The Effect of Eldercare on Job Search: Evidence from a Correspondence Study *

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Abstract

We provide experimental evidence quantifying the extent to which revealing eldercare as the reason for leaving a previous job and being currently unemployed impacts callback rates. The correspondence study involves submitting fictitious applications to real job postings in four occupations across ten large cities in the USA. We find a significant 3.0 percentage point reduction in the callback rate for the eldercare applicants compared to the continuously employed applicants. The reduction is as large as it is for applicants with an unexplained gap and holds for men and women. These results have implications for policies aimed at supporting informal caregivers.

JEL Classification Codes: J23, J14, J16
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1 Introduction

In the United States, 22.5 percent of adults provide care to another adult and that prevalence has increased over time (National Alliance for Caregiving (NAC) and AARP 2025). Recent estimates suggest that more people are providing care to another adult than to a child with a disability or medical condition (National Alliance for Caregiving (NAC) and AARP 2020, 2025) and the value of unpaid care to other adults with limitations in daily activities totals $600 billion (Reinhard et al. 2023). Caregiving can reduce the employment, earnings, and hours worked of caregivers (for some examples, see Maestas et al. 2024; Van Houtven et al. 2013; Casella & Mazzone 2023; Byambasuren & González 2026). Given the literature documenting scarring effects from unemployment spells (for example, see Kroft et al. 2013; Eriksson & Rooth 2014), this raises questions about whether revealing eldercare as the reason for leaving the previous job and as the activity undertaken while unemployed can help job candidates. Without empirical research on how employers treat applicants who reveal the eldercare spell, these caregivers are turning to online discussion boards to try to figure out what is the best option.[1] Knowing whether it is better to reveal the eldercare spell could help informal caregivers who are trying to re-enter the labor market.

We conduct an experiment (correspondence study) in the United States to (1) quantify whether and to what extent revealing eldercare as the reason for leaving your previous job and currently being unemployed impacts the callback rate relative to continuously employed workers, and (2) to quantify whether and to what extent the callback rate for the eldercare providers differs from the callback rate for people who also have similar work histories but provide no explanation as to why. Correspondence studies have been widely used to study hiring differences based on many different applicant traits such as race, sex, age, parenthood, etc. (for a summary, see Bertrand & Duflo 2017). We follow the experimental design previously used to study how caregiving related to parenthood impacts callback rates (Weisshaar 2018; Tomlin 2022) by creating three types of fictitious applicants and applying to real job advertisements.[2] Our triplets include one unemployed applicant due to eldercare (eldercare group), one unemployed applicant without any explanation (unexplained group), and one applicant without any gap in the employment history (continuously employed group). The eldercare applicant provides an explanation for the unemployment spell by stating that he/she left to provide eldercare in both the résumé and cover letter. The group that does not provide an explanation has an unemployment gap of the same length as the eldercare treatment group, but the unexplained group neither provides a reason for the last job separation nor explains what was done during the unemployment

[1] See https://www.quora.com/How-do-I-put-on-my-resume-that-I-took-a-few-years-off-from-work-to-help-a-family-member-Should-I-leave-it-off-and-only-say-that-in-my-interview, https://www.agingcare.com/questions/explain-taking-time-off-work-for-caregiving-158441.htm and https://www.reddit.com/r/careerguidance/comments/1fq24xg/how_do_i_explain_the_gap_on_my_resume_while/ for some examples.

[2] Tomlin (2022) also includes a fourth group that includes nanny in the work history, but the other three groups are similar to our three groups.

spell. Given the ongoing discussions about labor supply of non-college educated workers, our fictitious applicants all report that their educational attainment is a high school diploma.

We apply to administrative assistant, customer service, receptionist, and sales associate jobs across ten large cities in the USA. In total, this amounts to applying for 3,037 jobs. Across numerous econometric specifications, we consistently find negative impacts on callback rates for the eldercare group relative to the continuously employed group. There is a significant 3.0 percentage point reduction in the callback rate, or a 14.6 percent reduction relative to the continuously employed callback rate of 20.6. This matches the reduction in callback rates for the unexplained group relative to the continuously employed group (3.0 percentage points). The reduction in the callback rate occurs for both male (3.7 percentage points) and female applicants (2.9 percentage points). This means that taking time off to provide eldercare leads to friction in labor market re-entry, even among jobs that do not require a college degree. Providing the eldercare explanation does not help shrink the reduction in callback rates relative to those who provide no explanation.

While the related literature on labor market impacts of caregiving for adults tends to quantify the penalties in earnings and employment stemming from the onset of caregiving, we study frictions in labor market re-entry after a caregiving spell ends. The related literature finds that the impact on male caregiver's hours worked is small, but there are larger effects among female caregivers who remain employed (Van Houtven et al. 2013). Male caregivers are negatively selected based on prior labor market outcomes, while women face labor supply reductions coinciding with the onset of care (Maestas et al. 2024). Focusing specifically on children of parents who experience health shocks, there are reductions in income and hours worked (Casella & Mazzone 2023), with daughters experiencing larger declines in earnings than sons (Byambasuren & González 2026). There are reductions in employment and hours worked among women in their 50s and 60s who provide care (Fahle & McGarry 2018). Adult children's labor market outcomes are more impacted in the US by a parent's health shock than in European countries (Bergeot et al. 2024).[3] A key exception that focuses on the dynamic impacts of care in the United States is Skira (2015), as she importantly includes frictions in shifting between full-time and part-time work and labor market re-entry in her structural model.[4] Our paper complements Skira (2015) by providing experimental evidence of frictions in labor market re-entry

[3]There is also a robust literature using data from other countries to quantify the impact of caregiving on labor market outcomes. See, for example, Schmitz & Westphal (2017) and Korfhage & Fischer-Weckemann (2024) for Germany; Rellstab et al. (2020) for the Netherlands; Wen & Huang (2024) for China; Chen & Lin (2022) for Taiwan; Simard-Duplain (2022) for Canada; Løken et al. (2017) and Ramirez Lizardi et al. (2025) for Norway; Frimmel et al. (2025) for Austria; Brito & Contreras (2025) for Chile; Akyol & Nolan (2025) for Australia; Costa-Font & Wang (2026) study how impact on labor supply change due to a policy related to workplace flexibility in the UK.

[4]There is also research from outside the United States suggesting caregiving can cause frictions in the labor market. For example, a factorial survey experiment was recently conducted in Flanders to document negative impacts from caregiving (eldercare and other types of non-eldercare) on interview and hiring scores reported by HR representatives (D'hert et al. 2025). In contrast, data from Australia reveals sharp rebounds in employment and hours worked after caregiving ends (Liu et al. 2026).

after providing eldercare in the United States.

We extend the literature on whether caregiving impacts callback rates by focusing on care to adults, while the related literature tends to focus on caregiving as related to parenthood. Mothers face a callback penalty relative to childless women, but fathers face no such penalty (Correll et al. 2007). Both mothers and fathers who reveal they opted out of the labor market to care for their children have a callback rate of around half that of mothers or fathers who were laid off due to downsizing, and parents who opted out also face very large reductions in the callback rates relative to those who remain continuously employed (Weisshaar 2018). Including being a stay-at-home mother in the work history still leads to an 18.2 percentage point penalty relative to mothers who are continuously employed (Tomlin 2022).[5] We contribute to the literature on the impact of caregiving on callback rates by finding negative effects due to revealing eldercare. We find that the magnitude of the eldercare reduction in callback rates is similar to the unexplained group, while Weisshaar (2018) finds larger reductions in callback rates for parents who opted out compared to those who report being laid off. An important caveat to this comparison is that we did not clarify whether our unexplained group was laid off or not.

