)	(0.006)	(0.000)	(0.001)	(0.151)	(0.960)	(8.3)	(8.4)
Age	0.049***	-0.018***	0.002***	0.000*	0.694***	-3.461***	-129.3***	-129.3***
	(0.003)	(0.004)	(0.000)	(0.000)	(0.138)	(0.581)	(15.0)	(15.0)
Married	0.000	-0.002***	-0.001***	0.002***	1.647***	-0.434***	1.9*	1.9*
	(0.003)	(0.001)	(0.000)	(0.000)	(0.228)	(0.165)	(1.1)	(1.1)
Gender	0.008***	0.005***	-0.000***	-0.000	0.260***	-0.209**	0.4	0.4
	(0.001)	(0.002)	(0.000)	(0.000)	(0.035)	(0.085)	(0.7)	(0.7)
Metro Status	-0.018***	0.008***	0.000***	-0.000	-3.461***	0.260***	-3.0***	-3.0***
	(0.004)	(0.001)	(0.000)	(0.000)	(0.344)	(0.088)	(0.7)	(0.7)
Experience	-0.000	-0.000	0.000***	0.001	-0.295***	1.522***	19.5***	19.5***
	(0.000)	(0.003)	(0.000)	(0.001)	(0.058)	(0.473)	(5.4)	(5.8)
Survey Timing	0.003***	0.003***	-0.000	-0.000	0.151	0.151	0.3	0.3
	(0.001)	(0.001)	(0.000)	(0.000)	(0.215)	(0.224)	(0.3)	(0.3)
Children	0.005***	-0.000	-0.000	0.000**	-0.209**	-0.295**	-0.8	-0.8
	(0.002)	(0.000)	(0.000)	(0.000)	(0.082)	(0.115)	(0.6)	(0.6)
Unexplained	-0.041***	-0.041***	0.007***	0.007**	-2.887**	-2.887*	-15.5	-15.5
	(0.012)	(0.012)	(0.001)	(0.003)	(1.219)	(1.550)	(16.9)	(17.6)
White Mean	$19.13		0.037		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-percent level (***), 5-percent level (**) or 10-percent level (*). NHPIs include those who identify as NHPI alone. Results including NHPI in combination are similar and are presented in Online Appendix Tables G2, G3, and G4.

Online Appendix H: Sample Resumes and Cover Letters

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

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

Objective	To obtain a position as a sales associate.
Work Experience	**Sales Associate** Costco, Phoenix, AZ *Oct. 2009 - Present* Assist customers as they shop, answering questions and trying to find the merchandise that fits their needs the best. Straighten up merchandise to ensure a professional appearance. Ring up customers at check out.
	Cashier Walmart, Phoenix, AZ *July 2008 - Sept. 2009* Worked as a cashier and in customer service Primary responsibilities were related to working the cash register, but also assisted with stocking shelves. Occasionally, I checked merchandise for damage and incorrect tags.
	Sales Associate Target, Phoenix, AZ *Nov. 2004 - June 2008* Answer customers’ questions. Ring up customers at checkout. Handle returns and other customer service responsibilities. Straighten up merchandise to insure a professional appearance at all times.
Volunteering	**Volunteer** Warner A. Gabel Boys & Girls Club, Phoenix, AZ *Mar. 2014 - Present* I assisted kids with homework, played sports with them, and assisted staff in caring for the kids.
Education	**High School Diploma** Chandler High School, 2004 Chandler, AZ
References	References available upon request.

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

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

Dear Hiring Manager,

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

Please see my attached resume

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

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

Christopher Johnson
Email
Phone

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

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

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

Experience

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

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

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

Education

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

Skills

I speak English and Hawaiian (mother tongues).

Volunteering

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

References are available on request.

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

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

Dear Hiring Manager,

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

I have enclosed my resume.

I am looking forward to hearing from you soon.

Sincerely,

Emma Lewis
Email
Phone

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

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

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

Experience

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

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

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

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

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

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

Education

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

Skills

Fluent in English and Navajo (both native languages).

I have received training in food safety.

I have received CPR/AED and First Aid training.

Volunteer Experience

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

References available upon request.

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

Plus Language Signal and Occupation-Specific Skills, Cook

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

To Whom it May Concern,

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

I have enclosed my resume.

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

I am looking forward to hearing from you soon.

Sincerely,

Tyler King
Phone
Email

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

Online Appendix I: Additional Socioeconomic Status Statistics by Native American Tribal Group

Online Appendix Table I1 – Basic Socioeconomic Means by Native American Tribal Group

Indigenous Group	N	% High School Attainment	% Employed	Mean Income	% in a PUMA that includes an Indian Reservation
White	10,088,366	92.4%	94.6%	$47,553	16.5%
AIAN	137,632	83.3%	87.2%	$26,652	65.5%
Navajo	22,132	81.3%	83.8%	$20,024	89.1%
Apache	3,567	81.2%	80.8%	$22,280	58.8%
Blackfeet	1,380	87.9%	84.1%	$23,753	53.6%
Tohono O’odham	1,100	76.6%	80.3%	$18,677	78.5%

Notes: Data is from the American Community Survey data from 2010-2017. Our sample includes those of ages 26 to 65. Data for Oglala Lakota and Osage were not available. Income means include observations that are negative. The % in a PUMA [Public Use Microdata Area] variable comes from the HOMELAND variable in IPUMS-USA, which “indicates whether the household is in a PUMA [Public Use Microdata Area] that includes any Census block that was designated as an American Indian, Alaska Native, or Native Hawaiian homeland area.”

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