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ABSTRACT

We design and implement a large-scale field experiment – a resume correspondence study – to address a number of potential limitations of existing field experiments testing for age discrimination, which may bias their results. One limitation that may bias these studies towards finding discrimination is the practice of giving older and younger applicants similar experience in the job to which they are applying, to make them "otherwise comparable." The second limitation arises because greater unobserved differences in human capital investment of older applicants may bias existing field experiments against finding age discrimination. We also study ages closer to retirement than in past studies, and use a richer set of job profiles for older workers to test for differences associated with transitions to less demanding jobs ("bridge jobs") at older ages. Based on evidence from over 40,000 job applications, we find robust evidence of age discrimination in hiring against older women. But we find that there is considerably less evidence of age discrimination against men after correcting for the potential biases this study addresses.

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1. Introduction

Population aging in the United States and many advanced economies, coupled with the very low employment rate of seniors, implies slowing labor force growth relative to population, and a rising dependency ratio. This creates a public policy imperative to increase the employment of older individuals, typically pursued via reforms to public pension systems (e.g., Gruber and Wise, 2007). In addition, increased health and life expectancy implies a growing share of older individuals who plan to work longer, even if these plans are not always realized (e.g., van Solinge and Henken, 2010). Efforts to extend work lives – whether via public policy reforms to induce increased labor supply, or individual efforts to keep working at older ages – may be thwarted by age discrimination in labor markets. This study utilizes a large-scale field experiment to study whether age discrimination in hiring presents a significant barrier to extending the work lives of older individuals.

Age discrimination *in hiring* is especially important in thinking about lengthening work lives, for two reasons. First, a significant share of any increase in employment among seniors would be expected to come from new employment in part-time or shorter-term "partial retirement" or "bridge jobs," rather than continued employment of workers in their long-term career jobs (Cahill et al., 2006; Johnson et al., 2009). This path to retirement is likely driven in part by emerging health issues and other challenges as people age (Johnson, 2014).[1] Of workers age 50 who report leaving their employer by age 70, 23% cite poor health as a reason, and 58% report retirement as a reason (Johnson, 2014, Table 1). Perhaps reflecting these health reasons, 36% of those age 50 who leave their employer by age 70 report changing occupations, although a higher percentage (50) report moving to a different employer, suggesting that changing jobs but staying in the same occupation can help older workers realize their goals. And workers who change employers for reasons related to poor health report less physically demanding and less stressful work on their new jobs, as well as fewer hours and more flexible schedules (Johnson, 2014, Table 4). Age discrimination in hiring could interfere with these kinds of transitions to new jobs that let older workers extend their work lives. In contrast, it seems likely that only modest changes in employment of older individuals are possible if

[1] And workers may return to work after a period of retirement (e.g., Maestas, 2010).

difficulties in getting hired into new jobs limit older workers to staying in their long-term jobs a little longer.

Studying age discrimination in hiring is also potentially important because current policies to combat age discrimination may be ineffective at reducing or eliminating age discrimination in hiring. Although federal and state age discrimination laws have increased employment of protected workers, this effect has likely come through reduced terminations (Neumark and Stock, 1999; Adams, 2004). These laws are likely less effective at reducing discrimination in hiring because they rely on the legal process and hence on potential rewards to plaintiffs' attorneys. In hiring cases, it is difficult to identify a class of affected workers, inhibiting class action suits and thus substantially limiting awards. In addition, economic damages can be small in hiring cases because one employer's action may extend a worker's spell of unemployment only modestly. (Terminations, in contrast, can entail substantial lost earnings, health insurance benefits, and pension accruals.) And it could be worse: If age discrimination laws fail to reduce discrimination in hiring, but make it harder to terminate older workers, these laws could actually deter hiring of older workers (Bloch, 1994; Lahey, 2008a; Posner, 1995). Evidence on the effects of age discrimination laws on hiring is scant and somewhat mixed (Neumark and Button, 2014).

Existing field experiments generally – and nearly uniformly – point to substantial age discrimination in hiring (Bendick et al., 1997; Bendick et al., 1999; Riach and Rich, 2006, 2010; Lahey, 2008b). But this evidence is potentially flawed in ways that could bias estimates of age discrimination in existing studies towards either overstating or understating discrimination. Consequently, we designed and implemented a large-scale field experiment – a resume correspondence study – to address these potential limitations and sources of bias in existing field experiments testing for age discrimination, which may bias their results. One limitation that may bias these studies towards finding discrimination is the practice of giving older and younger applicants similar experience in the job to which they are applying, to make them "otherwise comparable." The second limitation arises because greater unobserved differences in human capital investment of older applicants may bias existing field experiments against finding age discrimination. We also study ages closer to retirement than in past studies, and use a richer set of job profiles for older workers to test for differences associated with transitions to less demanding jobs ("bridge jobs") at older ages.

Based on evidence from over 40,000 job applications, we find robust evidence of age discrimination in hiring against older women. But we find that the evidence for men is less robust, and that evidence of age discrimination against them may at least in part reflect the biases this study was designed to assess.

2. Past Research on Age Discrimination

Evidence on Age Discrimination using Observational Data

The research literature on age discrimination is less extensive than the research on discrimination by race and sex. One reason may be that the *prima facie* case for age discrimination is much weaker. For example, unlike the cases with blacks or women, older workers generally have higher earnings than do other workers. The other reason, almost certainly, is that aging does entail changing capacities, making it harder to interpret differences in outcomes in observational data as necessarily reflecting age discrimination.[2] Nonetheless, a number of types of evidence from observational data are at least consistent with the presence of age discrimination.

In the period prior to the passage of the Age Discrimination in Employment Act (ADEA), explicit age restrictions in hiring ads were documented. In five cities in states without anti-age discrimination statutes, nearly 60% of employers imposed upper age limits (usually between ages 45 and 55) on new hires (U.S. Department of Labor, 1965).

A persistent finding is that older workers have longer unemployment durations than many other age groups.[3] These longer durations need not reflect discrimination, however, and instead could arise from higher reservation wages of unemployed older workers, owing to a higher value of leisure, expectations of higher wage offers based on their most recent wage, etc.

There is substantial evidence of negative stereotypes regarding older workers, in both hypothetical

[2] However, it is hard to conclude that productivity during working ages clearly declines. Some research points to relatively steady skills as workers age (e.g., Meier and Kerr, 1976); some research points to substantial declines in some specific skills and performance-related behaviors, but improvements in other work-related characteristics, such as leadership (Posner, 1995). Jablonski et al. (1990), based on evidence of actual output or piece-rate pay in a narrow set of occupations, emphasize that in some occupations there is little or no evidence of productivity decline, and, more generally, that the variation within age groups swamps average variation across age groups. See also Warr (1993). Plant-level production functions for U.S. manufacturing workers also do not indicate productivity declines for those aged 55 and older (Hellerstein et al., 1999).

[3] For recent evidence, see http://www.bls.gov/cps/cpsaat31.pdf (viewed July 27, 2014).

scenarios and field research tying attitudes toward older workers to adverse labor market outcomes for them (Finkelstein et al., 1995; Kite et al., 2005), although more recent evidence suggests these negative stereotypes may have declined in importance (Gordon and Arvey, 2004). Researchers have also noted the common and widespread acceptance of ageist characterizations of workers, reflecting many of these stereotypes (e.g., McCann and Giles, 2002; Eglit, 2014).

The research on negative stereotypes about older workers is also significant because it may help to explain the nature of "age discrimination." In particular, these stereotypes are consistent with statistical discrimination. Alternatively, there may be simply animus towards older workers – perhaps not because of "dislike" of older workers, but because of negative attributes associated with them that lead to the same kind of disutility that drives the Becker (1971) model of discrimination.

There is also evidence that older workers "self-report" age discrimination. Such self-reports are potentially problematic, because they can reflect other adverse outcomes that survey respondents attribute to age discrimination. Thus, research studying the effects of these self-reports includes controls for job satisfaction and other measures of workers' perceptions of the workplace environment and fairness, and uses longitudinal data on workers whose self-report changes to control for individual heterogeneity in the propensity to report discrimination. Such studies indicate that workers who report experiencing age discrimination subsequently exhibit more separations, lower employment, slower wage growth, and reduced expectation of working past 62 or 65 (Johnson and Neumark, 1997; Adams, 2002).[4]

All told, this evidence based on observational data is generally consistent with the age discrimination. But it is hard to interpret it as providing decisive evidence.

Experimental Research on Age Discrimination in Hiring

In the discrimination literature generally, experimental audit or correspondence (AC) studies of hiring are viewed as the most reliable means of inferring discrimination (Hellerstein and Neumark, 2006; Fix and Struyk, 1993). Observational studies try to control for variables that might be associated with

[4] In the Lazear (1979) model of long-term incentive contracts (LTIC's), employers can have economic motivations to avoid hiring older workers. Whether the differential treatment of workers based on age implied by this model represents discrimination may be a semantic issue; however, it has been interpreted as such from a legal perspective (Issacharoff and Harris, 1997), as well as in the economics literature (e.g., Gottschalk, 1982; Cornwell et al., 1991).

productivity differences between groups, with questionable success. In contrast, AC studies create an artificial pool of job applicants, among which there are intended to be no average differences by group, so that differences in outcomes likely reflect discrimination. Audit studies use applicants coached to act alike, and capture the outcome of actual job offers, while correspondence studies create fake applicants (on paper, or electronically) and capture the outcome of "callbacks" for job interviews.

AC studies do have their critics (Heckman and Siegelman, 1993; Heckman, 1998). Audit studies have received particular criticism because of the potential for "experimenter effects," whereby the testers may affect the outcome of the experiment through their behavior, and because of the difficulty of controlling for all productivity-related differences that employers may observe. Correspondence studies avoid these criticisms. They also have the major advantage of being able to send out thousands of job applications, especially using the internet as more recent studies do. In contrast, even large-scale, expensive audit studies typically have sample sizes in the hundreds (Turner et al., 1991), because of the time costs involved in interviewing for jobs.[5] The principal downside of correspondence studies is probably that the researcher observes a callback for an interview (or some other positive response), rather than a job offer; we care most about job offers, although callbacks are a prerequisite.[6] There is, however, one key criticism that carries over from audit to correspondence studies, discussed in the next section.

AC methods have been applied to age discrimination; the main studies are Bendick et al. (1997, 1999), Lahey (2008b), and Riach and Rich (2006, 2010).[7] In general, applications of these methods to age

[5] In addition, current Institutional Review Board standards might deem audit studies unacceptable, because of the large time required of interviewers for what are ultimately false job applications. In contrast, researchers (including us) have successfully argued that the time spent reviewing electronic job applications is minimal, and hence that the benefits from the knowledge gained from these studies outweighs these concerns. One study – which is short on details – claims that on average recruiters spend 6 seconds looking at individual resumes (The Ladders, n.d.).

[6] Pager (2007) suggests we may find less evidence of discrimination in correspondence studies, because an employer may interview applicants from both groups tested, and only exert a biased decision at the job offer stage. That is not necessarily true, though, since employers may be sensitive to hiring in an apparently non-discriminatory fashion from those they interview for jobs. A related potential problem with inferring hiring discrimination from callbacks is that callback rates may vary for groups when average qualification levels in the population differ, even though the intended hiring rate for equally-qualified applicants is the same, because employers know there will more competition for the highly-qualified members of the less-qualified group (Bertrand and Mullainathan, 2004). However, while this scenario may be likely in studies of race discrimination, where minorities are disadvantaged relative to the rest of the population, it seems unlikely to be factor in studies of age discrimination, where neither young nor old applicants would be "surprisingly" qualified (or unqualified).

[7] See also Albert et al. (2011), although their study only covers ages 24, 28, and 38, and hence does not speak to

discrimination follow the paradigm used in studies of discrimination against other groups, such as blacks or women. Specifically, applicants are made identical (up to random variation) in all respects except age. There is an issue in applying this paradigm to age discrimination, because of age-related differences in experience. This, also, is discussed in the next section.

These studies – summarized in Table 1 – almost uniformly find evidence of age discrimination in hiring. For example, Bendick et al.'s correspondence study (1997) looks at 32 and 57 year-old applicants. Among applications in which at least one of the two applicants received a positive response, in 43% of cases only younger applicant received the positive response, versus 16.5% of cases in which the older applicant was favored, for a statistically significant difference of 26.5%. This difference is often referred to as "net discrimination," and ignores tests where both applicants have the same outcome.[8] Similar results are reported in the other studies covered in Table 1, although there are some differences in results reported, and, in one case, in the conclusion.[9] Note that the Riach and Rich and Bendick et al. papers are based on quite small numbers of applications, for correspondence studies.

Bendick et al. (1999) report results that capture more than just whether the callback was positive. In particular, they report the percentages of cases in which one paired tester received a more favorable response than the other paired tester with "favorable responses" defined to include: an interview, an opportunity to demonstrate skills, a job offer, or a job offer with higher compensation. In general, this echoes other features of his study that try to capture more of the richness of the hiring/recruiting process, which is of course more

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discrimination against older workers, in contrast to the other studies in which older workers are in their 50s or 60s. Similarly, in a recent study Baert et al. (2015) study 38, 44, and 50 year-olds; this paper also discusses a couple of other age discrimination studies.

[8] The analyses reported in this paper simply focus on differences in callback rates in the sample as a whole, as has become standard.

[9] Lahey (2008b) reports rounded estimates suggesting only a marginally significant result, but estimates provided by the author indicate that the difference is significant at the five-percent level. She reports the percentage of applications resulting in interviews, but not the percentage of tests with one or more positive responses (or equivalently, the distribution of responses based on whether only the older or only the young applicant received a call-back). Because of this, we can only calculate a range of net discrimination estimates. At one extreme, using Massachusetts as an example, assume that the results were generated by cases with both older and younger applicants offered interviews, or only younger applicants offered interviews. In that case, 5.3% of applications resulted in one or more positive responses, with 0% of the tests with positive responses favoring older applicants, and 28.3% (1.5/5.3) favoring younger applicants, for a 28.3% net discrimination estimate. At the other extreme, if there was no overlap of positive responses, then 9.1% of applications (5.3+3.8) resulted in at least one positive response, and the net discrimination rate is 16.5% (1.5/9.1). Similar calculations for Florida yield a range of 18.1 to 30.6%.

feasible in an audit study than a correspondence study. Measured this way, the percentage of tests with a more favorable response for younger applicants (age 32) was 42.2% for age 32, versus 1% for older applicants (age 57), for a statistically significant difference of 41.2%.

Finally, the only contrary evidence comes from one of three cases in Riach and Rich's (2010) correspondence study in England. Specifically, for female applicants for jobs as retail managers, there was statistically significant net discrimination against younger applicants (age 27 versus age 47) of 29.6% for retail manager jobs. Still the other two estimates in this paper provide statistically significant evidence of discrimination against older workers.

There are, however, two potentially important problems with this evidence, which this paper seeks to overcome. These problems, and the proposed solutions, are described in the next section.

3. Limitations of Experimental Evidence on Age Discrimination in Hiring

Experience of Older and Younger Applicants

One problem specific to using AC methods to study *age* discrimination is that the usual approach of making applicants identical (up to random variation) on all characteristics aside from the one in question is problematic. Clearly, a young applicant cannot have the experience of a long-employed older worker. The only option, then, is to give older and younger applicants the same low level of experience (commensurate with the young applicants' ages). However, this can make the older applicants in these studies look less qualified than the older applicants employers usually see, which could explain why older applicants in AC studies almost uniformly receive fewer job offers or callbacks. In other words, holding experience fixed may bias the evidence from AC studies of age and hiring towards evidence of age discrimination.

Researchers are aware of this problem. Bendick et al. (1997) had both older and younger applicants report 10 years of similar experience on their resumes. However, "[t]o account for older applicants' additional 25 years of living not covered by their 10 years of experience" (p. 31), they had the resumes for older applicants indicate that they had been out of the labor force raising children (for the female executive secretary applications), or working as a high school teacher (for the male or mixed applications).[10] However, experience in unrelated fields, or time out of the labor force, could negatively affect employers' assessments of older applicants, generating spurious evidence of discrimination.

Lahey (2008b) focuses on women, for whom time out of the labor force is less likely to be a negative signal than for men. She included only a 10-year job history, citing conversations with three human resources professionals who said 10-year histories were the "gold standard" for resumes, and would not convey a negative signal for older applicants. In addition, Lahey studies entry-level jobs, for which she suggests that "job-specific human capital should be less of a concern" (p. 34). Nonetheless, a lack of experience could be viewed as a negative.

In contrast, Riach and Rich (2006, 2010), who criticize this approach as using unrealistic resumes for older workers, give their applicants experience more commensurate with their age.[11] Interestingly, as noted earlier, one of three cases in one of these studies (Riach and Rich, 2010) reports evidence that does not point to discrimination against older workers, but rather the opposite. However, the results are based on quite small numbers of observations. Hence, between the small samples and mixed results we would argue that we do not have a firm understanding of how the evidence on age discrimination in hiring depends on how researcher treat the experience of older applicants.

Finally, in a recent study, Baert et al. (2015) also look at this question, which they label the "Difference in Post-Education Years" problem. Looking at 38, 44, and 50 year-olds, they give all applicants a job in the field to which they are applying for the same number of years prior to the application and immediately after graduating from school. But they otherwise construct three different resume types: one with inactivity in the "extra" years of older applicants, one with work in a different field, and one with work in the same field. Their evidence points to lower callback rates for older workers only in the first two cases

[10] Bendick et al. (1999) are vague, noting that their applicants indicate that they have several years of experience in an occupation related to the job to which they are applying, and that the additional years of experience an older applicant had accumulated were "ascribed" to a field unrelated to the position being sought, "such as military service of public school teaching" (p. 9).

[11] However, their resumes include only cursory descriptions of experience. In the 2006 paper, the resumes simply say that the person has worked as a server in restaurants since about age 20. In the 2010 paper, one resume lists three jobs held since age 17, rising to Senior Waiter, and the second has a paragraph description of the career since leaving school, again with rising responsibility.

of out-of-field employment or inactivity. This evidence is consistent with bias towards finding age discrimination when older resumes do not show greater continuous experience in the field. However, the narrow age range used in this study (38-50) calls into question whether its results should even be compared to the age discrimination literature. In addition, the study is based on a small number of tests (192 for each type of resume). Moreover, their evidence by age (ignoring the issue of the difference in post-education years) points to lower callback rates for 44 versus 38 year-olds and 50 versus 44 year-olds, but not 50 versus 38 year-olds (reflecting, in part, very different callback rates for 44 year-olds depending on whether their applications are paired with 38 or 50 year-olds). Thus, even the overarching age patterns in this study are unusual relative to the literature – although the age range is small and the upper age limit not very old.