We also contribute to the literature on caregiving by focusing on young adults, a group that has received less attention in the literature, even though 24% of caregivers for other adults are between 18 and 34 years old (National Alliance for Caregiving (NAC) and AARP 2020). The age at which a woman starts providing care affects the magnitude of the effect on both earnings and employment; that is, younger women (34-50 years old) face larger decreases in earnings and employment relative to older women (Maestas et al. 2024). This motivates our decision to focus on an even younger group of caregivers in our experiment. Young adults may be even more financially impacted by caregiving due to having accumulated less savings prior to the caregiving spell and thus may be less able to consumption smooth during the unemployment spell. This paper documents meaningful reductions in callback rates for both men and women who reveal eldercare relative to the continuously employed group, which indicates that these younger caregivers may need more assistance with labor market re-entry to avoid severe financial constraints.

The remainder of the paper is organized as follows. Section 2 provides more background on caregiving and theoretical predictions. Section 3 discusses the experimental design, including the résumé creation and implementation of the experiment. Section 4 discusses the results from the experiment. Section 5 summarizes and concludes.

[5]Pregnant women also face penalties when trying to find a job (Morgan et al. 2013).

2 Background on Caregiving

2.1 Caregiving in the US

Eldercare providers help with activities of daily living (ADLs), which are basic self-care tasks such as bathing, eating, dressing, and getting out of bed, and instrumental activities of daily living (IADLs) such as grocery shopping, household chores, managing finances, and providing transportation. Estimates suggest that 69 percent of people turning 65 in 2005 will need some form of long-term support or service (Kemper et al. 2005). Eldercare can be provided by formal sources, such as nursing homes and home healthcare agencies, or informal sources, such as family members and friends. Approximately 10.6 million people provide informal care, compared to around 1.9 million formal care workers (Gruber & McGarry 2025). Those who turned 65 in 2005 are estimated to need an average of 1.9 years of home care, with 1.4 years of that coming from only informal care, on average (Kemper et al. 2005).

Costs of formal care, lack of private insurance coverage, and a shortage of formal care workers all contribute to the reliance of informal care. The median annual cost of a home health aide was $77,792 and the cost of a semi-private nursing home was $111,325 in 2024 (Clark 2025). Due to these costs, many cannot afford formal care and among those who do have formal care, most of the costs are paid for through Medicaid (45.6%) or Medicare (18%) (Colello & Sorenson 2025). Medicare only covers formal care that is "medically necessary" and includes a limit to nursing home care of 100 days; however, Medicaid will provide for more types of care, but it is a means-tested program so individuals must spend down their own resources before they can qualify (Gruber & McGarry 2025). Individuals may also purchase private insurance to cover formal care, but take-up rates are low due to high cost and poor coverage (Rau & Aleccia 2023) and therefore private insurance covers only a small portion of caregiving costs (Johnson & Dey 2022; Colello & Sorenson 2025). While some state Medicaid programs have been allowed to expand coverage of home care (Gruber & McGarry 2025), the shortage of home health care providers continues to be well-documented (for example, see Shoemake 1994 and Shaw et al. 2022).

Due to the limitations in formal care, eldercare in the United States tends to be provided by informal caregivers, as informal care is seen as a substitute for formal care (Charles & Sevak 2005; Barczyk & Kredler 2018; Mommaerts 2018; Lieber & Lockwood 2019). The supply of foreign-born home care workers is negatively associated with informal care provision (James & Grecu 2026). Most adults receive this care from a family member (Mommaerts 2025), with an average of 26 hours per week dedicated to caregiving tasks (Gruber & McGarry 2025). These informal caregiving responsibilities are not evenly distributed across the population. Women account for roughly 60 percent of caregivers, nearly one-quarter are young adults aged 18 to 34 years, and the most common level of education among caregivers is a high school diploma (National

Alliance for Caregiving (NAC) and AARP 2020). These stylized facts motivate our experimental design, as we want our fictitious applicants to have characteristics like those of real eldercare providers.

2.2 Theoretical Predictions

Prolonged unemployment spells negatively impact callback rates because human capital depreciates during that time, and the spell sends a negative signal about the applicant's quality. Since both the unexplained group and the eldercare group have the same amount of time away from the labor market, both groups will face skill depreciation; therefore, the key question is whether the eldercare signal is different from the unexplained signal. It is important to note that our eldercare treatment involves information about why the applicant left their last job and what they were doing during their unemployment spell, while the unexplained group provides neither information regarding why they separated from their previous job nor what they were doing during their unemployment spell. Our experiment is therefore unable to determine whether any benefit of revealing eldercare relative to the unexplained group is due to providing information about the reason for the job separation and/or providing information about how the time was spent while unemployed. However, we believe that the first-order question is whether there is any difference in callback rates, and if we find a difference, then mechanisms could be explored in the future.

It is theoretically unclear whether the eldercare treatment group will have similar callback rates, smaller callback rates, or larger callback rates than the unexplained group. Eldercare is difficult to verify on the part of employers (as opposed to being able to verify time off due to enrolling in an educational program, for example), so the eldercare group might have a similar callback rate to the unexplained group. The eldercare group could have a worse callback rate relative to the unexplained group if eldercare workers are viewed as violating the "ideal worker norm" (Davies & Frink 2014) more than the unexplained group, since firms could perceive eldercare providers as prioritizing family responsibilities ahead of work responsibilities. Firms could also believe that past eldercare responsibilities are predictive of future eldercare responsibilities, and thus view eldercare applicants as more costly to hire due to higher turnover rates. Eldercare could be viewed more positively relative to the unexplained group if employers view eldercare responsibilities as being uncorrelated with productivity, as such signals could alleviate some of the scarring effects relative to the unexplained group (Namingit et al. 2021). Thus, we will empirically test whether the eldercare treatment (relative to the continuously employed group) is statistically different than the unexplained treatment (relative to the continuously employed group).

We hypothesize that sex could be an important source of heterogeneity to explore because whether eldercare is correlated with productivity likely varies between men and women. Two key reasons women end

up being caregivers over men is due to gender norms and differences in opportunity costs (Barigozzi et al. 2020; Byambasuren & González 2026). Theoretically, we would expect that men who are doing poorly in the labor market (i.e., less productive men) are less able to negotiate their way out of providing care using the opportunity cost argument. Empirical evidence is also consistent with this: Men who provide care are self-selected based on worse labor market outcomes prior to the onset of care (Maestas et al. 2024), men are more sensitive to unemployment insurance benefits in that they are more likely to provide care when unemployment insurance is less generous (Truskinovsky 2021), and whether or not men provide informal care is countercyclical in nature (Mommaerts & Truskinovsky 2020). We hypothesize that eldercare sends a negative signal about productivity for men and thus the eldercare group will have lower callback rates relative to the unexplained group for our male applicants. Women who provide care often do so because they are expected to do it (Barigozzi et al. 2020; Byambasuren & González 2026) and are not negatively selected based on prior labor market outcomes (Maestas et al. 2024). We hypothesize that signaling that the gap was due to eldercare will alleviate some of the scarring effect for women, meaning that the gap in callbacks relative to the continuously employed group will be smaller for the eldercare group than the unexplained group.

3 Experimental Design

3.1 Résumé Parameters and Creation

We conduct a correspondence study which means we create fictitious applications and submit them to real job postings. For each job posting, we submit a set of three applications, where our experimental arms are based on following Weisshaar (2018) and, to a lesser extent, Tomlin (2022). The first résumé contains the eldercare treatment, which is signaled in the work history as the reason for leaving their last job, following Weisshaar (2018). The exact phrasing is: "Left to provide care to an elderly relative". We also signal eldercare treatment through the cover letter, adjusting the phrasing used in Namingit et al. (2021) to fit eldercare.[6] The eldercare signal in the cover letter is: "I took time away from work to provide care to an elderly family member. I am ready to return to work now that the care is no longer needed." The second applicant in the triplet has an unexplained employment gap of the same duration (two years), with no information provided about the reason for the gap in either the résumé or cover letter. A two-year unemployment period is chosen because it is long enough for scarring effects to occur (Kroft et al. 2013; Eriksson & Rooth 2014) and fits the average duration of care provided at home (Kemper et al. 2005). The

[6] We upload a cover letter whenever there is the option to do so.

third applicant has no gaps in their work history and is conducting an on-the-job search.[7] The triplet allows us to compare the gap between the eldercare treatment and the continuous employment group with the gap between the unexplained group and the continuously employed group.