We provide what we regard as more thorough and compelling evidence on the question of how experience relative to age – and whether it is commensurate with age – affects the outcomes of these studies. We conduct a much larger study, and we embed in the same experiment evidence from defining older applicants as having the same experience as younger applicants, and defining them as having experience commensurate with their age. Finally, we cover a much larger age range, and extend to what we view as a very policy-relevant upper age range – near traditional retirement ages.

The Role of Unobservables

Another problem plagues AC studies of discrimination along any dimension. AC studies create applicants who are identical in terms of variables observable to employers, and in a well-designed study – especially a correspondence study that avoids issues like experimenter effects – we can assume that the mean qualifications and characteristics presented to employers are identical across the two groups.

But even in the best case scenario of a well-designed correspondence study where we can assume no differences in the *means* of unobservables, Heckman (1998) and Heckman and Siegelman (1993) show that differences in the *variances* of the unobservables render the effect of discrimination unidentified, suggesting discrimination where there is none, and vice versa. This is not an obscure statistical argument; differences in the distributions of unobservable variables are a central element of models of statistical discrimination (Aigner and Cain, 1977). Moreover, although it has not been noted previously, this problem can be particularly important in studying age discrimination. In the human capital model, earnings become more dispersed as workers age (after the overtaking age), because workers invest differentially in human capital and these differences accumulate as workers age (Mincer, 1974). Given that this variation is unlikely to be conveyed on the resumes used in correspondence studies, it presumably generates a larger variance of unobservables for older versus younger applicants.

What is the potential implication? As Heckman (1998) shows, this difference in the variance of unobservables interacts with the level of quality chosen for the resumes in a correspondence study. For example, suppose the study uses relatively low-quality applicants, to avoid over-qualified applicants who do not get any offers or callbacks. Then employers will favor the *high* variance group, since given the low observed qualifications, the high variance group has a higher probability of having sufficiently high qualifications to meet the hiring standard. In this case, there is bias in the direction of favoring older workers in hiring, even if the resumes present similar quality and characteristics by age. Of course, the reverse implication holds if the study uses high-quality applicants, since then employers avoid the high variance group. Without knowing the quality of applicants employers actually receive, we do not know which situation actually holds, and hence we do not necessarily know the direction of bias. As one example, though, Bertrand and Mullainathan (2004, p. 995) claim that they tried to avoid over-qualified applicants who employers might not bother trying to hire.

If past age discrimination AC studies have used relatively lower-quality applicants, and the variance of the unobservable is higher for older workers, then – in contrast to the bias introduced by the "experience-commensurate-with-age" problem – these past age discrimination AC studies are biased *against* finding age discrimination. In that case, correcting for *both* potential sources of bias could, in principle, move the evidence in either direction. Moreover, correcting for only one of them (as in the studies that, for older applicants, use experience that is more commensurate with age) can increase the bias by eliminating one of two sources of bias that are in offsetting directions. The next section of the paper explains, in general terms, the approaches we take in this paper to correct for both sources of bias, and the following section then delves into the details of the experimental design.

4. Empirical Approaches to Eliminating Biases in Correspondence Studies of Age Discrimination

Using Experience Commensurate with Age

In arguing that using older applicants with the same experience as younger applicants can create a bias towards finding discrimination against older workers, we are taking a stand on what parameter we are trying to estimate. In our view, there are both policy and legal arguments that the *right* comparison – and hence the relevant parameter – is the difference in outcomes between younger applicants and older applicants who have experience commensurate with their age.

The simpler argument concerns the policy question, which in our view is whether older job applicants who are in some sense "typical" face difficulties in getting hired because of their age. For example, media and research reports exploring whether age discrimination explains the long unemployment durations faced by older workers during the Great Recession do not consider hypothetical older job applicants who have not worked much and hence have equal experience to younger applicants; rather, they focus on actual older job applicants who *do* have much more experience.[12] Similarly, Riach and Rich (2002) argued that "It makes more sense to acknowledge the heterogeneity and control for the differences to be normally expected between the age groups being tested. Any differential response by employers to such realistic human capital circumstances is of far more relevance to policy makers, than the artificial situation contrived by Bendick et al. (1999)" (p. F508). Moreover, as the evidence described later suggests, how very inexperienced older job applicants fare is arguably of less interest as a matter of public policy because there are relatively few older job applicants in this category.

Perhaps more important, our reading of age discrimination law and legal rulings suggests that evidence of age discrimination garnered from correspondence (or audit) studies using experience commensurate with age, rather than equal experience, is more consonant with legal standards for age discrimination. The ADEA makes it unlawful for employers to "fail or refuse to hire or to discharge any

[12] See, e.g., http://www.nytimes.com/2013/02/03/business/americans-closest-to-retirement-were-hardest-hit-by-recession.html?pagewanted=all&_r=0 (viewed March 5, 2013); http://economix.blogs.nytimes.com/2011/05/06/older-workers-without-jobs-face-longest-time-out-of-work/ (viewed March 5, 2013); http://www.nytimes.com/2009/04/13/us/13age.html?pagewanted=all (viewed March 5, 2013); Mulvey (2011); and AARP Public Policy Institute (n.d.).

individual or otherwise discriminate against any individual with respect to his compensation, terms, conditions, or privileges of employment, because of such individual's age."[13] There is no mention, not surprisingly, of comparisons at different levels of experience.

To consider what this means with regard to evidence from AC studies of hiring discrimination, it is useful to review how discrimination is established legally. The standards for establishing an age discrimination claim in a hiring case are fairly well-established, and a critical part of the standard, in a hiring case, is that the plaintiff was qualified for the job and the defendant did not hire the plaintiff, yet continued to seek applicants with the plaintiff's qualifications (*McDonnell Douglas v. Green*, 1973; 411 U.S. at 792-793, 1973; Player, 1982-1983). These standards help establish a prima facie case for discrimination. If it is met, then the burden of proof shifts to the employer "to articulate some legitimate, nondiscriminatory reason for the employer's rejection" (411 U.S. at 802), otherwise known as a "reasonable factor other than age" (RFOA).[14] If the defendant does this, then the plaintiff has the burden of presenting additional evidence that there was an illegal motivation for the decision (411 U.S. at 803-805).[15]

In light of these standards, establishing that a decision not to hire an older worker was "because of an individual's age," and hence illegal, would be much clearer in comparing a younger applicant to an older applicant with experience commensurate with their age, rather than to an older applicants with unusually low experience, which introduces another factor that could be construed as an RFOA. Suppose three applicants are denoted: Y_L (young, low experience), O_L (old, low experience), and O_H (old, high experience, i.e., experience commensurate with age). Suppose that O_L and O_H are both passed up in favor of hiring Y_L, while O_L and O_H meet the prima facie standard of being qualified for the job (and not hired). The defense has to offer a non-discriminatory reason for not hiring one of the other of the older applicants. It is clearly easier to argue that O_L was less qualified for the job than Y_L, appealing to the lack of work for a good part of O_L's career.

We suspect that this is the intent of the law: that the ADEA meant to protect typical older workers

[13] See http://www.eeoc.gov/laws/statutes/adea.cfm (viewed August 4, 2014).
[14] See http://www1.eeoc.gov//laws/regulations/adea_rfoa_qa_final_rule.cfm?renderforprint=1 (viewed August 4, 2014).
[15] This last step is typical in disparate treatment cases, but is not always necessary in disparate impact cases (411 U.S. at 805; Tinkham, 2010).

from age discrimination, and hence to use a standard that, in the hiring context, defines discrimination in hiring as adverse treatment of older applicants who are otherwise similar to younger applicants but have experience commensurate with their age.

One possible counter-argument (Tinkham, 2010) is that an older employee with experience commensurate to their age who has reached the same professional level as a younger employee is less qualified, because it took him or her longer to reach that level. Regardless, O_L would almost surely still be regarded as an inferior applicant and hence discrimination against O_L would be easier to defend, unless employers truly regarded them as entering the labor market at an older age and hence having risen as fast as Y_L. Moreover, for the low-skill jobs we study (as is typical of AC studies), it is hard to imagine that the speed-of-success consideration is important.

Based on this discussion, we explore differences in results comparing young applicants (Y_L) to older applicants with low experience (O_L) – as in other key age discrimination studies – as well as to older applicants with experience commensurate with their age (O_H). If low-experience resumes send a negative signal, we expect less evidence of discrimination in comparing outcomes between young applicants and older applicants with commensurate experience. And we have argued that the latter comparison is more relevant to assessing whether there is age discrimination in hiring – on both policy grounds and legal grounds.

Correcting for Biases from Differences in the Variance of Unobservables

Neumark (2012) develops a method to address the "Heckman critique" of AC studies. Here, we present a cursory discussion, beginning with the analytical framework for studying data from a conventional AC study, and then using it to outline the method. The discussion is based on applications from only two groups – older and younger applicants; although much of the study considers a wider variety of applicant types, our work on the Heckman critique focuses on this simple two-way classification of applicants.

Productivity is assumed to depend on two individual characteristics, $P(X') = P(X^I,X^{II})$. X^I denotes observed productivity measures included on the resumes. S denotes a dummy variable for age, with S = 1 for older ("senior") individuals and 0 for younger ones. The treatment of a worker by an employer, which depends on P and possibly S (if there is discrimination), is denoted $T(P(X'),S)$.

Discrimination is defined as

(1) $T(P(X')|S = 1) \neq T(P(X')|S = 0)$.

Assume that P(.,.) and T(P(.,.)) are additive, so

(2) $P(X') = \beta_I'X^I + X^{II}$

(3) $T(P(X'),S) = P + \gamma'S$.

γ' is an additional linear, additive term that is intended to reflect taste discrimination against older workers, equivalent to undervaluation of productivity). Two testers with either S = 1 or S = 0 apply for jobs. The productivity measures are held constant in the study at a level denoted X^I. Expected productivity for older and younger individuals are denoted P_S^* and P_Y^*; these are based on X^I, with X^{II} unobserved by firms. The goal of the usual AC study design is to set $P_S^* = P_Y^*$.

Given these observables, the T is observed for each tester, and each test yields an observation

(4) $T(P_S^*,1) - T(P_Y^*,0) = P_S^* + \gamma' - P_Y^*$.

If $P_S^* = P_Y^*$, then averaging across tests yields an estimate of γ', or we can estimate γ' from a regression of the outcome T on a constant and the age indicator S

(5) $T(S) = \alpha' + \gamma'S_i + \varepsilon_i$.

Denote by X_S^j and X_Y^j the values of X^I and X^{II} for older and younger applicants, j = I, II, with $X_S^I = X_Y^I$, and denote by X^{I*} the level at which X^I is "standardized" across applicants. Then

(6) $P_S^* = \beta_I'X^{I*} + E(X_S^{II})$

(7) $P_Y^* = \beta_I'X^{I*} + E(X_Y^{II})$.

In this case, each individual test provides an observation equal to

(8) $T(P_S^*,1) - T(P_Y^*,0) = \gamma' + E(X_S^{II}) - E(X_Y^{II})$.

Clearly the data identify γ' only if $E(X_S^{II}) = E(X_Y^{II})$. Thus, a key assumption in AC studies is that productivity-related factors not controlled for in the test have equal means for the two groups of applicants. As discussed earlier, this can be hard to guarantee in an audit study using actual applicants, especially because of experimenter effects. Correspondence studies make the assumption that $E(X_S^{II}) = E(X_Y^{II})$ more tenable, by avoiding face-to-face interviews that might convey mean differences on uncontrolled variables between the two groups of applicants. Of course it is still possible that $E(X_S^{II}) \neq E(X_Y^{II})$. For example, expected job tenure might be shorter for older workers. However, acting on such a belief would clearly constitute statistical discrimination, which is illegal.[16] Thus, the estimated parameter from equation (8) has to be interpreted as the sum of taste and statistical discrimination.[17]

To see why differences in the variance of unobservables matters, assume that a job offer or interview is given if a worker's perceived productivity exceeds a threshold c'. Defining the treatment T as a hire (T = 1) or not (T = 0), the hiring rules for older and younger applicants are

(9) $T(P(X^{I*},X_S^{II})|S = 1) = 1 \text{ if } \beta_I'X^{I*} + X_S^{II} + \gamma' > c'$

(9') $T(P(X^{I*},X_Y^{II})|S = 0) = 1 \text{ if } \beta_I'X^{I*} + X_Y^{II} > c'$.

Assume the unobservables X_S^{II} and X_Y^{II} are normally distributed, with zero means, and standard deviations σ_S^{II} and σ_Y^{II}. The hiring probabilities for older and younger applicants are

(10) $\Pr[T(P(X^{I*},X_S^{II})|S = 1) = 1] = \Phi[(\beta_I'X^{I*} + \gamma' - c')/\sigma_S^{II}]$

(10') $\Pr[T(P(X^{I*},X_Y^{II})|S = 0) = 1] = \Phi[(\beta_I'X^{I*} - c')/\sigma_Y^{II}]$,

where Φ denotes the standard normal distribution function.

As equations (10) and (10') show, even if $\gamma' = 0$, so there is no discrimination, these two expressions need not be equal because σ_S^{II} and σ_Y^{II}, the standard deviations of X_S^{II} and X_Y^{II}, can be unequal. More generally, without knowledge or some restriction on σ_S^{II} and σ_Y^{II}, γ' is unidentified, which is the basis for the Heckman/Siegelman claim that AC studies can be uninformative about discrimination.

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[16] EEOC regulations state: "An employer may not base hiring decisions on stereotypes and assumptions about a person's race, color, religion, sex (including pregnancy), national origin, age (40 or older), disability or genetic information." (See http://www1.eeoc.gov//laws/practices/index.cfm?renderforprint=1, viewed September 27, 2015.)

[17] Some AC studies try to distinguish between these hypotheses, by adding information to resumes and testing whether differences in callback or job offer rates between groups are diminished (see Charles and Guryan, 2013); these studies suggest that evidence of diminished differences imply that employers must have been statistically discriminating with respect to the additional information. But we do not know, ex ante, on what characteristics employers might be statistically discriminating, in which case a null finding that adding information does not change offer or callback rates is uninformative. Also, a reduction in the difference between offer or callback rates from adding information to the resumes does not necessarily imply statistical discrimination. To take an extreme case, suppose we compare results using resumes with no information (i.e., only the group identifier), and with other typical information (like job histories), and suppose that the callback or offer rate difference between the groups diminishes. This does not imply that, in the real world, members of the disadvantaged group suffer from statistical discrimination, because hiring on the basis of resumes with no information does not actually occur. Rather, we would need to know what information is typically *not* provided in the job application process, on the basis of which employers statistically discriminate, and examine the effect of adding that information.

To make explicit the point about bias made earlier, if X^{I*} is standardized at a low level, then $\beta_I'X^{I*} < c'$. In this case, a larger variance for older workers, $\sigma_S^{II} > \sigma_Y^{II}$, implies that we can find $\Phi[(\beta_I'X^{I*} + \gamma' - c')/\sigma_S^{II}] > \Phi[(\beta_I'X^{I*} - c')/\sigma_Y^{II}]$ even when $\gamma' = 0$. That is, there is a bias towards spurious evidence of discrimination *in favor* of older workers, implying that correcting for this source of bias can lead to stronger evidence of discrimination *against* older workers.

As shown in Neumark (2012), with a particular type of data from a correspondence study, conditional on an identifying assumption, γ' can be identified. The intuition is that a higher variance for one group implies a smaller effect of observed characteristics on the probability that applicants from that group meet the hiring standard. Thus, information on how variation in observable qualifications is related to employment outcomes can be informative about the relative variance of the unobservables, and this, in turn, can identify the effect of discrimination. Based on this idea, the identification problem is solved by assuming that there is variation in some applicant characteristics in the study that affect productivity and that have equal effects across groups. The typical AC study does *not* include such characteristics because applicants are designed to be homogeneous. But if the applicants are made heterogeneous, this method can be used.

Formally, equations (10) and (10') imply an age difference in hiring of

$$(11) \quad \Phi[(\beta_I'X^{I*} + \gamma' - c')/\sigma_S^{II}] - \Phi[(\beta_I'X^{I*} - c')/\sigma_Y^{II}].$$

A standard probit identifies coefficients only relative to the standard deviation of the unobservable, so we normalize the variance of the unobservable to one. In this case, impose the normalization for young applicants ($\sigma_Y^{II} = 1$). The variance of the unobservable for older applicants is then replaced by its variance relative to the variance for younger applicants, denoted $\sigma_{S/Y}^{II}$. The normalization is equivalent to defining all of the coefficients in equation (11) as their ratios relative to σ_Y^{II}, denoted by dropping the prime subscripts, so that the equation becomes

$$(11') \quad \Phi[(\beta_I X^{I*} + \gamma - c)/\sigma_{S/Y}^{II}] - \Phi[\beta_I X^{I*} - c].$$

We cannot tell whether the intercepts of the two probits in equation (11') – and hence the hiring probabilities – differ because $\gamma \neq 0$ or because $\sigma_{S/Y}^{II} \neq 1$. But if there is variation in the level of qualifications used as controls (X^{I*}), and these qualifications affect hiring outcomes, then we can identify $\beta_I/\sigma_{S/Y}^{II}$ and β_I in equation (11'), and the ratio of these two estimates provides an estimate of $\sigma_{S/Y}^{II}$, and identification of $\sigma_{S/Y}^{II}$ implies identification of γ. The critical assumption to identify $\sigma_{S/Y}^{II}$ and hence γ is that β_I is equal for young and old applicants. Otherwise, the ratio of the two coefficients of X^{I*} for young and old applicants does not identify $\sigma_{S/Y}^{II}$. One can simply assume this, but when there are data on multiple productivity-related characteristics (and this can be built into the study design) this assumption can be tested as the overidentifying restriction that the ratios of coefficients on any variable measuring qualifications of older and younger applicants are equal (to the same inverse of the ratio of the standard deviations of the unobservable).

$\beta_I/\sigma_{S/Y}^{II}$ and β_I can be estimated using a heteroscedastic probit model (Williams, 2009). Similar to equation (5), letting i denote applicants and j firms, there is a latent variable for perceived productivity relative to the threshold, assumed to be generated by

(12) $T(P_{ij}^{*}) = -c + \beta_I X_{ij}^{I*} + \gamma S_i + \varepsilon_{ij}$.

As is standard, it is assumed that $E(\varepsilon_{ij}) = 0$. But the variance is assumed to follow

(13) $Var(\varepsilon_{ij}) = [\exp(\mu + \omega S_i)]^2$.

This model can be estimated via maximum likelihood. The normalization $\mu = 0$ can be imposed, given that there is an arbitrary normalization of the scale of the variance of one group (in this case the young, with $S_i = 0$). Then the estimate of $\exp(\omega)$ is exactly the estimate of $\sigma_{S/Y}^{II}$.

The assumption that β_I is the same for young and old applicants identifies γ. Observations on young applicants identify $-c$ and β_I, and observations on old applicants identify $(-c + \gamma)/\exp(\omega)$ and $\beta_I/\exp(\omega)$. The ratio of $\beta_I/\{\beta_I/\exp(\omega)\}$ identifies $\exp(\omega)$, which, from equation (13), is the ratio of the standard deviation of the unobservable for old relative to young applicants, identified from the ratio of the effect of X^{I*} on old applicants relative to young applicants. With the estimate of $\exp(\omega)$, along with the estimate of c identified from young applicants, the expression $(-c + \gamma)/\exp(\omega)$ identified from old applicants identifies γ as well.

The key to being able to use this method is to design job applications with more than one level of qualifications. And if there are multiple measures of these qualifications, then the overidentification test can be used. Thus, in the experimental design described later, we explain how we generate applicants of different skill levels for each job for which we apply.

5. The Experimental Design

The standard procedures for correspondence studies are well established. There are three key steps: creation of data on artificial job applicants; collection of data on hiring-related outcomes; and statistical analysis. The usual statistical analysis without quality variation in resumes is straightforward, and the extension to consider the Heckman critique closely follows what was described in the previous section. The creation and collection of data, which includes both the design of the resumes and applying for jobs, is of course central to the credibility and quality of the results from the experiment. This section describes these aspects of the experimental design in considerable detail.

Creating Resumes

The resumes are the central element in the research project, since they constitute the "observations" in the data. Three goals drive the design of the resumes. The first is to make choices (about target ages, for example), that enable us to answer the most interesting questions. The second is to make the resumes as realistic as possible, so that our artificial job applicants have the best chance of mirroring actual applicants to jobs, and hence the results are most likely to be reflective of the experiences of actual job applicants. We do this by grounding resume design decisions in empirically observable information, to the greatest extent possible. And the third is to generate valid comparisons of older and younger applicants by, again, using an empirically grounded approach to mimic actual resumes of older and younger workers.

<u>Ages</u>

We create resumes for older applicants chosen from two different age ranges. One set is assigned ages 64, 65, or 66. These are older ages than used in past studies (Table 1). From a public policy perspective, however, we are interested in people in the age range in which they are eligible for Social Security benefits, in part because it is in this age range that retirement really accelerates (in part because of these benefits), and because reforms aimed at extending work lives naturally focus on those currently eligible for benefits. There is, for example, no talk of *lowering* the Full Retirement Age (FRA) in the future, but there is talk of raising it (Business Roundtable, 2013). To better touch base with the existing literature, and to explore differences as workers age, we also use middle-aged job applicants (aged 49, 50, or 51). These ages are also of interest because an inability to find a job at these ages because of age discrimination can be costly since Social Security benefits (and Medicare, for those aged 65 and over) are not available. Finally, our younger applicants are aged 29, 30, or 31, in line with past studies. These are ages at which workers are relatively young, but should have begun to develop some stability in their careers and hence to have built up a resume identifying them as plausible and desirable applicants for the jobs to which they apply.

Bridge resumes

We also create variants of our resumes for the middle-aged and older workers that differ with respect to whether these older workers have made or are making a transition to a lower-skill "bridge" job. For the middle-aged applicants, these bridge resumes always show workers rising to higher-level jobs in the same occupation before their current job application. We make the same types of resumes for the older applicants as well, but also add a second type of bridge job resume in which applicants had shifted to a bridge job around 8-10 years earlier. We do not construct the latter resumes for the middle-aged applicants because they are much less likely to have made such a transition in their early- to mid-40s. In all cases, the bridge resumes are created only for the high-experience resumes – the only ones that can exhibit the rising level of jobs throughout the career and the possible downward career shift.

We have to introduce additional notation for our resumes for middle-aged and older workers. For middle-aged workers, the resumes are distinguished by both experience (L or H) and, for the high-experience resumes whether or not it is a bridge resume, so we use the notation {M_L, M_{HB}, and M_{HNB}} for the three middle-aged resumes (with B and NB denoting bridge and non-bridge). The older resumes are denoted {O_L, O_{HB}^E, O_{HB}^L, and O_{HNB}}; the E and L superscripts indicate whether the transition to the bridge job occurs early (i.e., 8-10 years before the current application) or late (contemporaneously with the current application).

Occupations

Given the constraints imposed by a correspondence study, we targeted jobs for which there are many job ads on the internet (we use a particular job-listing website) and jobs that are fairly low skill, so that electronic responses to these ads, providing resumes, can realistically be expected to generate requests for job interviews. Not surprisingly, we therefore end up with some jobs that overlap those used in other studies.

To some extent, we targeted jobs in which there were some low-tenure older workers (which is not much of a constraint since low-skill jobs tend to have high turnover) as well as low-tenure younger workers. In doing so we tried to balance two conflicting issues. On the one hand, we wanted the resumes to be realistic, avoiding jobs for which it would be very unusual for an older worker to apply. On the other hand, very low representation of low-tenure older workers could reflect age discrimination. With age, as opposed to other demographic characteristics, our view was that the former issue was predominant.

To get information on "new hires," we used data from the 2008 and 2012 Current Population Survey (CPS) tenure supplements to identify workers with fewer than five years of tenure.[18] We computed, separately for men and women, the shares of new hires in the age ranges 28-32 and 62-70,[19] relative to all new hires in each occupation. Tables 2 and 3 present, for the 100 largest occupations (by employment), the proportion of the young and old age groups indicated as a share of all new hires in the occupation, for men and women. We have highlighted in boldface the occupations we use for this study. Lower-tenure older men are quite common for retail salespersons, cashiers, janitors and building cleaners, and security guards. These occupations also have sizable, but somewhat smaller, shares of low-tenure younger men, implying that it would not be odd for an employer looking to fill these jobs to receive applications from both older and younger men. Also, these four occupations typically do not require a significant amount of skills, training, or experience, and are likely also accessible for older workers as partial retirement or bridge jobs. As shown in Table 3, for women we choose some occupations that overlap those for men (retail salespersons and cashiers), and some that are different (secretaries and administrative assistants, office clerks, receptionists and information clerks, and file clerks).

Employer job advertisements are not categorized the same way as the Census Bureau classifies occupations, as employers often lump sets of these occupations together (like administrative assistant and secretary). We grouped the highlighted occupations from Tables 2 and 3 into four larger groupings of jobs, for which we used common resumes: retail sales (corresponding to retail salespersons and cashiers in the

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[18] These are the Current Population Survey Displaced Worker, Employee Tenure, and Occupational Mobility Supplement Files (see http://www.nber.org/cps/cpsjan12.pdf, viewed August 18, 2014). We avoided using the 2009 and 2010 CPS tenure supplements because of the Great Recession. The supplements are not available for 2011 or 2013.

[19] These ranges are somewhat larger than the age ranges for our resumes (29-31, 64-66), to increase the sample size.

Census occupational classification); administrative assistant (secretaries and administrative assistants, receptionists and information clerks, office clerks (general), and file clerks); janitors; and security guards (security guards and gaming surveillance officers). These groupings were based on three criteria: how different jobs related to these occupations were in the resumes posted on the web that we studied; how different they were when employers looked to hire, based on job ads; and how many job postings were there for these occupations. While the separate occupations may require slightly different skills and experience, the core requirements and skills within these jobs are the same, allowing one resume to be used to apply to a larger number of occupations. This has the added benefit of allowing us to avoid having to parse job advertisements that are typically not written to fit into a Census occupation code niche, but rather fit broader jobs that entail similar skills. Since the representation of people in these jobs and occupations differs by sex, we only use male applicants for security guard and janitor jobs, and only female applicants for administrative assistant jobs. Sales jobs are commonly held by both sexes, so for these jobs we use both male and female applicants.

Our choices of jobs often overlap with past AC studies of age discrimination. One advantage of using similar jobs is that differences in results are more likely to be due to methods than to differences in the jobs studied. Lahey's (2008b) study of women focuses on female-dominated jobs (like cashiers, secretaries, and home health care). Riach and Rich (2010) studied waiters/waitresses and retail jobs.[20]

Figure 1 reports histograms, for all occupations with non-empty cells, for the share of hiring in each age group relative to hires in the occupation (by sex). The figures also show the value of this share for the occupations we use. For men, all of the occupations we use are fairly central in the distribution, although security guards tend to have more older hires, and janitors more younger hires. For women the shares are also in the mid-range of the distribution, although our occupations exhibit relatively more hiring of older women and less hiring of younger women, suggesting that it is possible our results for women could be biased against finding evidence of age discrimination.[21] Finally, we note that these are fairly low wage jobs,

[20] The Bendick et al. studies (1997, 1999) use a wider variety of jobs.

[21] Yet, as described later, our strongest evidence point to age discrimination against older women.

paying about 15-20% less than the median wage across all occupations, with the exception of administrative jobs, which pay a bit above the median; see Appendix Table A1.

Cities

Because we apply for jobs in specific cities using our job-listing site, we needed to narrow the set of cities used. Some past studies for the United States used a very small number of cities. Lahey (2008b) uses Boston, MA, and St. Petersburg, FL, while Bendick et al.'s (1999) audit study is based on applications to jobs only in the Washington, DC area. Other studies use a much broader geographic scope. Our goal in choosing cities was two-fold. First, we wanted to include a large number of cities to help ensure that results were not driven by city-specific idiosyncrasies. Second, we were interested in obtaining potentially interesting comparisons across cities, although because many steps of the study entail a good deal of work for each city covered, the number of cities was limited to 12. In particular, we focused on cities with different age demographics, and cities with different state age discrimination statutes, to see whether either of these dimensions is associated with different relative outcomes for younger and older applicants.[22]

The differences in age discrimination statutes are based on research reported in Neumark and Song (2013), which also indicated that the two key features of these laws that appear most important for employment and retirement are larger damages for age discrimination claims, and whether the laws apply to smaller firms than those covered by the ADEA (which covers firms with 20 or more employees). Thus, we chose cities spread across states with neither type of age discrimination law (so the ADEA prevails), and with one type of law or the other. We also chose some cities with a fairly old population more reflective of the age structure towards which the U.S. population is evolving, as well as contrasting cities with younger populations, based on American Community Survey (ACS) data.

The cities selected are displayed Table 4. Down the rows, the table groups cities from higher to lower percentages of the population aged 62 and over. As the first number reported in parentheses after each city indicates, this percentage ranges considerably, from a low of 11.6% in Salt Lake City to a high of 34.7%

[22] In contrast, the geographic breakdowns by Census region in Bendick et al. (1997) are likely too broad to be tied to either of these, and there is no variation in laws across the cities in the Riach and Rich studies (nor are any contrasts drawn based on demographics).

in Sarasota. The "older" and "much older" cities are distinctly above the national average of 16.3% of the population aged 62 and over, and the "younger" cities are distinctly below. The table also breaks these cities into whether the state law allows larger damages under its age discrimination law. Of course, we might not learn much that is reliable from 12 cities (in 11 states).

Job histories

Creating realistic resumes required a good understanding of how applicants for the jobs we target actually portray their experience. There are a number of general issues, such as what type of information is conveyed, what kinds of skill differences can be used to generate higher- and lower-quality applicants, etc. And there are specific questions about differences between younger and older applicants, including the issue of experience that is one central concern of this study.

To obtain this information, we downloaded publicly available resumes on a popular national job-hunting website. This website has massive numbers of resumes.[23] The website allows some tailoring of resume searches. We were able to search in the specific cities listed in Table 4, and to search for resumes looking for the jobs we chose to target. To select resumes of older applicants, we also selected those whose high school or college graduation dates would likely imply that they were age 50 or older. (Resumes typically do not list age.) Finally, we selected resumes with more than five years of work experience, to focus on resumes of older applicants who were not new labor market entrants.[24] While this search may not yield a representative sample of the universe of resumes of older applicants in the jobs and cities we target, it does yield a large number of resumes in these cities and for these jobs. We downloaded resumes, and then input relevant resume information into a database, including work experience, work-related skills, education, approximate age, gender, and information on the pattern of work experience reported on the resume.[25]

[23] For example, on August 13, 2014, searching for our jobs/occupations in Los Angeles yielded 72,835 sales associate resumes, 1,809 janitor resumes, 57,660 administrative assistant resumes, and 8,222 security guard resumes, and in New York City yielded 150,043 sales associate resumes, 2,418 janitor resumes, 121,394 administrative assistant resumes, and 22,275 security guard resumes. The oldest resumes in terms of date posted were from late 2011. The resumes we studied to design our resumes dated from August 2012. We did not use commercial websites for which terms-of-use agreements precluded using them for our study.

[24] The website also permits a restriction to resumes with more than 10 years of experience, but for the smaller cities and occupations, the weaker restriction was useful to obtain more resumes.

[25] Prior to creating any data based on the resumes we strip out the personal identifiers to protect the confidentiality of

The main objective from using the resume database was to assist in the construction of the resumes. Our first step in this process was to pool job titles and descriptions from the actual resumes to create a set of entries to draw from for the work history sections of our fictitious resumes. We made minor changes to job descriptions before we used them in our fictitious resumes, such as changing phrasing, grammar, specific job details (like the number of supervisees), or the order in which job responsibilities are listed. The same process was used to create entries for the "skills" section of the high-skilled resumes – discussed below.

For the construction of the non-bridge resumes, we combined these job descriptions using the resume characteristic randomizer program created by Lahey and Beasley (2009). The program randomized the combination of job titles and descriptions, and job tenures. The program runs backward from the most current job to the beginning of the potential job history (1970). We had to build in a probability of a job ending, and experimented with the randomizer to choose a probability that appeared to create job histories similar to the resumes we downloaded, in terms of number of jobs held and average tenure on a job; this iterative process led us to choose a 15% annual probability that the program will end the current job and move on to the next randomly assigned job.

We used the resume randomizer to produce a large number of job histories, and then selected a smaller set that looked the most realistic based on the resumes found on the job-hunting website. In particular, we dropped those that had very high levels of turnover, unusual sequences of jobs (such as repeatedly switching between a manager and a cashier, etc.), or long strings of employment in other occupations (e.g., spent 20 of the 40 years as a real estate agent). From this sample of acceptable histories, we created three job histories for each type of job and city. All job histories contained information going back to 1970, so to create the job histories of younger applicants, as well as older applicants reporting low experience, the job histories were truncated at the appropriate year. This way the most recent parts of the job histories (roughly 2000-2014) look very similar across any of our resumes distinguished by either age or experience.

We modified our process to create bridge resumes. Using information from job histories on real

the job applicants who posted the resumes.

resumes, we tried to match the patterns that workers exhibited. Two patterns stood out: a defined profile of responsibility; and longer job tenure on the higher-responsibility jobs. More specifically, these resumes showed a progression of jobs from low-level to high-level jobs. After progressing to increasingly more high-level positions that lasted longer, these individuals would peak at a high-responsibility job. In some of the resumes, they would then make a clear and pronounced downshift towards low-level jobs, which likely parallels what the literature refers to as bridge jobs (Cahill et al., 2006; Johnson et al., 2009). Workers would remain in bridge jobs until retiring.

To approximate these job profiles over time, we used jobs from our bank of actual resumes. Jobs were coded according to their level of responsibility. Entry level, low-skill jobs were coded at 1, while the most high-skill, high-level jobs were coded as a 5. The coding of jobs can be seen in Appendix Table A2. In retail sales, the lowest responsibility job is a cashier or sales associate. Individuals work their way through various levels of store management before peaking as a store manager. In security, workers start out as entry-level security guards. They will peak at directors of security; note that for security we do not really see mid-level jobs and therefore the career profiles go from jobs coded 1-2 to jobs coded as 5. For administrative assistants, workers start as a receptionist before working their way to a peak job as an office manager. Janitor resumes did not exhibit the same pattern of peaking and bridging that was found in other occupations, so we did not create bridge resumes for janitors.

To create a bridge resume, we arranged jobs so each job history exhibited the desired peaking behavior. All jobs held by these workers were within the same occupation. Each new job was the same level or higher. After peaking at the highest available job, workers would continue at jobs at that level until they downshifted to a bridge job. There were two types of bridge resumes: either with this downshift occurring 8-10 years prior (for older applicants only), or currently in progress with the bridge job being the job for which the person is applying. These bridge jobs are the same jobs that are used in the creation of the non-bridge resumes (O_{HNB} and O_L). Appendix Figures A1-A2 provides a visual representation of the "profiles" of these codes for the different resumes we created. These figures show how the level of responsibility evolves differently in the bridge and non-bridge resumes we use.

On the real resumes tenure in these high-responsibility jobs is longer than tenure on low-skill jobs. To adjust for this we used a lower annual transition probability (7.5%) to generate longer job tenures, so that on average these workers will stay at these jobs twice as long as they do at the low-skill jobs.[26] The tenure at each job was created using the randomizer code described earlier and then added to the resumes. After the worker downshifted to a low-skill bridge job, they had the same transition probability as other workers in our fictitious sample for every job subsequently held. This was done so bridge jobs appear identical to the jobs on the other resumes. The result is that all O_{HB}^{E} resumes will have very similar job histories to the O_{HNB} and O_{L} resumes for their last 8-10 years.

We added employer names and addresses manually to each job on our final job histories. We ensured that the job title and description was realistic for the employer. In addition, we used employers that were active at the time and in the region listed, relying mainly on the actual resumes, supplemented by additional research on chains. In some cases, we added large public or private institutions known to be open in a particular period as employers. Employer names were added randomly if they were valid for the job.