Occupations: Our applicants have a work history in one of four different occupations and are applying for jobs that match their previous occupation. They are (1) administrative assistants, (2) customer service representatives, (3) receptionists/secretaries, and (4) sales associates. We use these occupations because they have enough online job postings to achieve sufficient power for our study. These occupations are also widely used in correspondence studies (for some examples, see Bertrand & Mullainathan (2004); Oberholzer-Gee (2008); Kroft et al. (2013); Gaulke et al. (2019); Namingit et al. (2021); Tomlin (2022)).

Work histories: Work histories are structured to reflect 5 years of total experience for continuously employed applicants or 3 years for the unexplained and eldercare applicants. Tenure for each job within the work history is a maximum of two years, reflecting the median turnover normally seen for applicants in the age group we study (Bureau of Labor Statistics 2024). We have a bank of job titles and description pairs that are randomly assigned to each applicant and position inside the work history. No job title/description combination can be used more than once inside of a triplet, so applicants inside of a triplet have completely different jobs within their work histories. Applicants who are assigned the occupation of administrative assistants have an ascending structure in their history, similar to Neumark et al. (2019). We assign a receptionist or secretary position as the applicant's first job. Then their second job, and third job if assigned as a continuously employed applicant, is an administrative assistant role. For the other three job categories, we randomly assign a job from our bank to each position inside the work history, without an ascending structure due to a lack of experience for the next step up the career ladder.

Names: To avoid confounding the effect of eldercare with the sex of the applicant, we keep sex constant within the triplet but randomize between the triplets. Sex is signaled through names. First names come from the Social Security Administration's Popular Baby Names list for the years 1995-2004 (U.S. Social Security 2024). We selected first names based on the frequency in which they appear at the top of the list during those years and their similar scores on the warmth and competency ratings (Newman et al. 2018). Last names are selected from the top one thousand most frequently occurring surnames in the 2010 Census (U.S. Census Bureau 2021). We choose last names that are in the top 100 most common and that have more than 75% of individuals identifying as white. We arrive at a set of names that overlap with those used in Neumark et al. (2019), where their intention was also to signal white applicants. We have three male names

[7]Our continuous employment group does not experience a move across the country like in Namingit et al. (2021) for two reasons. Our caregivers have a high school diploma as their educational attainment, which means they are less geographically mobile than college graduates (Dahl 2002; Wozniak 2010; Molloy et al. 2011). Also, indicating that the applicant is moving for family reasons is vague enough that it could include caregiving.

and three female names, and the names are randomized within the triplet. The names used in our study are John Nelson, David Phillips, Daniel Roberts, Hannah Nelson, Sarah Phillips, and Samantha Roberts. Although eldercare responsibilities affect all groups of people, we focus solely on white applicants since our budget for the study did not allow us to send enough applications to be able to test for differences by race. This is a limitation of the study.

Location: The applicant's résumé lists the city or labor market where he/she is applying rather than their full address, following current trends (Fennell 2025; Eames 2025). Our applicants reside in one of the ten large cities that were picked based on having a large enough job market to ensure enough job advertisements and to allow for geographic variation within the United States. These cities include Boston, MA; Chicago, IL; Denver, CO; Houston, TX; New York, NY; Philadelphia, PA; Phoenix, AZ; San Francisco, CA; Washington D.C.; and Seattle, WA.

Phone numbers: We start by selecting area codes for each city, based on the area codes available when our applicants would have generally been old enough to get their first cellphone (which is around 16 years old based on Rideout et al. 2021). This tends to mean that we use the most recently created area code. We purchase phone numbers for our applicants through Vumber, which provides virtual numbers that appear no different from regular phone numbers. These virtual numbers allow all voicemails to be recorded in an online account, ensuring no direct phone interaction with potential employers. To ensure the applicant's area code matches the city in which we are applying we purchase 60 phone lines where each number is a unique combination of city (ten cities) and name (six names). This also reduces the chance of misallocating a callback to the incorrect applicant while remaining feasible to routinely check for callbacks.

Email addresses: We have six different email addresses, one for each name. The assignment of usernames follows Neumark et al. (2019) to avoid sending any additional signal.[8] Varying the email addresses by name aims to reduce the likelihood of misallocation of callbacks to applicants.

Education: All applicants have a high school degree, and none list additional educational enrollment or attainment, since high school is the most common level of education reported by caregivers (National Alliance for Caregiving (NAC) and AARP 2020). We pick the three median ranked high schools from the U.S. News HS Rankings list for each of the ten cities in our study. We use public schools to avoid sending any additional signal about our applicants (for example, religion). We use schools of similar rank to avoid sending any additional signal about the applicant's human capital. We pick schools in the middle of the ranking to make it believable that our applicants have a high school education as their highest level of education. One

[8]Each triplet contains one username of each type: full first and last name, first initial and full last name, or full first name and last initial. They are also either randomly assigned a middle initial (excluding l, y, z, q, u, and x), a period, underline, or nothing between the first and last name/initial. Sometimes we need to append a 1 or 2 at the end of the name to ensure that the email address is available, although some do not have any number.

of the three high schools is randomly assigned to each applicant within the triplet. We signal the applicant's age by assigning a high school graduation year.

Skills: Based on the pilot, all applicants are assigned certain skills to be competitive. Applicants assigned to receptionist/secretary, or administrative assistant positions list Microsoft Office programs as a skill, while those assigned to customer service representative or sales associate positions list experience with point-of-sale (POS) systems. Following Neumark et al. (2019), applicants also receive randomly assigned "buzzwords" (for example, reliable) to mirror the language used in typical résumés.

Interests: Each résumé is assigned two interests. The first interest listed is one of the following: recreational basketball, recreational softball, or hiking. The second interest listed is either painting, photography, or baking. We use these interests to enhance the realism of our résumés while keeping the signal minimal. We avoid volunteer activities because they can independently impact callback rates (Baert & Vujić 2018).

Templates and Objectives: We use three résumé and cover letter templates to prevent employers from detecting our experiment. Templates and naming conventions are randomly assigned inside each triplet.[9] Each applicant within a triplet receives a randomly assigned objective. The objectives vary in exact phrasing, but all indicate that the applicant is interested in a role within their assigned occupation.

Details of how the résumés are formatted for each experimental arm are provided in Appendix A. The brackets refer to where the information is added through the randomization process. More details on the specifics (for example, the list of area codes) are provided in Appendix B.

3.2 Implementation

Our experiment involves applying for jobs through a major online job board in the United States. It started in October 2025 and ran through September 2026. We sent out 8,587 fictitious résumés to 3,037 job advertisements, which closely matches our pre-specified target of 3,033 jobs. Our aim was a 70-30 female to male split because the mean callback rates across experimental arms in the pilot study indicate that we need a larger sample size for women than men to detect effects within sex-specific samples.

We start by identifying eligible job advertisements using our study protocol. This protocol is in place to ensure that our applicants have a positive probability of receiving a callback. The job locations must be within a twenty-five mile radius of the major city in which we are applying as we want to focus on those advertisements with a feasible commute. Each job advertisement needs to have been posted within the last 3 to 5 days (depending on which job board is being used) to minimize the number of job advertisements that close in the middle of a triplet being sent out. Job advertisements must also require three or fewer years of

[9] All résumés and cover letters are given one of three different naming conventions. One résumé is labeled "FirstLastResume," another as "ResumeFirstLast," and the final as "FirstLast," with "First" and "Last" replaced by the applicant's first and last names.

experience to fit our applicant's work history. We exclude job advertisements that require more than a high school degree or technical qualifications, since our applicants only have a high school diploma. We choose job advertisements that allow direct uploading of résumés and cover letters, exclude job advertisements that ask applicants for an in-person appearance or call, and exclude job advertisements that require uploading résumés and cover letters to an external site, following (Gaulke et al. 2019; Neumark et al. 2019; Namingit et al. 2021). Once we find an eligible job, we apply using one of the triplets. The order in which applications are submitted is randomly assigned within a triplet. More details on the implementation of the experiment are provided in Appendix B.