These steps complete the construction of job histories used in the resumes. The remaining steps, described below, concern other information that we needed to add to complete the resumes. All other resume information was added to the resumes using Visual Basic for Applications (VBA) programs that we created. Creating our own code allowed us to randomly add and track several resume characteristics. These included adding school names and addresses to the education information, skill information to create high-skilled resumes, current employment status, and then the more specific information that completes the resumes, including applicant name, specific ages,[27] and residential address, as well as phone numbers and email address so that employers can contact the applicants. Our VBA programs also grouped our completed resumes into triplets for us, created application scripts, saved our resumes with file names reflecting the

[26] With a constant hazard, the distribution of tenure is exponential, with mean equal to the inverse of the hazard. We also use this lower transition probability for the earlier, lower-responsibility jobs for the bridge resumes, to distinguish those more likely to progress to a higher-responsibility job at the same employer.

[27] To reduce the number of job histories, we do not change the job history based on these small variations in age within our three-year age ranges; we only change age via the high school graduation year. This should have no bearing on our results for differences across the three broad age groups, which is our focus. Also, it likely to be undetected because most resumes do not go quite all the way back to the likely school leaving age.

names on the resumes, and organized all these files an intuitive folder structure.

In addition to using the database of resumes to construct our resumes, we also wanted to use it to characterize real resumes more systematically, including documenting the age distribution of those looking for jobs, as well as features of resumes for older workers – with a particular focus on the amount of experience listed and trajectory of jobs held for those now applying to the jobs we were targeting. To this end, in addition to our resume construction efforts, we drew a systematic sample of resumes from the website from which to assemble this information. We first searched for resumes with more than two years of work experience, in the four occupations and 12 cities used in our study, and in three experience groups (3-5 years, 6-10 years, or 10+ years). In each group we extracted the greater of all resumes listed or 1,000 resumes, for a total of 25,460 resumes.[28] We then wrote code to extract information from the resume text.

Age is calculated based on the listed high school graduation year. Information on high school attendance is reported on 81% of the collected resumes, and of these, 68% include high school graduation year, reducing the sample to 14,316 resumes (56% of the total). We also determined the sex of applicants based on matching names to SSA data; our method assigns gender to 82% of individuals, reducing the sample to 11,751 individuals.[29] We calculate experience based on job history information, as the number of years worked. If there are multiple jobs held at the same time, experience is not double-counted.[30]

One finding was that – consistent with the CPS tenure supplement data showing some representation of low-tenure, older workers in the jobs we study – there are many older workers looking for jobs in these occupations. Figure 2 displays the age distribution of resumes in each of the four jobs we

[28] Resumes may be posted in multiple occupation categories. Based on name, age, and work experience, only 6% of the resumes with available age information repeat in the sample. Omitting these resumes does not change the descriptive results provided below.

[29] We use data from a 100% sample of Social Security card applications for U.S. births. In each year the Social Security Administration (SSA) records the number of males and females born with a name and reports frequency counts of those names by sex, as long as the name is at least two characters long with a frequency of at least five. We match using first name to the SSA data in an individual's birth year. If a name applies to both males and females, we assign the majority gender as long as at least 90% of children born with that name have the same gender.

[30] Computed experience from the resumes corresponded to the experience "bins" used on the website. Average computed experience was 5.4 for the 3-5 year group, 9.2 years for the 6-10 group, and 17.6 years for the 10+ group. Moreover, 34%, 40%, and 26% of cases had computed experience in the corresponding range. We would not expect an exact correspondence since the experience bins are selected by job posters rather than constructed from resumes, and hence may reflect other factors (like including experience only in the specific job for which the person is seeking work).

study, although note that because we were more likely to cut off the number of resumes extracted at 1,000 for lower experience cells, there is a bias towards older resumes in these histograms.[31] Nonetheless, the presence of older resumes on the website suggests that older workers do use on-line resources to apply for jobs. As additional evidence that job search methods do not differ sharply between older and younger job searchers, we examined data from the monthly CPS files for 2014 on job search methods among the unemployed. As reported in Appendix Table A3, the distributions of job-search methods are fairly similar across these age groups.

A second clear finding is that a large share of resumes of older applicants list job experience that is commensurate with their age, including jobs going all the way back to the 1970s and even the 1960s for those who were old enough; there was, in particular, no indication that older job applicants limited reported work experience to 10 years.[32] This is reflected in Figure 3, which plots average experience by age – overall in the top panel, and by job in the bottom panel. Both panels indicate that, on average, reported experience on the resumes rises approximately linearly with age. This information, in our view, further justifies our interest in differential treatment between younger job applicants and older job applicants with experience commensurate with their age.[33]

<u>Resume quality/skills</u>

To be able to implement the correction for differences in the variance of unobservable, we designate half the resumes to be high skilled (or high quality), and half to be low skilled. We chose quality- or skill-related items to include based on the actual resumes. These items show up in three ways on the resumes. First, high-skill resumes can include a post-secondary degree, while all low-skill resumes only list a high school diploma. These degrees were most commonly B.A. for sales, administrative assistant, and security

[31] We do not know the universe of resumes on the website, so there was no way to adjust for this.
[32] While resumes for older workers did not always feature a complete job history indicating near-continuous work, there was no consistent way that older workers explained gaps when they existed.
[33] We also examined the persistence of careers within the same occupations, using phrases that appear to cover the same jobs. Administrative assistant includes administrative, receptionist, office manager, file manager, file clerk, or secretary; retail sales includes cashier, store manager, or sales clerk; security includes security guard or officer; and janitor includes janitor, cleaner, maintenance, dishwasher, housekeeper, or custodian. Because we likely cannot classify all job titles as accurately within the four jobs covered in the study, we assume that we obtain lower-bound estimates of persistence. For each type of job included in the study, between 29 and 32% of all jobs were in the same job as the current job for which the person was seeking work.

guard applicants, and Associate of Arts for janitor applicants.

Second, high-skill resumes include a "skills" section that can include computer skills of some kind (appropriate to the job), and fluency in Spanish as a second language, and other occupation-specific skills. Thus, aside from Spanish fluency and education, for administrative/secretarial jobs, the higher skills include typing 45, 50, or 55 words per minute, and facility with computer software (showing a randomly chosen mix of Quickbooks, Microsoft Office, and inventory management software). For retail/cashier jobs, the higher skills include Microsoft Office and programs used to monitor inventory (VendPOS, AmberPOS, and Lightspeed), and the ability to learn new programs. For security jobs, applicants are described as licensed in their state, and their resumes show CPR training. For janitor jobs, the high-skilled resumes indicate a certificate in using particular machines and a certification in janitorial and cleaning sciences. In addition, the skills section can include one of three volunteer activities (food bank, homeless shelter, or animal shelter). We also phrase the skills descriptions to match what we observed in our sample of resumes.[34]

In addition to education and skills, we also use slight variations in resume quality. All low-quality or low-skill resumes include two typos (one missing space and one missing period, with one of these appearing for the most recent job, which employers are most likely to read). Some high-skill resumes do not, so we can think of "purging" these errors as adding a skill.[35] Finally, some high-skill resumes include a description of "employee of the month" awards on the most recent job.

We do not assign all of the skill or quality dimensions to every high-skill resume, because we obtain valuable information (and the overidentification test) from being able to estimate the coefficients on different skills in the heteroscedastic probit model. Rather, from the vector of skill or quality characteristics (education, skills, interests, typos, and employee awards) we randomly assign five of seven possible skill indicators to each high-skill resume. We assign all applicants within each triplet as either high skilled or low skilled, with 50% probability for each. In contrast, control variables (resume characteristics) that are not

[34] We used the same sample of resumes described earlier to provide some tabulations of skills on actual resumes, based on our scraping of these resumes for descriptions of skills. These are reported in Appendix Table A4, which shows the prevalence of the skills we use on all resumes in all of our occupations (e.g., Spanish), and also the greater prevalence of particular skills for specific occupations (e.g., Microsoft Office for administrative and sales resumes, CPR and first aid for security resumes, and cleaning and related skills and certification for janitor resumes).

[35] Typos and grammatical errors were more common on actual resumes than spelling errors.

supposed to affect hiring are randomized across resumes in audit and correspondence studies. We make triplets uniformly high-skill or low-skill because skill and age define different treatment groups, and we do not want random assignment of high-skill or low-skill resumes within a triplet to dominate the effect of age.

Separations, employment, and unemployment

Some resumes list months only for very recent jobs, and some list them going further back. We use months in the job histories to better match the majority of the resumes, varying across resumes whether or not months are shown for much earlier jobs.[36] To mimic the actual seasonal pattern of job changes for different types of jobs, we randomly draw the separation month for each job, except the most recently held job, from the distribution of job separation dates from the Job Openings and Labor Turnover Survey (JOLTS). We use the general monthly distribution of separations for janitor and security resumes, the distribution specific to "Retail Trade" for sales resumes, and the distribution specific to "Business Services" for the administrative assistant resumes. During the course of the field experiment, every month we moved the ending date of the most recent job forward one month, so that durations did not lengthen during the time the experiment was in the field. We distinguish resumes based on whether applicants are currently unemployed.[37] We assign all applicants within each triplet as either all employed (the most recent job end date listed as "Present"), or all unemployed, with 50% probability for each. When applicants are unemployed, the resumes indicate that their last job ended in the month prior to the job application.

Applicant names and age

Applicant names were selected randomly from a set of the most common first names and last names for the relevant cohorts. This information was taken from the Social Security Administration list of most popular baby names.[38] We chose first and last names that were most likely to signal that the applicant was Caucasian, by excluding names where fewer than 60% of individuals with the name were Caucasian.[39] All

[36] And when months are shown, job transitions vary randomly as to whether they occurred in the same month, one month later, two months later, or three months later.
[37] Kroft et al. (2013) use a correspondence study to estimate the effects of duration of unemployment on hiring of the unemployed.
[38] See http://www.ssa.gov/OACT/babynames/ (viewed August 11, 2014).
[39] We use U.S. Census records on most common last names in the 2000 Census for last names. The 2010 data were not available when we chose the names. However, there were only minor changes from 1990 to 2000, so we suspect that

applicants, regardless of age or gender, had last names randomly assigned from the same selected set of last names. For first names, we created six separate sets of first names to draw randomly from for each age group (64 to 66, 49 to 51, and 29 to 31) and sex, using the most common first names for those groups. We chose the 20 most common names for babies born in each corresponding birth year, dropping names that were gender ambiguous unless using the full name made this clear (e.g., Patrick instead of Pat). The composition of names for the middle and older categories were very similar so we combined these categories before choosing our most common names for applicants in each age group.

Residential addresses

Addresses on the resumes were selected carefully to ensure that they were realistic for both older and younger applicants, did not signal a race other than white, and were not likely to send an unusual signal (positive or negative) about the applicant. We first chose zip codes that were not too far from the central business district(s) in the metro areas (or the center of the sub-markets used on the job-posting website, as explained in more detail below),[40] so that an employer would not be less likely to offer a job to those perceived as having an excessive commute.[41] We also chose zip codes that were not sparsely populated, and did not have high or low unemployment, family income, share black, or shares of old or young residents.

We began with all zip codes entirely contained within the Core Based Statistical Area (CBSA), Census-defined metropolitan areas that capture a labor market within which people commute.[42] We used data at the zip code level from the American Community Survey (ACS) to exclude any zip codes for which the characteristics listed above were unusual. To avoid sparsely populated areas, we exclude zip codes in the bottom quintile of the total population distribution across zip codes in the CBSA. We first exclude zip codes in the bottom quintile of the proportion of the population aged 25 to 34, 60 to 64, or 65 to 74. We then also exclude any zip codes that have an age distribution that suggests far younger residents than older residents,

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using the 2000 list is not problematic. (See http://www.census.gov/genealogy/www/data/2000surnames/surnames.pdf and http://www.census.gov/genealogy/www/data/1990surnames/index.html, both viewed August 11, 2014.)

[40] Sub-markets are regions within a city's market.

[41] Data from the 2009 American Community Survey indicate that over 50% of (one-way) commute times to work are 24 minutes or less in length, and only fewer than 15% are 45 minutes or longer (U.S. Census Bureau, 2011).

[42] See http://www.census.gov/geo/reference/gtc/gtc_cbsa.html (viewed August 11, 2014).

or vice versa, based on the ratio of those aged 60 to 74 to those 25 to 34.[43] In addition, we drop zip codes in the top or bottom quintiles of the distributions of the unemployment rate or median family income, or if the share black is in the top quintile of the distribution (areas with a low share black are not problematic, as there are many of them). We also exclude military bases and similar areas.

Among the zip codes that remain after imposing these restrictions, we drop zip codes that are more than 25 miles from the central location of the corresponding job market for the job posting locations we use to identify jobs. For central areas, we use the central business district, excluding zip codes more than 25 miles from the center of the zip code to city hall. For sub-markets on the job-posting website, we use distance from the zip code to the center of the sub-market, using city hall if the sub-market included it, and otherwise approximating by visual inspection of maps. Distances are measured using Google Maps, assuming travel by car; Google maps calculates these based on geographic centers of zip codes, except for downtown areas where it uses the city hall. Table 5 shows, as an example, the zip codes selected for the New York City CBSA and the associated sub-markets. We present summary statistics for the entire CBSA, and then for each zip code we selected. Appendix Table A5 shows all of the zip codes used.

We then assign street addresses for the zip code, using *Zillow* to select streets and addresses so that house prices at the address are about average for the metro area (having already selected zip codes with intermediate values of median family income). For each zip code selected, we search on *Zillow* for all houses for sale or rent, and pick a street where prices were near the averages for the city. We then utilized the "street view" function to select streets that were primarily residential, rather than a mix of residential and business, and to determine if the majority of buildings on the street were apartment buildings or detached houses. Once a suitable street was found, we picked a house to get the exact address (123 Main Street for example) and then used that to create a range of numbers around the house to draw from for our addresses based on 100s (so 100-200, in this example). For streets with mostly apartments, we assigned apartment

[43] We do not simply use percentiles of the distribution, because in some cities that have particularly old populations, the ratio of old to young residents can be quite high even at the bottom of the distribution, for example. We thus base our exclusion rules for the ratio of young to old on a hybrid of relative and absolute criteria, dropping zip codes with the ratio of older to younger residents below the minimum of 0.5 and the 20th percentile, and above the maximum of 2 and the 80th percentile.

numbers, choosing randomly from two to nine.

Within each triplet of applications sent in response to an ad, all applications were from different zip codes and different addresses. These were randomly assigned so that applicants with certain characteristics do not have tendencies to be from different kinds of neighborhoods (or homes). Using zip codes and addresses that are not outliers ensures that within triplets applicants are similar on these dimensions.

<u>Phone numbers and email addresses</u>[44]

The resumes had to include information that employers could use to contact our fictitious applicants, which is how we collect data on outcomes of the hiring process. We purchased "online" phone numbers for our applicants using *Vumber*. These do not appear any different than regular phone numbers to the employer, but have the benefit that the calls and voicemails are recorded in an online account and no physical phones are required.

We selected phone number area codes for all applicants that were located centrally in each metro area whenever possible. From the set of centrally-located area codes, we tried to avoid picking those that were too old, as these may be difficult to get or are considered "posh" (e.g., 212 in Manhattan), or too young, such that it might be far more likely that the area code would belong to someone younger (e.g., 929 in New York, which was only created in 2010). Table 6 presents the area codes we used for each metro area, along with information on their coverage areas and dates of creation. Four of the area codes we use were ones that were the first to be assigned to the area, in 1947, in some cases because other area codes that covered a similar geographic area were overlaid too recently. In Birmingham, there is only area code (205). In Phoenix, there were not enough 602 area code numbers available from our provider that were unique enough from each other, so we assigned each of the three applicants to a different area code in the greater Phoenix area (602, 480, and 623).

When employers respond by phone, they may not always leave a message that provides enough information to match them to an exact applicant (let alone job ad). Assigning a unique phone number to

[44] We give credit for some of the ideas in this subsection to an earlier correspondence study by Figinski (2013).

every job applicant and job ad would solve this problem, but is prohibitively expensive and complicated.[45] We purchased enough phone numbers to assign unique numbers to each group of job applicants defined by occupation (administrative assistant, janitor, sales, and security), city, sex (for sales, where applicants are either male or female), and type of triplet that the resume is a part of (triplet with two resumes of age 64-66, two resumes of age 49-51, or one of each, along with a young applicant). This results in 360 unique phone numbers. With all of these numbers, it is very unlikely that we would not be able to assign a response to an applicant, although assigning it to a unique job ad requires more information (discussed below).

We also needed email addresses for our respondents. Because some of the main email providers do not permit the creation of email addresses for fictitious persons, and because we wanted complete control of the email addresses, we purchased our own domain names and used them to create our own addresses. We purchased three domain names so that we could use different domain names for the applications in each triplet we sent out. With our own domains, we could create unlimited email addresses, so the email addresses we use are almost unique to each applicant. We do this by making each of the following attributes of the email address different for each applicant in a triplet: the domain name; using either the full first and last name (janedoe), the first initial and full last name (jdoe), or the full first name and last initial (janed); using a randomly selected middle initial (using all letters except l, y, z, q, u, and x), a period, an underline, or none of the above between the first and last name or initial, although in the randomization more than one applicant is allowed to have none of the above; appending either a 1, 2, or no number at the end of the email address, with more than one applicant allowed to have no number. This procedure for assigning email addresses also allows us almost perfectly to associate a response with an applicant, if the response is through email and does not otherwise provide sufficient information to assign the response to an applicant.

We created unique websites for our three domains in case employers decided to investigate the domain for legitimacy. The websites look like typical email services and include branding elements such as a logo created by a graphic designer. To add realism, the home pages even include buttons for signing in and creating an account, as well as account access and "contact," although these are not fully functional.

[45] The phone numbers cost $1.25 per number per month.

Clicking on any of the latter three generates an email to an email account associated with the domain. These emails, in addition to the number of hits to our website, provide a useful way to gauge if employers are viewing our website, and ultimately, if there is evidence they are finding the domain names questionable, which could impact response rates. The emails and website hits suggest very limited engagement with our websites.[46]

Schools

We randomly assign one of three high schools, and colleges and universities for the high-skilled resumes, for each city, to each applicant in our triplet. We use local schools, colleges, and universities that were in operation since 1960 so that there is no possibility that an applicant attended a school that was not operational at the time. We avoided top-tier/flagship universities whenever possible.