After submitting the applications, we measure the callback rate, which is our main outcome of interest. A callback can be in the form of an email or a voicemail. Callback is a dummy variable that is set equal to one if an applicant receives an unambiguously positive response (i.e., asking for our applicant to schedule an interview) or a neutral response (i.e., asking for more information). Otherwise, it is set to zero. We study the callback rate within a two-week period after the application's submission since more than 90% of all callbacks occurred within two weeks during the pilot, and previous work has also found that most callbacks occur within two weeks (for example, see Kroft et al. 2013).[10]

After receiving responses from potential employers, we respond that the applicant is no longer available for the job. If an employer leaves a voicemail, we send a voicemail using Slybroadcast. The Slybroadcast system sends our call directly to voicemail so the recording can be played there, ensuring that we have no direct interaction with employers. If we receive an email, we respond via email to the employer. Our email and voicemail scripts vary within the triplet (we have three email scripts and three voicemail scripts).

4 Results

4.1 Descriptive Statistics and Balance Test

Table 1 shows descriptive statistics of our overall sample and separately by experimental arm. Our sample is 30 percent male and 70 percent female. Sales and customer service jobs were easier to find and thus made up 31.7 and 30.4 percent of the sample, respectively. Administrative assistants made up 11.5 percent and receptionists made up 26.3 percent. The overall callback rate is 18.5 percent. Continuously employed applicants had the highest callback rate of 20.6 percent, while the unexplained group and eldercare group have extremely similar callback rates (17.5 percent).

We next test for balance on the characteristics included in Table 1, excluding the callback rate, using

[10] During the main study, we received just over 97% of our callbacks within two weeks.

the World Bank's Iebaltab command (DIME Analytics 2023). The test compares characteristics within each set of experimental arms, and the results are shown in Table 2. We find no evidence of selection into any experimental arm of the study based on these characteristics, in accordance with using the the résumé randomizer from Lahey & Beasley (2009) to assign characteristics between groups of applicants.

We next descriptively compare the means of callback rates across our experimental arms, shown in the top row of Table 1. The eldercare group and the unexplained group both have significantly lower callback rates than the continuously employed group. Finding higher callback rates for continuously employed applicants than for those with an unexplained gap matches the previous literature (Kroft et al. 2013; Namingit et al. 2021). The eldercare group and the unexplained group do not have significantly different callback rates. This contrasts with Namingit et al. (2021) which finds that revealing cancer as the reason for an unemployment spell leads to a higher callback rate than that of the unexplained group.

Table 3 compares the distribution of callbacks across the possible combinations of callbacks each triplet could receive. The most common outcome is that none of the applicants in the triplet got a callback (73.5 percent), followed by all three getting a callback (9.4 percent). Among triplets in which only one applicant received a callback, the most common outcome was that only the continuously employed group gets a callback (5.0 percent versus 2.7 for unexplained only and 2.3 for eldercare only). Among the triplets in which two callbacks occur, the most common outcome is a callback for both the continuously employed applicant and eldercare applicant. There are some differences in the probability of having no callbacks by occupation (not shown in the table). The sales occupation had the lowest rate of zero callbacks (66.7 percent), followed by customer service (74.5 percent), receptionists (76.8 percent) and administrative assistants had the highest rate of zero callbacks (81.7 percent).

4.2 Methodology

We next more formally test for differences across our three experimental arms in terms of the callback rates by estimating:

$$Y_{ij} = \beta_1 E_i + \beta_2 U_i + \boldsymbol{R}'_{ij}\lambda + \boldsymbol{A}'_j\delta + \epsilon_{ij} \tag{1}$$

where Y_{ij} is a dummy variable for whether the individual i who applied to job j received a callback. This is set equal to one if the applicant got a positive or ambiguous response and zero otherwise. E_i is a dummy variable set equal to 1 if applicant i has an eldercare related gap, U_i is a dummy variable set equal to 1 if applicant i has an unexplained gap, and the continuously employed group is the omitted group. β_1 is the

estimate of the effect of an eldercare gap on the probability of receiving a callback relative to the continuously employed group. β_2 estimates the effect of an unexplained gap on the probability of receiving a callback relative to the continuously employed group. We expect that β_2 will be negative. To test whether revealing eldercare affects the scarring effect relative to the unexplained gap, we test whether β_1 is statistically different from β_2.

We include two sets of controls: $\boldsymbol{R}$ is a vector of résumé characteristics and $\boldsymbol{A}$ is a vector of job advertisement characteristics. The vector of résumé characteristics contains: the order of being sent within the triplet, template used, name, and high school.[11] The vector of job advertisement characteristics contains: the month-year the job advertisement was posted, sex of the triplet, city, whether the job was full-time or not, and RA fixed effects. Standard errors are clustered at the job advertisement level.

We conduct a robustness check that instead includes job advertisement fixed effects and thus drops all covariates that are constant within job advertisement (for example, occupation, city, and sex of the applicant). Specifically, we estimate:

$$Y_{ij} = \beta_1 E_i + \beta_2 U_i + \boldsymbol{R}'_{ij}\lambda + \rho_j + \epsilon_{ij} \tag{2}$$

where ρ_j is the job advertisement fixed effect. The set of résumé characteristics is the same as in equation 1. Standard errors are still clustered at the job advertisement level.

Additionally, we re-estimate equation 1 using a logit instead of the linear probability model. The sets of résumé and job advertisement controls remain the same, and we still cluster the standard errors by job advertisement. We present marginal effects in the tables, which are computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means.

4.3 Results

Table 4 presents the main results. Column (1) shows the results from equation 1, our main specification. The results indicate that the eldercare treatment results in a significant 3.0 percentage point reduction in the callback rate relative to the continuously employed group. This corresponds to a 14.6 percent reduction. The unexplained group also experiences a significant 3.0 percentage point reduction in the callback rate relative to the continuously employed group and therefore there is no significant difference in the callback rate between the two groups. Column (2) shows the results from estimating equation 2. Note that the sample

[11] We had originally wanted to potentially control for days since the job advertisement was posted, but the online job board changed what information was available and we were therefore unable to determine this.

size is smaller because we condition the sample to only include observations in which all three applications were submitted before the job ad closed. Otherwise, the job fixed effect could result in an application specific fixed effect. The results are extremely consistent with those in column (1) in that the eldercare reduction in callback rates is 3.2 percentage points, and it is not significantly different from the reduction in callback rate for the unexplained group. In column (3) we present the specification in which we use the logit version of equation 1. The results are very robust to this alternative specification. We also re-estimate columns (1) and (3) using the same sample restriction as column (2) and find results that are comparable in direction and magnitude to our main results. These results are shown in Table C1.

Our results are robust to many alternative samples. Since large nationwide firms could have jobs in multiple cities or multiple positions within a city, those types of jobs may be overrepresented in the sample. While we have a 30-day rule about applying to the same company and try to avoid it beyond 30 days, there are some companies in which multiple sets of applications were submitted. To examine whether these large firms are driving the results, we reproduce Table 4 but change the sample to only include one set of applications per large firm. The results are shown in Table C2. The impact of revealing eldercare is the same as before: column (1) shows there is still a significant decline of 2.8 percentage points relative to the continuously employed group. The other two columns with the alternative specifications are robust as well. The magnitude remains statistically indistinguishable from the reduction for the unexplained group. Table C3 shows the results where we re-estimate equation 2 but also include RA fixed effects to take into account the fact an RA had to occasionally step in and finish another RA's triplet. The results are very robust (see column 1) as the estimated reduction in callback rates is still 3.2 percentage points. We also estimate an alternative way to adjust for RAs occasionally covering for each other by restricting the sample to only including observations in which all applications within a triplet were submitted by the same RA. Columns (2), (3), and (4) in Table C3 present these results. The estimates are qualitatively the same across the three specifications previously used.