Resume triplets

After creating the final resumes, we combined them into triplets that go out in response to each job for which we apply. We send a triplet consisting of a young applicant and either (1) two older applicants, (2) two middle-aged applicants, or (3) one older applicant and one middle-aged applicant. These triplet types are assigned with probability one-third each. We then randomly choose the resumes, in cases (1) and (2) sampling without replacement two resumes from either the middle-aged or older resumes, and in case (3) sampling randomly one middle-aged resume and one older resume.

Each of the three resumes in the triplet was randomly assigned a different resume template, which ensured that all three resumes looked different. Most other characteristics were randomly and uniquely assigned to each resume in each triplet to further ensure that the applicants were distinguished from each other, and that any resume characteristics that inadvertently were more or less appealing to employers were distributed randomly with respect to the three applicants in each triplet. These characteristics included first

[46] Averaged over the months of February 2015 to May 2015 (four months where we applied for jobs the entire month), and averaged over all three domains, we had 84 unique visits per website per month. We looked at visits data for our websites before we started applying for jobs, and we looked at country of origin of our visitors, and this information roughly suggests that about half of these visits could be attributable to employers and that at least the other half is noise. About 93% of our visits are shorter than 30 seconds, suggesting limited engagement with our websites entailing simply taking a glance at an uncommon domain name for email. We also received 10 emails to these websites (that were not explicitly spam).

and last names, school names, addresses, phone numbers, email address formats and domains, cover letter style, and the language describing jobs and skills.

Appendix A shows our three resume templates. These are not complete resumes, but also display the "wild cards" (demarcated by asterisks) where we inserted different skills as appropriate, different school names, job ending dates, etc.

Applying for Jobs

Jobs to apply for were identified using a common job-posting website.[47] Research assistants read the posts each day and identified potential jobs. How companies use job titles is fairly idiosyncratic, with no clear rules for what would constitute a difference between a cashier and a retail sales associate for example. Given this potential ambiguity, research assistants were given a specific set of rules to select the jobs for which they applied. Jobs had to be entry level (e.g., not managers or supervisors) and fit the correct job description. Job ads requiring in-person applications or inquiries by phone were discarded, along with any ad directing applicants to an external website to apply.[48] Other reasons a job ad might be rejected include requiring additional documents that we did not prepare (head shots, salary history, etc.), requiring skills that our resumes did not have (such as speaking Mandarin), requiring a skill that was part of the vector of randomized skills assigned to a resume (or other features that our resumes might not have, like more than 10 years of experience), if the advertisement was for temporary or seasonal work, or if the job ad seemed like a scam collecting emails and other information. In the event of the same ad being posted twice, we endeavored to respond to the job at most once every 30 days. Companies with many openings at the same location received a response for one of the openings listed, but not all of them. When an ad listed openings across multiple locations, resumes were sent without indicating preference for one location.

Some of the exclusion criteria were occupation-specific. In particular, administrative assistant ads

[47] We planned to use electronic versions of Sunday newspapers as well, but many cities have contracted out job listings to major job posting sites that we cannot use under their terms of service.

[48] Large companies often contract out with external human resources firms to recruit. Retail stores such as H&M, Express, and the Gap utilize the services of Workforce1 Recruiting. Workforce1 requires applicants to go to an external page and submit their application using their own system. Other firms such as Walmart, Target, and Best Buy do not advertise online, but will only accept applications on their websites. In addition, there were some ads for Taskrabbit-type employers that were essentially getting people to sign up and be listed as an on-demand employee.

were excluded if the job advertised was for a personal assistant, bookkeeping, data entry, appointment setters, or if the job required different technical skills (e.g., assisting with IT). Ads were also excluded if they required the applicant to type at certain speeds, requested more than 10 years of experience, required a Bachelor's degree, or required knowledge of Quickbooks or Outlook. Retail sales ads were excluded if they were for sales that was not in a retail environment, or were for a merchandiser. Sales ads were also excluded from the list if they requested a Bachelor's degree, experience using POS software, or more than 10 years of experience in sales. Security guard ads that requested a Bachelor's degree or certification in CPR and first aid were excluded.[49]

Search methods for each occupation were standardized so that each research assistant performed their search the same way in each city, to ensure that applications were sent to similar jobs in each city and occupation, or at least that the selection rules were similar. With 16 research assistants applying for jobs, we set up numerous procedures to continually monitor and enforce similar job search decisions in each city and occupation. These included direct supervision of research assistants, a Facebook page where research assistants would post questions as they came up that were then answered (with answers conveyed to all research assistants), and periodic meetings of the entire research team to discuss procedures and clarify questions that could lead to research assistants using different procedures. To check that research assistants were following the guidelines, for a four-week period all ads that were read to determine eligibility were saved. Every time a research assistant opened an ad, it was saved as either a rejected ad or an ad to which a research assistant applied. Research assistants also tabulated the reasons that these ads were rejected. Appendix Table A6 provides information from these tabulations.[50]

[49] For security guards, requiring a state license was not one of the reasons used to restrict the job ads, because each state has different licensing requirements, with additional differences between armed and unarmed security guard jobs. To be consistent across states, we applied to any job that required a license for two reasons: the fact that our resumes claim to be currently employed implies that they possess a security guard license; and jobs that do not ask for the license would presumably have the same requirement but are not stating it explicitly in the posting. However, if the ad required providing a copy of the license, we did not apply.

[50] Research assistants were directed to avoid all ads that seemed to be spam when they applied to jobs, but in some cases they could not identify the ads as such. When a research assistant applied to a spam ad, the response came to the spam folders of our email clients. These responses often asked for credit card or bank information, contained egregious spelling and grammar errors, and were obviously not from legitimate companies (e.g., Canadian sculptors looking for personal assistants in Birmingham, AL). We saved the ads before the email client deleted the responses. At the end of

Once a list of jobs to apply for was identified, research assistants applied for the jobs using a set procedure. Each day was randomly assigned a different triplet of resumes in terms of skill levels, employed or unemployed, and the sex of the applicants. Within each triplet the order of resumes was randomized. The first resume was sent on the day the ad is found, and the remaining two were sent subsequently, at least one day later.[51] We created Word and PDF versions, but sent out PDFs unless otherwise specified, since since this format is the easiest for employers to open.[52]

To distinguish further the resumes in each triplet, we named the computer files slightly differently. One resume in the triplet was named "FirstLastResume," where First and Last were replaced with the applicant's first and last names, another resume was named "ResumeFirstLast," while the final resume was named "FirstLast." This naming convention is randomly assigned. Each ad that was applied to was saved for later research.

In our email responses to the posting, each application within a triplet uses a different subject line, opening, body, closing, and signature order.[53] Some of these scripts are based on examples and advice articles by job search experts.[54] We assumed that the text of our email responses would satisfy employers' requests to include a cover letter. Differentiating our email scripts further ensures that applicants from the

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the study, we attempted to identify the spam ads to get a sense of what share of negative/non-responses they constituted, and what cities and occupations generated them. We erred on the side of caution and only flagged the responses and associated job ids where we were very confident of the match. We identified 3,674 spam emails, 2,775 of which could be matched to 1,220 job ids that generated them (suggesting that in most cases spam responses went to all three applicants to the job id). Spam responses were most common in the administrative assistant ads. The majority of spam ads came from cities where it is free to post a job ad, but they did appear in other cities as well. Of the ones that we could match, 93% were for administrative assistants and 78% were in Birmingham, Salt Lake City, and Sarasota. We did not delete these observations for two reasons. First, there may have been other spam responses we did not identify. And second, from the point of view of a job applicant a spam response is an unproductive response to a job application.

[51] Normally, the three resumes would go out on consecutive days. However, if the ad had been up for more than a day (i.e., posted on Saturday and we found it Monday), then the second resume would go out one day later in the morning, and the third resume that evening (at least 12 hours apart). The scheduling of ad submission was done using the "send later" add-on to Mozilla Thunderbird.

[52] We used .doc instead of .docx since job search experts suggest that .doc is easier for employers to use. (See http://jobsearch.about.com/b/2014/02/21/resume-file-format.htm, viewed November 8, 2014.) We removed author and edit history data from our Microsoft Word format resumes so that employers could not potentially see that one of this study's authors or research personnel created or edited the document. We tried to accommodate the requests of the employer (e.g., pasting the resume in the email), as long as the request did not require any changes to the document.

[53] Note that there are only two openings and signature orders used. Our perusal of job application websites generally found only these two openings, so we randomly assigned the two versions to the three resumes. Based on the websites, we used "Dear Hiring Manager" as the opening in two out of three, and made the indicated choice for the signatures.

[54] See http://jobsearch.about.com/od/jobapplications/u/job-applications.htm (viewed August 7, 2014)

same triplet are not perceived as related by the employer.

With such a complicated protocol, based in part on subjective decisions, it would not be surprising if some errors were made regarding which application went to which job. In the early going of applying for jobs, this process was monitored closely, to reduce errors, and after the first month or so, applications were spot-checked. We tabulated errors that were detected (either by this monitoring, or self-reported by the research assistants in checking their work); these are reported in Appendix Table A7. The rates of occurrence of these errors declined sharply once early errors were pointed out to research assistants and they were better trained. Moreover, the errors that occur in a non-negligible share of cases ("Sent resumes at wrong time" and "Sent resumes in the wrong order") do not invalidate the data. Moreover, these were random with respect to the age of the applicant, as we verified by estimating probit models of these types of errors on the age dummy variables, finding estimated coefficients very close to zero and statistically insignificant. The other errors that could conceivably lead to an invalid observations (e.g., "Sent resume from the wrong occupation") occurred with such low incidence that we chose to retain the observations and avoid subjective decisions about which observations to drop.

Sample size

Our original plan was simply to have three types of resumes: young, old low experience, and old high experience. For this design, we had a target sample size of 11,520 observations, based largely on desired sample size in light of precision of other studies, to detect as significant estimated callback differentials similar to those in past studies.[55] In the course of getting feedback on the research design, we expanded to the eight different resumes used in this study, adding the three middle-aged resumes, and splitting the older, high experience resumes into three groups based on bridging behavior. With eight groups instead of three, this implies a desired sample size of 30,720 (11,520 x 8/3). However, having hired research

[55] Bertrand and Mullainathan (2004) – in a study focused on race – had about 5,000 observations for four types of applicants, and were able to detect as statistically significant relatively small differences in callback rates of 0.03; their standard errors were 0.01. Bendick et al. (1997) had 1,550 applicants, obtained a huge age difference in callback rates (0.265) for the relevant employers, and had standard errors of 0.01. Riach and Rich (2006) found similarly large differences in callback rates by age. Lahey (2008) found smaller differences by age (0.016 or so), but with fewer than 5,000 applicants could detect these interview rate differences as statistically significant. Because we using quite old older applicants, we expected to find larger baseline differences in hiring rates by age than in these studies.

assistants for the job application process on a quarterly basis, we continued our efforts for the quarter in which the sample size was reached, ultimately applying to just over 40,000 jobs. Of course, there are additional comparisons of interest for which the larger sample is useful, such as men versus women within sales, and comparisons across cities in states with different age discrimination laws.

Collecting Responses

Responses to job applications could be received by email or by phone. All responses were forwarded to a central email account, with voicemails arriving as attachments. Research assistants then read each email and listened to each voicemail to record the response. The actual content of the responses did not always enable us to match the response to a specific job ad. However, because of the way we designed the email addresses and chose phone numbers, each mode of contact contained identifying information that allowed us to match the response to the resume that was sent. Research assistants then used additional information extracted from the email or voicemail to match a response to a specific job ad.[56] All email responses contained the firm's contact information and the job applicant's email address. In addition to the text of their response, some combination of the firm's name, email address, and website was present in the email. If the email was sent as a reply to the job-listing website submission, then the email also contained a unique id number for the job ad. Each id number provided a one-to-one match to a job ad. However, if firms responded directly to the individual, thus not providing a match to a job id number, then company name or type, job ad title, and location were used to match to the specific job.

Phone call responses conveyed less information. Every voicemail contained the phone number of the firm calling and the phone number on the resume they were trying to contact. The automated voicemail message instructed firms to include their name and their number in their message. Identifying information that was extracted from a voicemail included the firm name, applicant name, the job title, and any other information that could be used to narrow down the list of possible job ads (e.g., how long ago they received the resume). The information extracted from the voicemail was used to match each voicemail to a job ad

[56] From the job ads, research assistants had recorded the date the resume was sent, the company name or a description of the company (e.g., "Home Depot" or "home improvement store"), the city in which the ad was posted, the job for which the application was submitted, and the title of the job ad. Titles for each job ad included the title of the job and the part of the city the job was located in (e.g., "Seeking Part-Time Administrative Assistant/Receptionist (NW Houston)").

whenever possible.

Sometimes we were not able to match responses to a unique job ad. If a match to a single job ad could not be made using the information in the response, responses were matched at the highest level of detail possible. Using first names and last names, nearly all email responses could be matched to the email that generated the response. This allowed a one-to-one match to the exact resume, but not the ad to which it was sent.[57] A one-to-one match with a resume could be made for a voicemail using the first and last names. If the voicemail only included the first name or the last name, an attempt was made to narrow down the possible resumes using other information. In a small number of cases (about 200), we could not match the response to any resume. These cases are dropped because without the resume match we do not know the age of the applicant.

Table 7 reports the matching of responses by voicemail and email to job ids or emails. Even though most responses can be matched to job ids, we want to make use of all the data. Furthermore, for the analysis in this paper, there is no information beyond that on the resumes that is useful for the analysis, and the email match identifies the resume used. Thus, we make use of all of these data, and we cluster at the email/resume level in our statistical estimation.

Each response was coded as an unambiguous positive response (e.g. "Please call to set up an interview"), an ambiguous response (e.g. "Please return our call, we have a few additional questions"), or an unambiguous negative response (e.g. "Thank you for your interest, but the job has been filled"). To avoid having to classify subjectively the ambiguous responses, they were treated as callbacks;[58] the negative responses were treated the same as no callbacks.[59] Responses were then matched to the record of job to which the applications was sent, whenever possible. This allowed for us to determine how long it had taken for a response to be received, what order the responses come in, and who else in a triplet received a response.

[57] There was a handful of cases where because of same last names and first names that start with the same letter, the randomization that creates the emails (using names as well as other features) leads to the same email address. Thirteen of these resumes received a response. We attempted to narrow down the resume that was sent using the timing of the response, but were unable to do so because the individuals were sent in close proximity. These responses were left at the phone number level.

[58] The ambiguous responses are 6.6% of all cases coded as positive callbacks.

[59] See the earlier discussion of spam responses.

These kinds of characteristics of responses have been used in past studies, and we also look at them, briefly, in addition to the simple callback/no-callback response.

6. Results[60]

Basic Callback Rates

Table 8 reports raw differences in callback rates for each occupation, and for the four occupations combined. We report statistical tests of whether callback rates are independent of age for the different possible three-way and two-way comparisons. Beginning with administrative jobs (Panel A), for which we found by far the most ads eligible for the study (about 61% of the total), the callback rate is 14.4% for young applicants aged 29-31. It is about 29% lower for middle-aged applicants (ages 49-51), with a callback rate of 10.3%, and about 47% lower for older applicants (64-66), with a callback rate of about 7.6%. For the three-way test of independence, and each possible two-way test, we strongly reject the hypothesis that age of applicant and callback rates are independent, and clearly the evidence is strongly in the direction of lower callback rates for older applicants.[61]

The next largest number of applications was in sales. Because we have both male and female applicants in sales, we report callback rates by sex. As Panel B – for males – shows, the callback rates for middle-aged versus young applicants were not very different, and the callback rate is actually a shade higher for the middle-aged group. But the callback rate for older applicants was a lot lower – 14.7%, versus 20.89% for young applicants, a difference of 30%. And the differences between young and old (as well as middle-aged and old) applicants are strongly statistically significant. For female sales applicants (Panel C), in contrast to the case for men, the callback rate for middle-aged applicants is lower than for younger applicants (25.9 versus 28.7%), although only marginally significant (p-value = 0.11). And the callback differential

[60] We considered developing and filing a pre-analysis plan. However, we ultimately decided this was unlikely to be fruitful. The basic analysis of callback differences by age is standard in these studies, typically entailing testing for differences with no controls, and then verifying that results are robust to including controls, which virtually has to be the case because of the randomization. In contrast, the analysis of the role of differences in the variances of unobservables is potentially a sequential procedure, involving testing which skills on the resumes predict hiring, incorporating them into the heteroscedastic probit estimation, testing the overidentifying restrictions, and adjusting the specification as necessary to avoid using rejected restrictions. Paralleling the discussion in Olken (2015), it could be very complex to specify the decision tree for this process in advance. Nonetheless, this latter analysis closely follows Neumark (2012).
[61] This test treats the observations as independent. In the regression (probit) analyses that follow, the standard errors are clustered appropriately.

between old and young applicants is larger (over 10 percentage points). Thus, there is evidence of stronger age discrimination for women than for men in sales. Note also, however, that the callback rates at all ages are higher for women than for men.[62]

There were far fewer ads to apply to for security (around 4,100) and janitor (around 1,700) jobs. For security jobs (Panel D), the data indicate roughly equal callback rates for middle-aged and older applicants (around 21.5%). Both are lower than the callback rate for younger applicants (24.3%), with p-values of 0.09 and 0.12. For janitor jobs (Panel E), the callback rate was slightly higher for middle-aged than younger workers. But the callback rate for older workers was significantly lower for older applicants (25.9%), providing statistically significant evidence of discrimination against the oldest applicants.

Finally, combining all four occupations, in Panel F we find strong overall evidence of age discrimination, with callback rates statistically significantly lower by about 18% for middle-aged workers, and about 35% for older workers. Of course, these differences are driven by the occupations with more applications. This raises the question of how one might weight the differences across occupations to be more representative of the jobs to which older workers might apply. We do not apply any such weighting, because this study – like all audit or correspondence studies – is ultimately a case study (or case studies) of a number of occupations rather than an attempt to be representative. Nonetheless, two conclusions seem fair. First, the distribution of ads to which we applied is to some extent representative of hiring opportunities for older workers, at least in this set of jobs and on the job-listing website we used. Second, however, the large number of administrative job ads, coupled with the sex composition of new, older hires in this occupation, suggests that our results may speak more to hiring of older female workers than of older male workers. And coupled with the evidence in Table 8, at least for the jobs we study the evidence of age discrimination in hiring is stronger for the female or mixed jobs (administration and sales) – and in the mixed job (sales), stronger for women.

Other Callback Metrics

[62] Bertrand and Mullainathan (2004) did not find discrimination against women in retail.

We also examined other dimensions of callback behavior.[63] Table 9 presents raw data on whether there were multiple callbacks for the same job ad. Panel A shows that the multiple callback rate was highest for young applicants, and falls monotonically with the age of applicants. Although the multiple callback rates are very low (1.3 to 2.4%), the differences by age are statistically significant. Panel B, which conditions on a callback occurring, shows the same pattern; the differences are statistically weaker, although significant for the young versus old comparison. Thus, the analysis of multiple callbacks gives similar qualitative evidence of discrimination against older job applicants. However, given the very low incidence of multiple callbacks, we do not analyze this outcome further.

Figure 4 presents evidence on how quickly the employer responded to the application, to see whether – reflecting the other differences documented thus far – employers responded to the younger applicants earlier, perhaps in the hope of first trying to hire them. (Recall that we do not respond to callbacks, so the employer might conclude that a younger applicant to whom they responded first was not interested, and only then turn to an older applicant.) As Figure 4 shows, there were no detectable differences in the distribution of days until callbacks, for applications that prompted callbacks.

Multivariate Estimates for Young, Middle-Aged, and Old Applicants

We next turn to multivariate analyses of differences in callback rates by age. Given the random assignment of age to resumes, except for resume characteristics associated with age – mainly, experience on the high experience resumes – there is no reason to expect conditioning on characteristics of the resumes or applications to have much impact on the results – and that is reflected in what we see in these analyses.

In Table 10, for each occupation separately and then the four occupations combined, we report results of probit estimates for callbacks (showing the marginal effects). In each case, we first report results with controls for the city, the order in which applications were submitted (first, second, or third for the job ad), whether the worker is currently employed or unemployed, and the vector of skills randomly assigned to the resume. We then add controls for an extensive set of resume features that are listed in the notes to the

[63] These analyses use only positive response observations that can be matched to specific job ads (Table 7).

table. In the combined specifications, we add controls for female, and for occupation.[64] Note that we do not condition on experience shown on the resumes, based on our argument that the most relevant comparison is between older workers and younger workers when each has experience commensurate with their age. Of course, if we only included younger applicants and older applicants with higher experience, then we could not separately identify the effects of age and experience.[65] With the addition of older, low-experience applicants to the applicant sample, age and experience are separately identifiable in a meaningful way, but conditioning on experience would imply that the estimate of γ_{OHE} would reflect outcomes for older workers as if they had low experience, which is not what we want.

For the administrative jobs, there is statistically significant evidence of discrimination against both middle-aged and older applicants relative to young applicants, and the differential is nearly twice as large for older workers (with a callback rate lower by 6.3 percentage points for older applicants, and 3.5 percentage points for middle-aged applicants). Here and in the rest of this table, the marginal effects are close to the differences in the raw data from Table 8 (compare, e.g., the marginal effect of −0.063 for older versus younger workers in administrative jobs to the differential of 6.8 percentage points in Panel A of Table 8). And, as expected, the addition of the control variables makes little difference.

For male sales applicants, there is a small and insignificant difference between middle-aged and younger applicants (in the direction of lower callback rates for middle-aged applicants), while the difference between older and young applicants is about 4.5 percentage points, and is strongly significant. For females, in contrast, the estimated differentials for both middle-aged and older applicants are larger (about 5.1 and 9.5 percentage points), and both differences are statistically significant relative to younger applicants.

For security jobs, as for male sales applicants, the point estimates indicate lower callback rates for middle-aged applicants, but the differences are not statistically significant. For older applicants the estimated

[64] The model is estimated using standard errors clustered at the resume level, since it possible that there are random features of resumes that affect the outcomes. There may also be random influences at the level of the job ad, but as already discussed we cannot match all responses perfectly to job ads. If we cluster at the job ad level instead of the resume level (for the observations for which we can match to job ads), standard errors are a bit smaller. However, these latter standard errors are likely too low if there are random sources of variation at the resume level that influence callbacks, given that the same resumes are used for many job ads.

[65] This would be strictly true only if measured experience exactly reflected the age difference between applicants.

differentials are a bit larger (about 2.8 percentage points), but only significant (and at the 10-percent level) for the second specification. For janitor jobs, there is no evidence of age discrimination against middle-aged applicants. For older applicants, however, callback rates are lower by about 5.5-6 percentage points, although the evidence is not as strong statistically, likely reflecting the smaller sample size. Finally, combining all occupations, we find strong evidence of discrimination against both middle-aged and older applicants, with callback rates lower by about 3.3 and 6.2 percentage points, respectively, relative to a callback rate of 18.7% for younger applicants.[66]

These results point to a conclusion that will be echoed in analyses that follow. In particular, for the occupations we study there is unambiguous evidence of age discrimination for female job applicants, and this is true for both the middle-aged and older groups. For males the evidence is less clear. We never find statistically significant evidence of age discrimination for the middle-aged relative to the younger applicants, and in one case (janitors) the point estimates are in the opposite direction. And the evidence for older applicants is weaker – with smaller estimated differentials in sales, and quite weak evidence in security. Other analyses discussed below further weaken the evidence of age discrimination for men.

A Richer Characterization of Resume Types

Table 11 turns to our analysis that estimates differences between the types of resumes used for middle-aged and older applicants. We estimate differences in outcomes between resumes for older applicants (both age groups) that show the same experience as the younger resumes, as compared with showing experience that is commensurate with age. We also estimate differences associated with whether applicants are "bridging" to a lower-skilled job, or – for the older applicants – whether they have already done so. In all cases, we use the more detailed set of controls from Table 10.

Turning first to administrative jobs, the first three estimates reported in column (1) are for the three types of middle-aged resumes: commensurate experience and no bridging (M_{HNB}); commensurate experience with bridging (M_{HB}); and low experience (M_L, also with no bridging, but since the low-experience resumes

[66] Perhaps not surprisingly given the large sample and differences in parameter estimates, we strongly reject the pooling restrictions implied by combining the results for all occupations. (For this test, we simply use the high-skill indicator for the models for each occupation, and we estimate separate models by sex for both sales and the combined occupations, to avoid non-nested models.)

never entail bridging there is only an "L" subscript). All three estimates indicate lower callback rates than young applicants, with the range of estimates from 2.7 to 4.1 percentage points lower. The next four estimates are for older applicants. Again, all four estimates are strongly significantly different from zero, indicating lower callback rates than for young applicants regardless or resume type. The range of estimates for the different older resumes is small – from 4.8 to 5.8 percentage points lower.

Subsequent rows in the table report statistical tests. First, as shown in Panel A, we strongly reject the hypothesis that there is no difference between the middle-aged callback rates versus the callback rates for younger applicants, which is not surprising given that all three coefficient estimates in the top three rows are significant at the one-percent level. Similarly, we strongly reject the hypothesis that callback rates are equal for older and younger applicants.

Panel B considers broad hypotheses about differences between the resumes for middle-aged and older applicants. We cannot reject any of the following hypotheses: (1) the estimated effects for the three middle-aged resumes are equal (p-value of 0.12); (2) the estimated effects for the four older resumes are equal (p-value = 0.50); or (3) the joint restrictions that the middle-aged resumes have the same effects, and that the older resumes have the same effects – the restrictions implicit in Table 10 (p-value = 0.26).

Finally, we break these hypotheses into more substantive subsets, focusing separately on the question of how much experience the resumes show, and bridge versus non-bridge resumes. Panel C shows that there are not significant differences between the estimated callback rates for resumes showing low experience versus experience commensurate with age, for either middle-aged or older applicants, although the p-value for middle-aged applicants is relatively low (0.12) and the point estimate is most negative for the low-experience resumes. But coupled with the absence of any evidence that for older applicants the callback rate is lower for the low-experience resumes, overall, the evidence for administrative jobs contrasts with the conjecture that showing low experience on resumes for older job applicants could lead to spurious evidence of age discrimination.[67] Similarly, Panel D reveals no significant differences in the estimated effects of resumes

[67] If we exclude the spam ads, which is really relevant only for administrative job applications, the statistical evidence for middle-aged applicants was stronger, with a p-value of 0.05. However, the results for older applicants still did not indicate any difference between low- and high-experience resumes.

based on whether the current applicant is bridging to a lower-skilled job (M_{HB} or $O_{HB}{}^{L}$) or already has ($O_{HB}{}^{E}$); and as the top rows of the table show, the point estimates are substantively the same.

The remaining columns of Table 11 report the same analyses for the three other occupations, and the four occupations combined. Looking at male or female sales applicants, security jobs, or all jobs combined, the conclusions from the key statistical tests are similar. In almost every case we do not reject hypotheses – whether for middle-aged and older applicants separately, or considered together – that the estimated effects are equal regardless of experience, or for the different bridge or non-bridge resumes.[68]

For janitors, however, a significant difference emerges. In Table 10, we found evidence of discrimination against older but not middle-aged janitor applicants, and the evidence was statistically weaker than for administrative or sales jobs. However, in Table 11 we find no evidence of discrimination against older janitor applicants showing high experience, but strong evidence of discrimination against older janitor applicants reporting low experience. (Recall that we did not construct bridge resumes for janitors, because we did not see such resumes in the real resumes we examined.) For the older applicants reporting the same experience as younger applicants, the estimated callback differential relative to young applicants is 9.4 percentage points, significant at the one-percent level. And the test statistics reported in Panel C indicate that the difference depending on whether experience commensurate with age is shown on the resume is significant at the five-percent level.

Thus, for this occupation, there is arguably a bias against finding age discrimination from using resumes that do not report a "full" job history. It is possible that this result arises for janitors for the same reason that we did not find "bridge" resumes – that janitors tend to stay in the same job throughout their career, in which case missing experience might be viewed as reflecting a period of non-employment, rather than employment in other jobs that not be regarded as relevant in hiring.[69]

[68] The one exception (in 20 tests), for the restriction M_{HNB}=M_{HB} for male applicants in sales), is not in the direction of lower callbacks for bridge resumes.

[69] Indeed, we have some weak anecdotal evidence of something even worse. In research seminars in which we presented the research design and protocol prior to collecting data, we asked participants to comment on and compare the different resumes we intended to use. One comment we received regarding janitorial resumes was when they showed low experience for an older applicant, it seemed natural to assume they had spent time in prison during the years not covered in the resume's job history. This might be less likely for the security positions where a prior

To this point, we have found stronger evidence of age discrimination for the female jobs or mixed jobs we study, stronger evidence of age discrimination against women than against men in sales, only weak evidence of age discrimination against men in security jobs, and, now, more ambiguous results for janitor jobs depending on the resumes used. Thus, there are a number of indications that the evidence of age discrimination is less consistent and compelling for older men than for older women.

Differences by City Demographics and State Age Discrimination Laws

We next briefly examine differences in results between the younger, middle-aged, and older *cities* included our study, and between states with stronger age discrimination laws relative to those where the ADEA applies. With only 12 cities in 11 states, we can obtain at best suggestive evidence.

The first three columns of Table 12 report results based on the age composition of our cities (Table 4). For middle-aged applicants, callback rates were lower in the youngest cities (by 3.9 percentage points), compared to the other two sets of cities (2.7 to 2.8 percentage points lower). For older applicants, the pattern is not monotonic with age, and the point estimates for the young and old cities are the same (6.7 percentage points lower callback rates). The lower part of the table reports statistical tests of the equality of coefficients across cities, for models that interact only the age dummy variables with city demographics, and for fully interactive models. We never reject equality of the age effects across the different groups of cities.

In columns (4)-(7) of Table 12, which look at differences in age discrimination laws, we find a consistent pattern. Where age discrimination laws are stronger, the evidence of discrimination is weaker. For example, for the old applicants, the callback rate is 7.4 percentage points lower than for young applicants when damages are restricted by the ADEA, but 5.8 percentage points lower where larger damages are available under state law. This pattern holds in every case – for middle-aged and older applicants, and for the two dimensions of age discrimination laws (damages and a lower firm-size cutoff). However, the lower part of the table shows that these differences are never statistically significant.

Are the Results Driven by Differences in the Variances of Unobservables of Older versus Younger Workers?

Finally, we turn to potential biases introduced by differences in the variance of unobservables. Here,

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conviction or prison term might disqualify an applicant and hence they would not apply (or have a license).

because the analysis is fairly complex, we focus on the sharpest and more important results – the differences in outcomes between young and old applicants.

We begin, in Table 13, by reporting estimates of models that correspond to those in the odd-numbered columns of Table 10. However, we add interactions between the skills included and the old indicator (and unlike in Table 10, we report the estimates of the skill variables). The interactions are informative because under the identifying assumption that the underlying coefficients of the latent variable model for hiring for the two age groups are equal, differences between the probit coefficients – picked up in the interactions – are informative about differences in the variances of the unobservables. For example, if – as we conjectured – the unobserved variance is larger for older workers, then, if the main effect of the skill variable is positive, the estimated interaction should be negative and reduce the overall effect towards zero, and vice versa.[70] The first part of the table reports results for the five common skills, followed by rows that report the occupation-specific skills.

For administrative jobs, three of the main skill effects have statistically significant positive effects – college, volunteer (at the 10-percent level), and words per minute ("Skill 2"). In all three cases, the interactions are negative, and the combined main and interactive effects are smaller than the main effects (in absolute value), consistent with a larger variance of the unobservable for older applicants. However, other skills point to larger effects for older applicants (most notably, computer skills). So the overall implications for the relative variances of the unobservables are not immediately clear.

For sales workers, the skill variables are less successful in predicting hiring. Indeed none of the estimated main effects or interactions are statistically significant (and none of the main effects were significant when omitting the interactions). For males, the only main effect with a t-statistic exceeding one is employee of the month, for which the estimated interaction is of the opposite sign and points to a diminished effect for older applicants, although there are also estimates pointing to a larger effect for older applicants (customer service). For female sales applicants, none of the t-statistics exceeds one; for six of the seven

[70] The standard computation of marginal effects for interactions accounts for changes in each variable in the interactions. Here, though, our main interest is in the signs and magnitudes of the underlying probit coefficients on the "Old" and the "Old-skill" interactions, of which the marginal effects reported here are approximately rescaled versions. That is, we could have just reported probit estimates, but these have no clear interpretation on their own.

skills the main effects and interactions are of opposite signs, but not consistently pointing to diminished effects for older applicants (e.g., college versus employee of the month). Thus, for sales workers, it is less clear which way the heteroscedastic probit estimates will point with regard to the variances of the unobservables.

For security workers, Spanish strongly predicts hiring, although the interaction suggests the effect is larger for older applicants, consistent with a lower variance of the unobservable for older workers. The main employee of the month effect is, counterintuitively, negative. For a number of other skills, though, the estimates point to large effects for the young applicants but effects closer to zero for the old applicants, consistent with a larger variance of the unobservable for older applicants. For janitors, college (which in this case means an Associates degree) strongly predicts hiring, and the interaction is negative with a combined effect (for older applicants) closer to zero. The same is true of technical skills and volunteer (although volunteer has an unexpected negative main effect). Even more than for security, for janitors many of the estimates point to large effects of the skills for the young applicants but effects much closer to zero for the old applicants – again consistent with a larger variance of the unobservable for older applicants.

Finally, column (6) reports estimates for all applicants combined, using only the five skills common to these applicants. Only college significantly predicts hiring, and the interaction points to a smaller effect for older workers, consistent with a larger variance for them.

Table 14 turns to the heteroscedastic probit estimates that correct for biases from differences in the variance of unobservables (when combined with using resumes from a narrow range of the distribution of actual applicants). The first row of Table 14 reports the marginal effects from the standard probit model for each specification and sample. The only difference here, and it is trivial, is that we use the continuous version of the partial derivative, because this version gives an unambiguous decomposition of the estimates from the heteroscedastic probit model (Neumark, 2012). These estimates are similar to the corresponding ones reported in Table 10. The first row of Panel B reports the overall effect from the heteroscedastic probit estimates. These are similar to the probit estimates. Next, we report the p-value from the overidentification test that the ratios of the skill coefficients between younger and older workers are equal across all of the skills. This p-value is uniformly high, indicating that we do not reject the overidentifying restrictions in any case. Looking back at Table 13, however, we can see that in some cases the estimated coefficients of the skill variables (and their interactions) are imprecise, so the failure to reject may partly reflect low power.

Turning to the more substantive findings, we next report the ratio of the standard deviation of the unobservables for old relative to young applicants. Recall that our conjecture was that the standard deviation (or variance) would be higher for older applicants, so that if the resumes were of lower quality than the average applicant, there would be a bias *against* finding age discrimination in hiring, because the higher-variance group would be preferred. The last two rows of the table decompose the heteroscedastic probit estimates. The "level" effect (labelled "Old-level" in the table) is the unbiased estimate, and the "variance" effect is the artifact of the correspondence study design – which is sensitive to the quality of the resumes sent out relative to the actual distribution, as well as differences in the variances of unobservables.

For administrative applicants, the estimated ratio of standard deviations is just below one (0.94), suggesting no substantive difference and hence no bias. Similarly, the p-value from the test of equality of the standard deviations is high (0.61). The very similar standard deviations are reflected in the failure to reject the restriction to a homoscedastic probit model (p-value = 0.56). More substantively, the similar standard deviations are reflected in the decomposition of the heteroscedastic probit estimates. The level effect (a 5.4 percentage point lower callback rate) is close to the probit effect and overall heteroscedastic probit effect, and, while less precise owing to the more-demanding estimation, is still significant at the 10-percent level. And the estimated variance effect is near zero.