We next test for differences by sex of the applicant, which is the margin of heterogeneity that we pre-specified in our analysis plan. To do this, we split the sample by sex of the applicant triplet and re-estimate all three specifications. We decided to focus on splitting the sample instead of using interaction terms to allow the controls and fixed effects to vary by sex.[12] Results are shown in Table 5. Men with eldercare treatment are 3.7 percentage points less likely to receive a callback relative to a continuously employed male (column 1), an effect size that is not significantly different than the effect size for males in the unexplained group. For women, revealing eldercare leads to a 2.6 percentage point reduction in the callback rate relative to the continuously employed group (column 4). Similar to men, this effect size is not significantly different

[12]This matches what we stated in the RCT registry.

than for the unexplained group. We also test if callback rates are significantly different between men and women. There is neither a significant difference among the eldercare groups (p-value = 0.545) nor among the unexplained groups (p-value = 0.412). Our results for men and women are robust to instead using equation 2 (shown in columns 2 and 5) and to re-estimating equation 1 using the logit specification (shown in columns 3 and 6).

We also conduct a similar set of robustness tests for the effects by sex as we did for the full sample. Table C4 displays the results for restricting the sample to include only one set of applications for large firms. The estimates are in line with our main results, with significant reductions in the callback rate for the eldercare group that is of the same magnitude as the unexplained group. Table C5 contains multiple robustness checks to account for situations where an RA would finish sending out another RA's triplet. Columns (1) and (5) show the results for men and women when estimating equation 2 with RA fixed effects. These results are very robust, with the reductions in callback rates the same as those estimated in columns (2) and (5) of Table 5. The remaining columns re-estimate our three specifications separately by sex, while restricting the sample to triplets where one RA sends out all three applications. The results again remain robust to this sample change. Taken together, our results are consistent with significant reductions in the callback rate for both men and women in the eldercare treatment relative to the continuously employed group. Neither sex benefits from revealing eldercare relative to the unexplained group, suggesting that revealing eldercare does not reduce scarring effects.

We next allow for the effects to vary by occupation, as shown in Table 6. Note that this is not a pre-specified heterogeneity analysis and thus should be interpreted as exploratory in nature. All results come from the estimation of equation 1 with the linear probability model. The results are largely consistent with what we found for the full sample. The point estimates are negative, although not always significant due to smaller sample sizes within occupations.[13] The reduction in callback rates for the eldercare group is never significantly different than the reduction for the unexplained group at the five percent level. This suggests that eldercare is treated similarly across these occupations, although future research could be done on an expanded set of occupations to further study this.

4.4 Discussion

In Section 2.2 we discuss that it is theoretically unclear whether revealing eldercare would lead to a different treatment effect than that for the unexplained group. Although both groups face human capital depreciation due to having the same duration of the unemployment spell, eldercare could send a different signal than the

[13] Given that our key area of focus for heterogeneity was the sex of the applicant, our sample size was based on having power within each sex and not within each occupation.

unexplained group sends. However, we find that the magnitude of the treatment effect for the eldercare group is the same as the treatment effect for the unexplained group, which is consistent with the eldercare signal not being better or worse than saying nothing. This could be due to the fact that eldercare is not something that could be easily verified (and therefore may not even be trusted as the real reason by potential employers) or the fact that eldercare skills are not viewed as easily translating to the skill sets required for the occupations that were included as part of the study. It could also be the case that potential employers view those with a prolonged unemployment spell as lower quality/less productive (as in, they are "scarred," as documented in Kroft et al. 2013 and Eriksson & Rooth 2014) regardless of whether they provide eldercare or not. This could be plausible if potential employers view those who select into eldercare as being less productive or lower quality workers to begin with and thus as undesirable as any worker with a prolonged unemployment spell. Despite men and women having different reasons to provide eldercare, we find that the reduction in callback rates relative to the continuously employed group is the same for both sexes. This is consistent with potential employers not viewing the eldercare signal differently based on the sex of the applicant.

Our experiment has several limitations. The set of fictitious applicants is based on a specific group of potential workers: white people living in large cities who are in their early/mid-twenties who report that their highest level of education is a high school diploma. The eldercare treatment effect could certainly vary with a different set of characteristics. For example, given the age group we focus on, employers may assume that our female applicants who provide no explanation have been stay-at-home mothers of young children during their unemployment spell, while they may have other beliefs for older women. Thus, employer beliefs about the unexplained group could vary, which could then impact whether and to what extent the eldercare treatment differs from the unexplained treatment. Furthermore, one could expand the set of occupations in future work, as providing eldercare could be viewed more positively in caring focused occupations (like formal caregiving jobs). Questions about how representative online job boards are have been raised (Dalton et al. 2026) but in a recent survey 68.8 percent of employers report that all or most of their hiring is done through online job boards/recruiting platforms and 39.2 percent report increasing their reliance on online job boards/recruiting platforms in the last year (iHire 2026). Online job boards have been found to be more used for job search and applications among those in urban areas (Smith 2015) which is part of the reason we focus on large cities. This means that our results may have less external validity for those living in more rural areas. Lastly, our study focused on a specific way to signal eldercare by using an eldercare variation of parental opt out behavior in Weisshaar (2018). An alternative would be to instead list eldercare as a job, akin to Tomlin (2022). We are currently working on securing grant money to do this as a follow-up experiment. A limitation of all correspondence studies is that they only provide information on one stage of

the hiring process: the callback rate. The key benefit is that examining a later stage in hiring would require information on prior stages that we usually do not have data on (Kroft et al. 2013); further, later stages of hiring are conditional on passing the callback stage, which this paper documents is impacted by eldercare.

Previous work has documented that informal caregiving has a cost (for example, see Van Houtven et al. 2013; Skira 2015; Fahle & McGarry 2018; Casella & Mazzone 2023; Maestas et al. 2024; Bergeot et al. 2024; Byambasuren & González 2026). Research documenting negative impacts on labor supply at the start of caregiving indicates that eldercare could impact the larger economy in many ways, from aggregate labor supply, which can impact economic growth, to how fiscally viable Social Security and Medicare are going forward with people not paying as much into those programs when they reduce their labor supply. Our paper extends Skira (2015) by providing experimental evidence consistent with her point on the need to account for potential frictions in the labor market. The significant and negative impact on the callback rate for eldercare applicants relative to continuously employed applicants suggests that the estimated cost of eldercare is inaccurate. We provide evidence that the cost of eldercare should include not only lost productivity and lower payments into social insurance programs when people are providing eldercare instead of working, but also in the time it takes to resolve the re-entry friction. If people have persistent challenges in re-entry, this could lead to even higher rates of people leaving the labor force altogether.

5 Conclusion

We quantify whether and to what extent eldercare impacts callback rates by conducting an experiment in which we submit fictitious applications to real job advertisements in four occupations and ten large US cities. We find that revealing eldercare as the reason for the two-year unemployment spell leads to a significant 3.0 percentage point (14.6 percent) reduction in the callback rate relative to the continuously employed applicant. This reduction is the same magnitude as the reduction in callback rates for those who have been unemployed for two years but provide no explanation (unexplained group). This pattern holds for both men and women and is robust to multiple alternative specifications and samples.

These results have implications for on-going policy debates surrounding the appropriate role of government in providing eldercare: if eldercare responsibilities fall to informal caregivers and those people need to stop working to provide that care, they will face challenges when trying to re-enter the labor market. A thorough cost-benefit analysis would, therefore, consider these frictions in weighing different policy choices. Future research could help inform this debate by evaluating different policy proposals aimed at helping informal caregivers to determine which are most effective and efficient.