For the other occupations, though, some more interesting differences emerge. For male sales applicants, the estimated ratio of standard deviations is not as close to one (0.84), and – in contrast to our conjecture – is lower for older workers. Reflecting this, the p-values for the test of equality of the standard deviations and the likelihood ratio test are lower, although still above 0.1. This has interesting implications for the decomposition. In particular, the estimated level effect, which is the unbiased estimate of discrimination, is now near zero (−0.005), and nearly all of the effect comes from the variance – interpreted as spurious evidence from the research design – although these estimates are imprecise. Note also that the lower variance for older, male sales applicants would predict that the standard probit estimates would overstate discrimination if the resumes were on average low quality, which is what we find.

For female sales applicants the results are reversed. The variance of the unobservable is much higher for older women, with a ratio of 1.44 that is significantly different from one at the 10-percent level. Correspondingly, in this case the estimated true discrimination effect is much larger (−0.16), and strongly significant. With a higher variance of the unobservable for older applicants, low-quality resumes should imply downward bias (towards zero) in the estimate of discrimination, which is what we find. Thus, the results for sales job applicants further drive apart the evidence on age discrimination for male and female sales workers – beyond the difference noted in Table 10 – and reinforce the strong and robust evidence we are finding of age discrimination against older women, and the ambiguity of the results for older men.

Next, we turn to the results for security. The ratio of standard deviations of the unobservables for old relative to young applicants (1.16) points to a higher variance for older applicants. This leads, in the decomposition, to somewhat stronger evidence of age discrimination against older security workers. The finding of a higher variance of the unobservable for older applicants coupled with an increased estimate of discrimination from the heteroscedastic probit estimates is, again, consistent with lower-quality resumes that bias downward the estimate of age discrimination.

For janitor jobs, the standard deviation of the observable is much higher for older workers (the estimated ratio is 1.66). Likely reflecting the small sample size, the difference is not statistically significant. But the p-value for the likelihood ratio test is only 0.11. In the decomposition, the point estimate of the unbiased effect of discrimination is much larger (−0.15 versus 0.05), and despite quite large standard errors the estimate is statistically significant at the 10-percent level. Thus, the unobservables correction here suggests that the probit estimates are biased against finding discrimination against older workers – again what we would expect from a larger variance of the unobservable but lower-quality resumes.

Finally, the last two columns report results for all of the occupations combined. Given the differences we have just discussed for each occupation, the pooled results might be discounted. Regardless, overall we do find a somewhat larger variance of the unobservable for older workers, albeit not by much, which translates into somewhat larger estimates of age discrimination. Note that this is consistent with the general conjecture that the issue of different variances of the unobservables generate a bias against finding age discrimination.

What do we make, overall, of the evidence from the heteroscedastic probit estimation? We think there are a few conclusions. First, some of the estimates of age discrimination are sensitive to this correction. The evidence of age discrimination for women is reinforced, as this evidence is shown to be robust to differences in the variances of unobservables for administrative applicants (all of whom are female), and the evidence of discrimination for female sales workers becomes considerably stronger.

For men, on the other hand, there is some ambiguity, as the evidence of age discrimination for male sales workers disappears, while for janitors and security workers the estimates – albeit less precise – suggest that the standard approach ignoring the unobservables problem may understate discrimination. However, recall from Table 11 that the evidence for janitors is driven by the low-experience resumes, and hence may be spurious for other reasons. Indeed when the unobservables analysis was re-estimated using only the high-experience resumes – which, we have argued, better address the policy and legal question – the estimated level effect fell by half and was not statistically significant (p-value = 0.38).[71] Discounting this occupation, then, the unobservables correction leaves relatively little evidence of age discrimination for men (only for security workers, and then significant only at the 10-percent level).

Second, though, formally there is only one case – for female sales workers – where there is statistically significant evidence of differences in the variances of unobservables that bias the estimates. So if we focus exclusively on this case, the evidence of age discrimination does strengthen as a result of this approach, albeit only for one group. Moreover, we remind the reader that this was the group of applicants for which the skill variables were least successful in predicting hiring, so it would seem that we might want to withhold firm conclusions until other studies can examine more evidence on age discrimination against women (in this and other occupations) with more successful incorporation of skill-related elements that shift

[71] Interestingly, the ratio of standard deviations of the unobservables falls from 1.66 to 1.33, suggesting that the low-experience resumes are perceived by employers as providing less information that might be relevant to the hiring decision.

hiring outcomes.[72]

Robustness to Sample Decisions

Earlier, we discussed cases where the experimental protocol was not followed correctly. To assess the sensitivity of the results, we re-estimated all of our models dropping cases with errors in the protocol. The results were very robust.

We also discussed the issue of spam ads, and why we retained these records in the analysis. A potential consequence of retaining these observations, though, is understating age discrimination, because the spam ads generate null responses in a manner that should be unrelated to age. Because nearly all spam ads were for administrative assistant jobs, including or excluding them had no bearing on results for other occupations, for either the unobservables analysis or the other results the tables report (which we verified by re-estimating all of our models excluding these observations). However, the results for administrative jobs generally showed slightly stronger evidence of age discrimination when the spam ads were dropped, as expected. The only place this makes a qualitative difference is for the estimates in the column (1) of Table 14. Here, dropping the spam ads led to stronger evidence of age discrimination after correcting for differences in the variances of unobservables. Specifically, the "Old-level (marginal)" estimate was −0.081, significant at the one-percent level (versus −0.054, significant only at the 10-percent level, in Table 14). The "Old-variance (marginal)" estimate remained small and statistically insignificant.

Thus, the implication of this additional analysis is that these sample decisions do nothing to drive our findings that evidence of age discrimination for men is ambiguous (which arises for sales and janitors). Similarly, it does not drive our findings for women; if anything, the evidence of age discrimination in administrative jobs (for women) is stronger than what we already report in the tables.

7. Conclusions

There are a number of audit and correspondence studies of age discrimination, which almost

[72] The relatively weak effects of these skill variables on callbacks may lead to the unobservables analysis being less precise than we might hope, because it is these estimated coefficients (and some of them being nonzero) that identify the difference in the variance of unobservables. However, the precision of the estimates is similar to the re-analysis, in Neumark (2012), of the Bertrand and Mullainathan (2004) data, despite the fact that in those data the skill variables appear to predict callbacks more strongly.

uniformly point to discrimination against older workers in hiring. In this study, we sought new evidence that improves on the existing evidence in a number of ways. Two of these are not fundamental, but seek to enrich the evidence on age discrimination in ways that might better inform policy. These include: focusing in part on workers near the normal retirement age, for whom increasing employment is a key policy goals; and accounting better for the richness of types of older job applicants, including those moving to lower skill jobs as part of the prevalent process of partial retirement or bridge employment.

The other two innovations are potentially more fundamental because they address potential biases in the existing literature. First, we explore whether past studies that gave older applicants limited experience in the job to which they were applying, to make them comparable to younger applicants, led to bias *towards* finding age discrimination. Second, we examine whether differences in the variances of unobservables bias the results (the "Heckman critique") – with a specific conjecture that the direction of bias is *against* finding age discrimination. If these two biases are potentially present in past research, one might conclude that the past research does not establish the existence of age discrimination in hiring.

To address these issues, we designed and conducted by far the largest-scale correspondence study of hiring discrimination that has been attempted, with about 40,000 job applications submitted. The very large sample increases the generalizability of the results, and also permits analysis of many more refined questions we ask. In addition to adding features that let us address the questions just described, we also implemented many study design elements that were grounded in empirical observations on job applicants and the job application process, to increase the credibility of our findings.

We have a number of central findings to report, organized in terms of the issues posed above, as well as other findings that emerged. First, most of our evidence indicates that discrimination against job applicants near the retirement age (64-66) is stronger than for middle-aged workers (49-51). The latter group is closer to the age range used in most existing work, while the former group is more relevant to policy changes intended to encourage older people to work longer – like past and proposed Social Security reforms.

Second, the evidence is robust to the "bridge job" properties of the resumes of either middle-aged or older workers. There does not appear, for example, to be worse age discrimination for older workers looking to move to lower-responsibility, less demanding jobs, on the way to retirement. From the point of view of extending work lives of older people, in which bridge jobs are likely to play an important role, this might be viewed as somewhat encouraging. That must be tempered, however, by evidence of age discrimination we find, which suggests barriers to some older workers getting hired in new jobs generally.

Third, in most cases there is not much indication that using resumes that limit older applicants' experience, to make them more comparable to younger applicants, biases these kinds of studies towards finding evidence of age discrimination. This is useful knowledge with regard to how to interpret the entire body of evidence from this and previous studies, which – with regard to this issue – might therefore be regarded as providing consistent and valid evidence of age discrimination in hiring. We do note, though, that there is one exception – for the janitor job applications included in our study, which were for men only. In this case, evidence of age discrimination emerges only for the low experience applications. And in this particular study this exception is important because janitor jobs are the one case where the combined evidence from our other analyses points to consistent and strong evidence of age discrimination against men.

On the more general issue of how to use experience in designing resumes for age discrimination AC studies, we do not regard the issue of whether using low experience generates bias against hiring or calling back older workers as completely settled. Given that janitors are a small share of our total number of applications, we tentatively conclude that the specification of a lower level of experience is not generally problematic. Nonetheless, we have argued that comparisons of high experience older applicants to low experience younger applicants is the more policy-relevant question, and probably the more appropriate legal question, so we would advocate that future studies use this design.

Fourth, our analysis of the potential role of differences in the variances of unobservables, which can generate bias in estimated effects of discrimination, generates some variation in results. In particular, for the female job applicants we study we find either robust evidence of age discrimination, or stronger evidence than in the estimates that do not correct for this problem (consistent with bias against finding age discrimination, as conjectured). On the other hand, for men things are more ambiguous, with the evidence of age discrimination largely evaporating for one occupation (sales), while strengthening to some extent for security and janitors. However, in this analysis, as well, for janitors the evidence of age discrimination seems to stem from low-experience resumes.

Fifth, there is a hint of evidence that the age discrimination that our study detects is weaker in states with stronger age discrimination laws. This is consistent with other evidence that stronger age discrimination laws boost hiring of older workers (Neumark and Song, 2013), but clearly evidence is needed from far more states than the 11 in our study.

Finally, we have two things to say about the differences in the evidence on discrimination faced by older men versus older women. First, for the one occupation where we study both men and women – sales – we find considerably stronger evidence of discrimination against older women than older men; indeed if one emphasizes the evidence from the unobservables correction, there is evidence of age discrimination only for women. Second, more generally across the many analyses we present, the evidence of age discrimination against older women is strong and robust, while the evidence for older men is less clear. We only consistently find evidence of age discrimination for one of three occupations in which we study men (security), and in this case the evidence is not statistically strong.

We might, therefore, conclude that the really strong evidence from our study establishes that it is harder for older female workers to find jobs. In contrast, consideration of the biases we take up in this paper leads to results that appear to undermine the uniform evidence from past AC studies that there is age discrimination in hiring against older men.[73]

This, in turn, raises the question of why older women might suffer from age discrimination more than older men do. There are two related possibilities. One is that age discrimination laws do less to protect older women who may suffer from both age and sex discrimination. Because the law that protects women (Title VII of the Civil Rights Act) is separate from the law that protects older workers (the ADEA), "intersectional" claims of age discrimination against older women are difficult to bring before the courts (Song, 2013; Day, 2014). Second, older women may in fact experience more discrimination than older men, because physical appearance matters more for women (Jackson, 1992) and because age detracts more from

[73] Note that the one exception to finding evidence of age discrimination, in Table 1, was for women.

physical appearance for women than for men (Berman et al., 1981).

We do not know whether these factors explain our evidence. But the stronger and more robust evidence of age discrimination against older women than older men suggests that researchers should do more to see if this finding, itself, is robust, to understand the sources of these differences, and potentially to point out how policy efforts to extend working lives might productively focus on reducing discriminatory barriers to older women's employment.

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Table 1: Evidence from Past Audit/Correspondence Studies of Age Discrimination

Study	Type	Occupation	Ages	Total number of tests	Tests with ≥ 1 positive response	Older applicant favored, cases with at least one positive outcome (%/no.)	Younger applicant favored, cases with at least one positive outcome (%/no.)	Net discrimination
Bendick et al. (1997)	Correspondence	Management information systems (men only); executive secretary (women only); writer/editor	57 vs. 32	775	79	16.5% (13)	43% (34)	26.5%*
Riach and Rich (2006)	Correspondence (France)	Waitstaff (men only)	47 vs. 27	345	31	19.4% (6)	77.4% (24)	58.1%*
Lahey (2008b)	Correspondence	Entry-level jobs (women only)	50/55/62 vs. 35/45	3,996	Not reported	MA: 3.8% FL: 4.3% (Note: overall interview rates)	MA: 5.3%* FL: 6.2% (Note: overall interview rates)	MA: 16.5 to 28.3% FL: 18.1 to 30.6%
Bendick et al. (1999)	Audit	Entry-level sales or management	57 vs. 32	102	Not reported	1% (Note: from set of 4 possible positive responses)	42.2% (Note: from set of 4 possible positive responses)	41.2%*
				102	Not reported	36.3% (Note: overall interview rate)	41.2% (Note: overall interview rate)	6.3 to 11.9%
Riach and Rich (2010)	Correspondence (England)	New graduates (women only)	39 vs. 21	420	47	4.3% (2)	63.8% (30)	59.6%*
		Waitstaff (men only)	47 vs. 27	470	80	28.8% (23)	57.5% (46)	28.8%*
		Retail managers (women only)	47 vs. 27	300	27	59.3% (16)	29.6% (8)	−29.6%*

Notes: "Net discrimination" is the difference between the percentage of cases in which the older applicant was favored relative to the younger applicant, and the percentage in which the younger applicant was favored relative to the older applicant. * indicates that the estimate is statistically significant at the 5% level or better, as reported in the study. "Total number of tests" refers to the number of jobs for which pairs of applications were submitted. See text for additional explanation.

Table 2: Shares of Recent Male Hires (< 5 Years of Tenure) in Age Group Relative to All Male Hires in Occupation, 100 Largest Occupations for Men, 2008 and 2012 CPS Tenure Supplements

	Age-specific recent hires/all recent hired in occupation			Age-specific recent hires/all recent hired in occupation	
Occupation	**Age 62 to 70**	**Age 28 to 32**	**Occupation**	**Age 62 to 70**	**Age 28 to 32**
Average across all occupations	10.79%	9.11%	Average across all occupations	10.79%	9.11%
Managers, all other	9.23%	5.82%	Machinists	11.60%	2.40%
Driver/sales workers and truck drivers	9.99%	4.52%	Education administrators	22.31%	3.69%
First-line supervisors/managers of retail sales workers	9.46%	6.83%	Computer programmers	5.25%	6.92%
Chief executives	14.77%	2.19%	Civil engineers	9.78%	5.47%
Carpenters	6.71%	8.37%	**Security guards and gaming surveillance officers**	16.32%	8.57%
First-line supervisors/managers of non-retail sales workers	12.81%	5.62%	Bus and truck mechanics and diesel engine specialists	11.39%	6.75%
Construction managers	8.53%	7.52%	First-line supervisors/managers of mechanics, installers, and repairers	8.26%	5.72%
Janitors and building cleaners	11.91%	2.64%	Property, real estate, and community association managers	15.49%	4.00%
Sales representatives, wholesale and manufacturing	10.40%	5.75%	Postal service mail carriers	6.89%	0.28%
First-line supervisors/managers of production and operating workers	6.15%	4.99%	Insurance sales agents	15.76%	5.74%
First-line supervisors/managers of construction trades and extraction workers	6.68%	8.54%	Real estate brokers and sales agents	19.76%	1.85%
Farmers and ranchers	16.61%	5.15%	Engineers, all other	7.37%	3.00%
Retail salespersons	11.31%	7.55%	Customer service representatives	9.41%	9.95%
Laborers and freight, stock, and material movers, hand	6.94%	7.04%	Bailiffs, correctional officers, and jailers	3.59%	4.83%
Lawyers, judges, magistrates, and other judicial workers	14.78%	1.68%	Bus drivers	23.01%	3.52%
General and operations managers	6.85%	9.60%	Heating, air conditioning, and refrigeration mechanics and installers	3.82%	9.71%
Electricians	7.78%	10.46%	Miscellaneous agricultural workers	12.86%	6.73%
Police and sheriff's patrol officers	1.07%	15.45%	Mechanical engineers	5.04%	2.82%
Secondary school teachers	5.06%	7.77%	Shipping, receiving, and traffic clerks	5.97%	4.90%
Farmers, ranchers, and other agricultural managers	11.81%	5.42%	Transportation, storage, and distribution managers	9.43%	3.19%
Automotive service technicians and mechanics	6.56%	7.60%	First-line supervisors/managers of landscaping, lawn service, and groundskeeping	4.15%	7.05%
Accountants and auditors	13.90%	6.84%	Sales representatives, services, all other	13.48%	6.58%
Construction laborers	6.46%	10.21%	**Cashiers**	12.62%	11.33%
Software developers, applications and systems software	2.76%	13.07%	Personal financial advisors	18.07%	5.27%
Production workers, all other	3.16%	7.29%	Human resources, training, and labor relations specialists	7.16%	3.50%
Postsecondary teachers	24.85%	0.51%	Metalworkers and plastic workers, all other	3.06%	2.84%
Physicians and surgeons	18.68%	2.26%	Radio and telecommunications equipment installers and repairers	4.47%	7.42%
Grounds maintenance workers	9.56%	7.08%	Heavy vehicle and mobile equipment service technicians and mechanics	5.30%	7.60%
Elementary and middle school teachers	5.22%	9.53%	Other teachers and instructors	16.29%	3.68%
Computer scientists and systems analysts	7.43%	8.41%	Printing press operators	13.63%	4.63%
First-line supervisors/managers of office and administrative support workers	4.62%	10.06%	Computer, automated teller, and office machine repairers	8.74%	1.48%
Computer and information systems managers	3.37%	3.37%	Industrial production managers	5.98%	2.52%

	Age-specific recent hires/all recent hired in occupation			Age-specific recent hires/all recent hired in occupation	
Occupation	**Age 62 to 70**	**Age 28 to 32**	**Occupation**	**Age 62 to 70**	**Age 28 to 32**
Industrial and refractory machinery mechanics	7.78%	2.91%	Computer support specialists	6.08%	10.47%
Food service managers	6.84%	10.02%	Registered nurses	9.31%	9.28%
Marketing and sales managers	3.79%	7.07%	Securities, commodities, and financial services sales agents	18.93%	4.07%
Miscellaneous assemblers and fabricators	10.05%	5.13%	Taxi drivers and chauffeurs	11.21%	4.29%
Stock clerks and order fillers	6.12%	8.87%	Butchers and other meat, poultry, and fish processing workers	13.29%	6.51%
Pipelayers, plumbers, pipefitters, and steamfitters	6.16%	7.06%	Telecommunications line installers and repairers	1.71%	9.91%
Financial managers	6.79%	11.98%	Dentists	10.95%	3.74%
Cooks	3.59%	12.61%	First-line supervisors/managers of police and detectives	6.02%	5.30%
Maintenance and repair workers, general	13.06%	4.66%	Carpet, floor, and tile installers and finishers	6.24%	4.06%
Welding, soldering, and brazing workers	6.70%	8.61%	Medical and health services managers	5.26%	8.05%
Engineering technicians, except drafters	7.71%	2.01%	First-line supervisors/managers of housekeeping and janitorial workers	5.39%	3.49%
Electrical and electronic engineers	13.16%	3.50%	First-line supervisors/managers of food preparation and serving workers	5.63%	5.03%
Clergy	18.58%	2.97%	Supervisors, transportation and material moving workers	2.24%	13.95%
Industrial truck and tractor operators	4.64%	11.40%	Counselors	13.53%	6.65%
Painters, construction and maintenance	7.67%	5.33%	Aircraft pilots and flight engineers	6.48%	2.46%
Management analysts	18.17%	4.29%	Industrial engineers, including health and safety	13.15%	7.27%
Inspectors, testers, sorters, samplers, and weighers	7.16%	9.01%	Aircraft mechanics and service technicians	7.92%	5.45%
Operating engineers and other construction equipment operators	7.29%	10.16%	Other installation, maintenance, and repair workers	4.71%	4.20%

Notes: The table shows the 100 largest Census occupations for men, ranked by occupation size. Some occupations had empty cells for one or both age groups not in the top 100, and hence are not shown in this table. Occupations that would have been in the top 100 but had an empty cell include firefighters, designers, detectives and criminal investigators, and waiters and waitresses. Occupations in boldface are used in study.