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Table 1. Descriptive Statistics

	(1) All	(2) Continuous	(3) Unexplained	(4) Eldercare
Callback	0.185	0.206	0.175	0.175
	(0.388)	(0.404)	(0.380)	(0.380)
Sex				
Male	0.298	0.297	0.298	0.298
	(0.457)	(0.457)	(0.458)	(0.458)
Female	0.702	0.703	0.702	0.702
	(0.457)	(0.457)	(0.458)	(0.458)
Occupation				
Administrative Assistant	0.115	0.115	0.113	0.118
	(0.319)	(0.319)	(0.317)	(0.322)
Customer Service	0.304	0.302	0.307	0.302
	(0.460)	(0.459)	(0.461)	(0.459)
Receptionist	0.263	0.265	0.264	0.261
	(0.441)	(0.442)	(0.441)	(0.439)
Sales Associate	0.317	0.317	0.316	0.319
	(0.466)	(0.466)	(0.465)	(0.466)
City				
Boston	0.101	0.101	0.102	0.101
	(0.302)	(0.301)	(0.303)	(0.302)
Chicago	0.103	0.103	0.104	0.103
	(0.304)	(0.304)	(0.305)	(0.304)
Washington DC	0.103	0.104	0.103	0.103
	(0.304)	(0.306)	(0.304)	(0.304)
Denver	0.097	0.098	0.096	0.098
	(0.296)	(0.297)	(0.295)	(0.297)
Houston	0.099	0.100	0.097	0.101
	(0.299)	(0.300)	(0.296)	(0.301)
New York City	0.105	0.102	0.106	0.106
	(0.306)	(0.303)	(0.307)	(0.308)
Philadelphia	0.103	0.104	0.105	0.102
	(0.305)	(0.305)	(0.306)	(0.302)
Phoenix	0.100	0.101	0.097	0.101
	(0.299)	(0.301)	(0.297)	(0.301)
Seattle	0.097	0.098	0.097	0.094
	(0.295)	(0.298)	(0.297)	(0.292)
San Francisco	0.091	0.090	0.093	0.092
	(0.288)	(0.286)	(0.290)	(0.289)
Observations	8,587	2,880	2,842	2,865

Note: Each observation is one application. Each column shows the mean of each characteristic followed by the respective standard deviations in parentheses.

Table 2. Joint Test of Balance by Treatment Arm Pairs

Comparison	F-statistic	p-value
Continuously Employed vs. Unexplained Gap	0.190	0.903
Continuously Employed vs. Eldercare	0.385	0.764
Eldercare vs. Unexplained Gap	0.180	0.910

Note: Each row is a joint F-test testing whether the covariates in Table 1 predict assignment to one experimental arm relative to another, excluding the callback variable.

Table 3. Distribution of Applications Across Number of Callbacks

	Number	Percent
No Callbacks	2233	73.5
One Callback - Continuous	153	5.0
One Callback - Unexplained	81	2.7
One Callback - Eldercare	70	2.3
Two Callbacks - Continuous & Unexplained	69	2.3
Two Callbacks - Continuous & Eldercare	84	2.8
Two Callbacks - Unexplained & Eldercare	61	2.0
Three Callbacks	286	9.4
Total	3037	100

Note: Each observation is one triplet.

Table 4. Impact on Callback Rate Relative to Continuously Employed

	(1) Callback	(2) Callback	(3) Callback
Unexplained Gap	-0.030***	-0.032***	-0.030***
	(0.007)	(0.007)	(0.007)
Eldercare	-0.030***	-0.032***	-0.030***
	(0.006)	(0.007)	(0.006)
LPM w/ résumé and job ad controls	Yes	No	No
LPM w/ résumé controls and job ad fixed effects	No	Yes	No
Logit w/ résumé and job ad controls	No	No	Yes
Observations	8,587	8,091	8,587
Mean Callback Rate Continuously Employed	0.206	0.209	0.206
p-value$_{\text{Evs.U}}$	0.962	0.996	0.985
Clusters	3,037	2,697	3,037

Note: Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Marginal effects are reported for column (3), computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. Columns (1) and (3) include job ad controls for the month-year the job advertisement was posted, sex, city, occupation, whether the job was full-time or not, and RA fixed effects. Column (2) include job ad fixed effects. Column (2) is restricted to triplets where all résumés are sent out before the job ad closes. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5. Impact on Callback Rate Relative to Continuously Employed by Sex

	Male			Female		
	(1) Callback	(2) Callback	(3) Callback	(4) Callback	(5) Callback	(6) Callback
Unexplained Gap	-0.036**	-0.044***	-0.034**	-0.027***	-0.027**	-0.027***
	(0.012)	(0.012)	(0.011)	(0.008)	(0.008)	(0.008)
Eldercare	-0.037***	-0.038***	-0.035**	-0.026**	-0.029***	-0.026***
	(0.011)	(0.011)	(0.011)	(0.008)	(0.008)	(0.008)
LPM w/ résumé and job ad controls	Yes	No	No	Yes	No	No
LPM w/ résumé controls and job ad fe	No	Yes	No	No	Yes	No
Logit w/ résumé and job ad controls	No	No	Yes	No	No	Yes
Observations	2,558	2,400	2,558	6,029	5,691	6,029
Mean Callback Rate Continuously Employed	0.188	0.195	0.188	0.213	0.215	0.213
p-value$_{\text{Evs.U}}$	0.896	0.536	0.896	0.874	0.772	0.901
Clusters	908	800	908	2,129	1,897	2,129

Note: Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Marginal effects are reported for columns (3) and (6), computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. Columns (1), (3), (4) and (6) include job ad controls for the month-year the job advertisement was posted, city, occupation, whether the job was full-time or not, and RA fixed effects. Columns (2) and (5) include job ad fixed effects. Columns (2) and (5) are restricted to triplets where all résumés are sent out before the job ad closes. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6. Impact on Callback Rate Relative to Continuously Employed by Occupation

	Administrative Assistant			Customer Service			Receptionist			Sales Associate		
	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female	(7) All	(8) Male	(9) Female	(10) All	(11) Male	(12) Female
Unexplained Gap	-0.031	-0.036	-0.027	-0.028*	-0.028	-0.026	-0.030*	-0.050*	-0.026	-0.036**	-0.022	-0.039**
	(0.019)	(0.027)	(0.024)	(0.012)	(0.023)	(0.015)	(0.013)	(0.022)	(0.016)	(0.013)	(0.023)	(0.015)
Eldercare	-0.059**	-0.074*	-0.052*	-0.026*	-0.043*	-0.017	-0.032*	-0.052*	-0.025	-0.028*	-0.002	-0.035*
	(0.019)	(0.031)	(0.023)	(0.012)	(0.021)	(0.015)	(0.013)	(0.023)	(0.016)	(0.012)	(0.020)	(0.014)
LPM w/ résumé and job ad controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	990	273	717	2,609	841	1,768	2,262	652	1,610	2,726	792	1,934
Mean Callback Rate Continuously Employed	0.139	0.111	0.149	0.194	0.180	0.201	0.174	0.161	0.179	0.267	0.245	0.276
p-value$_{\text{Evs.U}}$	0.061	0.150	0.163	0.884	0.376	0.481	0.895	0.953	0.946	0.475	0.367	0.791
Clusters	355	97	258	910	295	615	820	241	579	952	275	677

Note: Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. All columns also include job ad controls for the month-year the job advertisement was posted, city, whether the job was full-time or not, and RA fixed effects. Columns (1), (4), (7), and (10) also includes a control for sex. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A Appendix: Sample Résumés

Experimental Arm 1: Continuously Employed

[First and Last Name]
[Email]
[Phone]
[Location]

Objective

[Objective]

Experience

[Job 3] | **[Company 3]**
[Month] [Year] to Present
[Job Description 3]

[Job 2] | **[Company 2]**
[Month] [Year] to [Month] [Year]
[Job Description 2]

[Job 1] | **[Company 1]**
[Month] [Year] to [Month] [Year]
[Job Description 1]

Education

[High School]
Class of [Grad Year]

Skills

[Skill 1]
[Skill 2]
[Skill 3]
[Skill 4]

Interests & Misc.

[Interest 1]
[Interest 2]

References

References are available upon request.

Experimental Arm 2: Eldercare

[First and Last Name]

[Email] | [Phone] | [Location]

Objective

[Objective]

Experience

[Job 2]
[Company 2]
[Month] [Year] – [Month] [Year]. Left to provide care to an elderly relative.
[Job Description 2]

[Job 1]
[Company 1]
[Month] [Year] – [Month] [Year]
[Job Description 1]

Education

[High School]
Class of [Grad Year]

Skills

[Skill 1]
[Skill 2]
[Skill 3]
[Skill 4]

Interests & Misc.

[Interest 1]
[Interest 2]

References

References are available upon request.

Experimental Arm 3: Unexplained

[First and Last Name]

[Email] | [Phone] | [Location]

Objective

[Objective]

Experience

[Job 2] – **[Company 2]**
[Month] [Year] – [Month] [Year]
[Job Description 2]

[Job 1] – **[Company 1]**
[Month] [Year] – [Month] [Year]
[Job Description 1]

Education

[High School] Class of [Grad Year]

Skills

[Skill 1]
[Skill 2]
[Skill 3]
[Skill 4]

Interests & Misc.

[Interest 1]
[Interest 2]

References

References are available upon request.

B Appendix: Additional Experimental Detail

B.1 Résumé and Cover Letter Creation

New sets of applications were created each month using the randomizer. This allowed for making changes to the months and years inside of the applicant's work histories to ensure that the unemployment spell was a constant length for the eldercare and unexplained applicants throughout the experiment. We also altered the high school graduation years as needed so the applicants are relatively the same age throughout the experiment.

B.2 High Schools

High schools were randomly assigned inside of triplets. Table B1 shows the high schools used in each city.

Table B1. High Schools

City	High School
Boston, MA	Bridgewater-Raynham Regional
	Malden High School
	Middleborough High School
Chicago, IL	Fenton High School
	Sycamore High School
	Elmwood Park High School
Denver, CO	Riverdale Ridge High School
	Northglenn High School
	Bruce Randolph School
Houston, TX	B F Terry High School
	Galena Park High School
	Cypress Springs High School
New York, NY	Robert H Goddard High School
	Lindenhurst Senior High School
	Jackson Memorial High School
Philadelphia, PA	Rancocas Valley Regional High School
	Sun Valley High School
	Christina High School
Phoenix, AZ	Glendale High School
	Barry Goldwater High School
	Ironwood High School
San Francisco, CA	James Logan High School
	San Leandro High School
	El Cerrito Senior High School
Seattle, WA	White River High School
	Emerald Ridge High School
	Mount Rainier High School
Washington, D.C.	Bowie High School
	Lewis High School
	Gov. Thomas Johnson High School

B.3 Phone Area Codes

Table B2 shows the area codes we used for each city.

Table B2. Phone Area Codes

Metro Area	Area Code	Year Created	Geographical Area
Boston, MA	857	2001	Greater Boston Area
Chicago, IL	872	2009	Chicago (Excluding Downtown)
Denver, CO	720	1998	Greater Denver Area
Houston, TX	832	1999	Greater Houston Area
New York, NY	929	2011	The Bronx, Queens, Brooklyn, Staten Island, Marble Hill
Philadelphia, PA	267	1999	Philadelphia and surrounding suburban area
Phoenix, AZ	623	1999	Greater Phoenix Area
San Francisco, CA	415	1947	San Francisco, Marin County
Seattle, WA	425	1997	Seattle Area, northern and eastern suburbs
Washington, D.C.	202	1947	Washington D.C.

B.4 Email Addresses

The email handles were created in a way to avoid sending any additional signal about the applicant because they focused on variations of the applicant's name plus numbers and special characters (underscore, periods, etc.) as needed to find a variation that was not already taken. Each name had its own email address. The callbacks tended to happen very fast given the occupations we focused on, and thus not allowing ourselves to apply to the same company (whether to the same job or a different job within the same company) within thirty days meant it was unlikely that callbacks would be allocated to the wrong applicant.

B.5 Skills and Interests

Applicants reported having skills in Microsoft Office programs or point-of-sale (POS) systems, depending on the occupation. The exact phrasing varied within the triplet to minimize detection and was randomly assigned to each applicant. They were also randomly assigned three buzzwords, one from each of the following sets: set 1 (dependable, reliable, flexible), set 2 (team player, self-starter, time management), and set 3 (active listening, interpersonal skills, communication). Our applicants were also given two interests based on random assignment within sets. Set 1 included baking, photography, and painting, while set 2 included recreational basketball, recreational softball, and hiking.

B.6 Applying to Jobs

After applying the job filters discussed in Section 3.2 like distance from the city, we check whether the job title sounds like it fits the occupational category of the applicant. Jobs within an occupation can have a range of different names; however, they are all similar in responsibilities. For example, sales associate jobs

could have the following titles: retail sales associate, sales lead, counter sales, cashier, etc. If the job title fits the occupational category, we then proceed to read the job description.

We verify that the job ad was posted by an employer and not a job agency. We avoid company names that include "staffing", "employment", etc. We also check that the employer is not used inside of our work histories, the required level of education is a high school diploma, and the job does not need more than three years of experience. Applications are also restricted to full-time and part-time jobs only (no temporary or seasonal work), with a prioritization of full-time positions.

We do not apply for the same job within a 30-day period to avoid sending multiple sets of applications to the same job. If the job is still open or re-posted after 30 days, we discourage reapplying to it to prevent detection of our experiment. However, if there are no other jobs available, we would apply using a new (and thus different) set of applications. Large companies may also have multiple positions available in multiple cities. Only one position can be applied to within a 30-day period to avoid detection. The positions were selected without any preference for a specific city.

After all these checks, we mock apply for the job. Some job advertisements include additional screening questions. We inspect these questions before applying to ensure that they can be answered. If they cannot, we do not apply. Some examples of questions that we can answer are contact information, highest level of education completed, and last job title and company worked for. Some examples of questions our applicants are unable to answer are short answer questions (for example, "What makes you a good candidate?"), salary expectations, and years of experience with a specific skill. There are also questions that we try to avoid, if possible, but in some cases they must be answered. If we cannot respond with "I do not want to answer", then we answer them as follows: gender, male/female depending on the applicant's name; disability, no; veteran status, no; race/ethnicity, white; AI to process your application, no. Our reason for avoiding some questions and following these rules for other questions is to avoid creating additional variation between our applications.

If a job meets all the criteria above, we record the job's information. The information recorded is the job position title, date of applying to the job, company who posted the ad, city, zip code (if applicable), full-time vs part-time, work arrangement (in-person, remote, or hybrid), if a cover letter can be sent to the ad, and the link to the job post. This data collection aims to reduce errors that result from both using the wrong job for the second or third applicant and applying for the same job twice. This was a purposeful quality control measure.

Once the data for the job are recorded, we apply for the job. We go through the application and answer all the questions. The résumés are then converted to PDF and attached to the application. The cover letters are also converted to PDF and attached to the application, if possible. If the cover letter could only be

added through a text box, we copy and paste the cover letter content directly into the text box.

The résumé triplets are submitted in two consecutive days. On the first day, jobs are found and the first résumé is sent. The second résumé is sent in the morning the following day, and the third is sent later that day, with 12 hours separating the submission of the second and third résumés. We are using this strategy in an attempt to submit all applications before the job advertisement closes and to minimize the chance of detection.

Each résumé within the triplet has a different naming convention. The naming conventions were "First-Last", "ResumeFirstLast", and "FirstLastResume". First and Last refer to the first name and last name of the applicants. The naming convention is randomly assigned. Cover letters follow the same naming convention as their corresponding résumés, with "CoverLetter" replacing "Resume" in the file name. We did this to minimize the chance of detection by employers.

C Appendix: Additional Tables

Table C1. Impact on Callback Rate Relative to Continuously Employed Conditional on All Applications Being Submitted to a Job Ad

	(1) Callback	(2) Callback
Unexplained Gap	-0.032***	-0.031***
	(0.007)	(0.007)
Eldercare	-0.032***	-0.032***
	(0.007)	(0.006)
LPM w/ résumé and job ad controls	Yes	No
Logit w/ résumé and job ad controls	No	Yes
Observations	8,091	8,091
Mean Callback Rate Continuously Employed	0.209	0.209
p-value$_{\text{Evs.U}}$	0.992	0.969
Clusters	2,697	2,697

Note: Since job ads could close before we are able to submit all three applications, we run this robustness check in which we restrict the sample to triplets where all three applications got submitted. Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Marginal effects are reported for column (2), computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. All columns include job ad controls for the month-year the job advertisement was posted, sex, city, occupation, whether the job was full-time or not, and RA fixed effects. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C2. Impact on Callback Rate Relative to Continuously Employed when Large Firms Only Have One Set of Applicants Included

	(1) Callback	(2) Callback	(3) Callback
Unexplained Gap	-0.029***	-0.031***	-0.028***
	(0.007)	(0.007)	(0.007)
Eldercare	-0.028***	-0.030***	-0.027***
	(0.007)	(0.007)	(0.007)
LPM w/ résumé and job ad controls	Yes	No	No
LPM w/ résumé controls and job ad fixed effects	No	Yes	No
Logit w/ résumé and job ad controls	No	No	Yes
Observations	7,880	7,410	7,880
Mean Callback Rate Continuously Employed	0.201	0.204	0.201
p-value$_{\text{Evs.U}}$	0.862	0.882	0.872
Clusters	2,795	2,470	2,795

Note: Since there are some large national firms that could post multiple jobs, we run this robustness check in which we only include one set of applicants (the first set submitted) for these companies. Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Marginal effects are reported for column (3), computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. Columns (1) and (3) include job ad controls for the month-year the job advertisement was posted, sex, city, occupation, whether the job was full-time or not, and RA fixed effects. Column (2) includes job ad fixed effects. Column (2) is restricted to triplets where all résumés are sent out before the job ad closes. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, $^{***}p < 0.001$

Table C3. Impact on Callback Rate Relative to Continuously Employed with RA Robustness Checks

	(1) Callback	(2) Callback	(3) Callback	(4) Callback
Unexplained Gap	-0.032***	-0.036***	-0.039***	-0.035***
	(0.007)	(0.007)	(0.007)	(0.007)
Eldercare	-0.032***	-0.035***	-0.038***	-0.035***
	(0.007)	(0.007)	(0.007)	(0.007)
LPM w/ job ad and RA fixed effects	Yes	No	No	No
LPM w/ résumé and job ad controls	No	Yes	No	No
LPM w/ résumé controls and job ad fixed effects	No	No	Yes	No
Logit w/ résumé and job ad controls	No	No	No	Yes
Observations	8,091	7,179	6,717	7,179
Mean Callback Rate Continuously Employed	0.209	0.211	0.215	0.211
p-value$_{\text{Evs.U}}$	0.998	0.957	0.975	0.961
Clusters	2,697	2,562	2,239	2,562

Note: Occasionally the RAs would cover for each other and thus some triplets did not have the same RA for all applicants. Column (1) contains job ad and RA fixed effects to account for this. Columns (2), (3), and (4) address this by restricting the sample to triplets where one RA sends out all three applications. Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Marginal effects are reported for column (4), computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. Columns (2) and (4) include job ad controls for the month-year the job advertisement was posted, sex, city, occupation, whether the job was full-time or not, and RA fixed effects. Columns (1) and (3) include job ad fixed effects. Columns (1) and (3) are also restricted to triplets where all résumés are sent out before the job ad closes. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C4. Impact on Callback Rate Relative to Continuously Employed when Large Firms Only Have One Set of Applicants Included by Sex

	Male			Female		
	(1) Callback	(2) Callback	(3) Callback	(4) Callback	(5) Callback	(6) Callback
Unexplained Gap	-0.036**	-0.046***	-0.033**	-0.025**	-0.024**	-0.025**
	(0.012)	(0.013)	(0.012)	(0.009)	(0.009)	(0.008)
Eldercare	-0.030**	-0.033**	-0.028*	-0.026**	-0.027***	-0.025**
	(0.012)	(0.012)	(0.011)	(0.008)	(0.008)	(0.008)
LPM w/ résumé and job ad controls	Yes	No	No	Yes	No	No
LPM w/ résumé controls and job ad fixed effects	No	Yes	No	No	Yes	No
Logit w/ résumé and job ad controls	No	No	Yes	No	No	Yes
Observations	2,347	2,202	2,347	5,533	5,208	5,533
Mean Callback Rate Continuously Employed	0.184	0.192	0.184	0.208	0.209	0.208
p-value$_{\text{Evs.U}}$	0.612	0.208	0.603	0.953	0.640	0.941
Clusters	835	734	835	1,960	1,736	1,960

Note: Since there are some large national firms that could post multiple jobs, we run this robustness check in which we only include one set of applicants (the first set submitted) for these companies. Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Marginal effects are reported for columns (3) and (6), computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. Columns (1), (3), (4) and (6) include job ad controls for the month-year the job advertisement was posted, city, occupation, whether the job was full-time or not, and RA fixed effects. Columns (2) and (5) include job ad fixed effects. Columns (2) and (5) are restricted to triplets where all résumés are sent out before the job ad closes. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C5. Impact on Callback Rate Relative to Continuously Employed with RA Robustness Checks by Sex

	Male				Female			
	(1) Callback	(2) Callback	(3) Callback	(4) Callback	(5) Callback	(6) Callback	(7) Callback	(8) Callback
Unexplained Gap	-0.044***	-0.040**	-0.049***	-0.038**	-0.027**	-0.033***	-0.033***	-0.033***
	(0.012)	(0.013)	(0.013)	(0.013)	(0.008)	(0.009)	(0.009)	(0.009)
Eldercare	-0.038***	-0.037**	-0.038**	-0.035**	-0.029***	-0.033***	-0.037***	-0.033***
	(0.011)	(0.012)	(0.012)	(0.012)	(0.008)	(0.009)	(0.009)	(0.009)
LPM w/ job ad and RA fixed effects	Yes	No	No	No	Yes	No	No	No
LPM w/ résumé and job ad controls	No	Yes	No	No	No	Yes	No	No
LPM w/ résumé controls and job ad fixed effects	No	No	Yes	No	No	No	Yes	No
Logit w/ résumé and job ad controls	No	No	No	Yes	No	No	No	Yes
Observations	2,400	2,161	2,019	2,161	5,691	5,018	4,698	5,018
Mean Callback Rate Continuously Employed	0.195	0.196	0.204	0.196	0.215	0.217	0.220	0.217
p-value_Evs.U	0.533	0.806	0.330	0.766	0.772	0.967	0.632	0.966
Clusters	800	773	673	773	1,897	1,789	1,566	1,789

Note: Occasionally the RAs would cover for each other and thus some triplets did not have the same RA for all three applicants. Columns (1) and (5) contain job ad and RA fixed effects to account for this. Columns (2), (3), (4), (6), (7), and (8) address this by restricting the sample to triplets where one RA sends out all three applications. Dependent variable is binary, and equals one if an applicant received a callback, and zero otherwise. Omitted group is continuously employed. Marginal effects are reported for columns (4) and (8), computed as a discrete change in the probability of the outcome variable, evaluating all other variables at their means. Standard errors are clustered at the triplet level. All columns include résumé controls for the order of being sent within the triplet, template used, name, and high school. Columns (2), (4), (6) and (8) include job ad controls for the month-year the job advertisement was posted, city, occupation, whether the job was full-time or not, and RA fixed effects. Columns (1), (3), (5) and (7) include job ad fixed effects. Columns (1), (3), (5) and (7) are also restricted to triplets where all résumés are sent out before the job ad closes. p-value$_{\text{Evs.U}}$ is the p-value for the test of significant differences between the eldercare and unexplained gap coefficients.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$