Table 3: Shares of Recent Female Hires (< 5 Years of Tenure) in Age Group Relative to All Female Hires in Occupation, 100 Largest Occupations, 2008 and 2012 CPS Tenure Supplements

	Age-specific recent hires/all recent hires in occupation			Age-specific recent hires/all recent hires in occupation	
Occupation	**Age 62-70**	**Age 28-32**	**Occupation**	**Age 62-70**	**Age 28-32**
Average across all occupations	10.98%	7.48%	Average across all occupations	10.98%	7.48%
Secretaries and administrative assistants	13.18%	3.39%	First-line supervisors/managers of non-retail sales workers	8.20%	3.65%
Elementary and middle school teachers	6.29%	7.33%	Paralegals and legal assistants	3.52%	6.39%
Registered nurses	7.97%	6.80%	**File clerks**	16.00%	5.86%
Bookkeeping, accounting, and auditing clerks	14.17%	3.36%	Inspectors, testers, sorters, samplers, and weighers	6.84%	3.42%
First-line supervisors/managers of retail sales workers	9.27%	10.11%	Computer scientists and systems analysts	5.59%	6.12%
First-line supervisors/managers of office and administrative support workers	9.91%	5.90%	First-line supervisors/managers of food preparation and serving workers	7.65%	3.20%
Managers, all other	7.87%	3.64%	Management analysts	3.04%	6.19%
Accountants and auditors	8.40%	7.54%	Farmers and ranchers	26.19%	3.25%
Nursing, psychiatric, and home health aides	12.68%	5.37%	Data entry keyers	8.20%	10.44%
Secondary school teachers	9.01%	7.63%	Insurance claims and policy processing clerks	4.51%	7.11%
Maids and housekeeping cleaners	13.01%	2.28%	Production workers, all other	6.30%	7.79%
Teacher assistants	9.29%	4.99%	Loan counselors and officers	3.48%	20.97%
Customer service representatives	3.90%	7.16%	Sales representatives, wholesale and manufacturing	5.32%	7.13%
Office clerks, general	10.70%	4.34%	Clinical laboratory technologists and technicians	9.65%	4.79%
Retail salespersons	12.35%	4.65%	Diagnostic related technologists and technicians	5.07%	2.65%
Receptionists and information clerks	14.55%	6.83%	Laborers and freight, stock, and material movers, hand	11.40%	3.60%
Cashiers	15.60%	4.59%	Librarians	16.38%	2.76%
Financial managers	6.45%	10.40%	Dental assistants	8.33%	7.19%
Education administrators	8.72%	4.42%	Purchasing agents, except wholesale, retail, and farm products	3.87%	2.90%
Child care workers	8.22%	3.03%	Insurance sales agents	12.17%	6.84%
Hairdressers, hairstylists, and cosmetologists	12.67%	8.34%	Social and community service managers	9.27%	5.21%
Chief executives	12.70%	1.57%	Dental hygienists	6.85%	6.76%
Postsecondary teachers	17.16%	2.32%	Software developers, applications and systems software	5.70%	3.78%
Preschool and kindergarten teachers	6.15%	7.67%	Miscellaneous community and social service specialists	5.92%	3.31%
Cooks	15.96%	5.48%	First-line supervisors/managers of production and operating workers	11.33%	2.04%
Office and administrative support workers, all other	9.51%	4.86%	Miscellaneous legal support workers	6.33%	9.92%
Medical assistants and other healthcare support occupations	7.97%	9.70%	Tellers	9.52%	16.12%
Social workers	8.58%	5.87%	Claims adjusters, appraisers, examiners, and investigators	9.12%	6.95%
Human resources, training, and labor relations specialists	6.53%	9.33%	Sewing machine operators	16.06%	3.82%
Janitors and building cleaners	14.50%	2.94%	Business operations specialists, all other	5.87%	12.76%
Medical and health services managers	11.83%	5.13%	Food preparation workers	21.67%	3.28%
Personal and home care aides	13.33%	7.14%	Human resources managers	0.37%	4.99%
Counselors	5.94%	8.38%	Postal service mail carriers	13.36%	3.68%

	Age-specific recent hires/all recent hires in occupation			Age-specific recent hires/all recent hires in occupation	
Occupation	**Age 62-70**	**Age 28-32**	**Occupation**	**Age 62-70**	**Age 28-32**
Real estate brokers and sales agents	24.60%	3.51%	Payroll and timekeeping clerks	2.42%	5.89%
Other teachers and instructors	10.86%	5.99%	Packers and packagers, hand	6.63%	3.59%
Billing and posting clerks and machine operators	8.55%	5.52%	Recreation and fitness workers	13.37%	13.38%
Licensed practical and licensed vocational nurses	7.38%	2.98%	Sales representatives, services, all other	3.06%	7.76%
Food service managers	5.06%	6.25%	Psychologists	21.46%	3.87%
Bus drivers	10.97%	1.88%	Production, planning, and expediting clerks	7.63%	4.06%
Special education teachers	1.99%	5.22%	Shipping, receiving, and traffic clerks	6.16%	6.95%
Lawyers, judges, magistrates, and other judicial workers	10.50%	7.78%	Transportation attendants	17.76%	18.31%
Waiters and waitresses	9.77%	12.58%	Driver/sales workers and truck drivers	3.88%	14.27%
Farmers, ranchers, and other agricultural managers	19.79%	0.25%	Dispatchers	14.50%	7.66%
Marketing and sales managers	4.82%	12.81%	Computer support specialists	13.09%	10.76%
Designers	15.54%	5.41%	Supervisors, transportation and material moving workers	5.92%	0.75%
Property, real estate, and community association managers	16.21%	3.06%	Computer programmers	3.51%	3.15%
Health diagnosing and treating practitioner support technicians	5.13%	9.46%	Construction managers	11.25%	7.24%
General and operations managers	7.73%	7.81%	First-line supervisors/managers of housekeeping and janitorial workers	28.49%	11.68%
Miscellaneous assemblers and fabricators	9.06%	9.72%	Artists and related workers	7.72%	6.09%
Stock clerks and order fillers	11.06%	6.39%	Computer and information systems managers	1.12%	4.65%

Notes: The table shows the 100 largest Census occupations for women, ranked by occupation size. Some occupations not in the top 100, and hence not shown in this table, had empty cells for one or both age groups. Occupations that would have been in the top 100 but had an empty cell include first-line supervisors/managers of personal service workers, pharmacists, physicians and surgeons, and postal service clerks. Occupations in boldface are used in study.

Table 4: Cities Used for Study, by Percent of Population Aged 62+, and Age Discrimination Laws

	Stronger laws (larger damages)	**Weaker laws (smaller damages)**
Much older cities	Sarasota (34.7%, 15)	
Older cities	Miami (19.6%, 15)	Pittsburgh (21.7%, 4)
Mixed cities	New York (16.9%, 4), Boston (17.4%, 6), Chicago (15.2%, 15)	Charlotte (15.1%, 15), Phoenix (16.3%, 15), Birmingham (17.6%, 20)
Younger cities	Houston (12.1%, 15), Los Angeles (14.3%, 5)	Salt Lake City (11.6%, 15)

Notes: The first number in parentheses is percent of population aged 62 and over, based on 2012 ACS five-year files. Second number in parentheses is firm-size cutoff for applicability of state age discrimination law. Nationally, 16.3% of the population is aged 62+.

Table 5: Examples of Zip Codes Selected for New York City CBSA and Associated Sub-Markets

Sub-market	Zip code	City	State	Population	% aged 25 - 34	% aged 60 to 64	% aged 65 to 74	Ratio 60-74 to 25-34	% black	Unemployment rate	Median family income
CBSA 20th percentile	All			6,497	7.6	4.4	5.6	0.65	1.5	5.5	62,576
CBSA median	All			17,505	12	5.5	7.1	1.09	4.5	7.4	98,046
CBSA 80th percentile	All			40,680	16.4	6.8	9.1	1.97	20.1	10.2	130,535
General, Manhattan, Queens, the Bronx	11358	Flushing	NY	39,143	14.5	5.8	7.6	0.92	2.5	9.1	80,428
	11364	Bayside	NY	35,106	13.5	6.2	8.1	1.06	2.5	7.1	81,657
	11379	Flushing	NY	35,680	11.9	7.1	8.8	1.34	1.7	6.4	84,139
Brooklyn	11209	Brooklyn	NY	72,434	17.2	5.3	6.9	0.71	2.7	8.4	72,535
	11228	Brooklyn	NY	43,396	14	6.3	9.3	1.11	1.9	9	70,667
	11379	Flushing	NY	35,680	11.9	7.1	8.8	1.34	1.7	6.4	84,139
Staten Island	10306	Staten Island	NY	55,692	11.8	6.2	8.3	1.23	3.7	7.3	92,114
	10307	Staten Island	NY	14,418	10.8	4.8	7	1.09	1.1	6.2	101,442
	10314	Staten Island	NY	87,921	11.8	6.7	8.1	1.25	4.2	6.2	91,470
New Jersey	07605	Leonia	NJ	8,998	7.8	6.2	8.1	1.83	4.3	5.5	98,629
	07070	Rutherford	NJ	18,084	12.7	5.7	5.8	0.91	5	7.8	100,278
	07110	Nutley	NJ	28,311	13.1	6.7	7.9	1.11	3.7	8.8	102,049

Notes: Source is the American Community Survey Demographic and Housing Estimates (2012, 5-year estimates), at the zip code level.

Table 6: Selected Phone Area Codes

Metro area	Area code	Year created	Geographical area
Birmingham, AL	205	1947	Birmingham and portions of northwestern Alabama
Boston, MA	857	2001	Greater Boston (approximately the area within I-95)
Charlotte, NC	980	2001	Charlotte and all or part of the 12 surrounding counties in North Carolina
Chicago, IL	773	1996	Chicago excluding the downtown core
Houston, TX	832	1999	Greater Houston area
Los Angeles, CA	323	1998	Central Los Angeles, excluding Downtown, Koreatown, Echo Park, and Chinatown
Miami, FL	786	1998	Miami-Dade and Monroe Counties
New York, NY	347	1999	The Bronx, Queens, Brooklyn, Staten Island, Marble Hill (Manhattan)
Phoenix, AZ	602	1947	Most of Phoenix
	480	1999	East Valley
	623	1999	West Valley
Pittsburgh, PA	412	1947	Greater Pittsburgh Area
Salt Lake City, UT	801	1947	Davis, Morgan, Salt Lake, Utah, and Weber counties
Sarasota, FL	941	1996	Manatee, Sarasota, and Charlotte counties

Table 7: Level of Matching of Callbacks

	Matched positive responses			
	Job id match	**Email/resume match**	**No responses**	**Total**
Voicemail	2,495	765	N.A.	3,260
Email	2,822	97	N.A.	2,919
All	5,317	862	34,044	40,223

Notes: There are 6,179 matched responses to 40,223 resumes that were sent out. Each response received from an employer was matched either to a unique job identifier or to the email and resume that was sent.

Table 8: Callback Rates by Age

		Young (29-31)	Middle (49-51)	Old (64-66)
A. Administration (N=24,350, female)				
Callback (%)	No	85.59	89.70	92.42
	Yes	14.41	10.30	7.58
Tests of independence (p-value)	Young vs. middle vs. old (0.00)	Young vs. middle (0.00)	Young vs. old (0.00)	Middle vs. old (0.00)
B. Sales (N=5,348, males)				
Callback (%)	No	79.11	78.89	85.30
	Yes	20.89	21.09	14.70
Tests of independence (p-value)	Young vs. middle vs. old (0.00)	Young vs. middle (0.90)	Young vs. old (0.00)	Middle vs. old (0.00)
C. Sales (N=4,707, females)				
Callback (%)	No	71.32	74.13	81.57
	Yes	28.68	25.87	18.43
Tests of independence (p-value)	Young vs. middle vs. old (0.00)	Young vs. middle (0.11)	Young vs. old (0.00)	Middle vs. old (0.00)
D. Security (N=4,138, male)				
Callback (%)	No	75.72	78.45	78.26
	Yes	24.28	21.55	21.74
Tests of independence (p-value)	Young vs. middle vs. old (0.16)	Young vs. middle (0.09)	Young vs. old (0.12)	Middle vs. old (0.93)
E. Janitors (N=1,680, male)				
Callback (%)	No	67.92	66.55	74.11
	Yes	32.08	33.45	25.89
Tests of independence (p-value)	Young vs. middle vs. old (0.01)	Young vs. middle (0.66)	Young vs. old (0.03)	Middle vs. old (0.01)
F. Combined (N=40,223)				
Callback (%)	No	81.31	84.60	87.864
	Yes	18.69	15.40	12.16
Tests of independence (p-value)	Young vs. middle vs. old (0.00)	Young vs. middle (0.00)	Young vs. old (0.00)	Middle vs. old (0.00)

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

Table 9: Multiple Callback Rates by Age

		Young (29-31)	Middle (49-51)	Old (64-66)
A. All applications (N=39,361)				
Callback (%)	No callback or single callback	97.61	98.10	98.69
	Multiple callbacks	2.39	1.90	1.31
Tests of independence (p-value)	Young vs. middle vs. old (0.00)	Young vs. middle (0.01)	Young vs. old (0.00)	Middle vs. old (0.00)
B. Applications with callbacks (N=5,317)				
Callback (%)	Single callback	85.47	86.15	87.94
	Multiple callbacks	14.53	13.85	12.06
Tests of independence (p-value)	Young vs. middle vs. old (0.10)	Young vs. middle (0.58)	Young vs. old (0.03)	Middle vs. old (0.14)

Notes: The p-values reported for the tests of independence are from Fisher's exact test (two-sided). Note that responses that could not be matched to a specific job, but only to a resume, are not included in this analysis.

Table 10: Probit Estimates for Callbacks by Age, Marginal Effects

	Administrative		Sales-Males		Sales-Females		Security		Janitor		Combined	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Callback estimates												
Middle (49-51)	-0.035*** (0.005)	-0.035*** (0.005)	-0.015 (0.013)	-0.017 (0.014)	-0.051*** (0.015)	-0.052*** (0.018)	-0.024 (0.016)	-0.023 (0.018)	0.015 (0.027)	0.025 (0.029)	-0.033*** (0.005)	-0.033*** (0.005)
Old (64-66)	-0.063*** (0.005)	-0.063*** (0.005)	-0.044*** (0.012)	-0.047*** (0.014)	-0.095*** (0.014)	-0.095*** (0.017)	-0.027 (0.017)	-0.029* (0.017)	-0.061** (0.027)	-0.055* (0.029)	-0.062*** (0.004)	-0.062*** (0.005)
Controls												
City, order, unemployed	X	X	X	X	X	X	X	X	X	X	X	X
Skills	X	X	X	X	X	X	X	X	X	X		
High-skill indicator											X	X
Resume features		X		X		X		X		X		X
Female											X	X
Occupation											X	X
Callback rate for young (29-31)	14.41		20.89		28.68		24.28		32.08		18.69	
N	24,350		5,348		4,707		4,138		1,680		40,223	
Clusters	1,052		544		513		893		694		3,694	

Notes: Marginal effects are reported, computed as the discrete change in the probability associated with the dummy variable, evaluating other variables at their means. Standard errors are computed based on clustering at the resume level. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Resume features include: template; email script; email format; script subject, opening, body, and signature; and file name format.

Table 11: Probit Estimates for Callbacks by Age and Resume Type, Marginal Effects, Full Controls

	Administrative	Sales-Males	Sales-Females	Security	Janitor	Combined
	(1)	(2)	(3)	(4)	(5)	(6)
Callback estimates						
Middle, commensurate experience (M_{HNB})	-0.029*** (0.006)	-0.029* (0.017)	-0.057** (0.022)	-0.028 (0.022)	0.015 (0.036)	-0.033*** (0.006)
Middle, commensurate experience, bridge application (M_{HB})	-0.027*** (0.006)	0.020 (0.019)	-0.064*** (0.022)	-0.046* (0.022)	…	-0.027** (0.006)
Middle, experience = young (M_L)	-0.041*** (0.006)	-0.037* (0.018)	-0.027 (0.025)	0.009 (0.028)	0.035 (0.034)	-0.035*** (0.006)
Old, commensurate experience (O_{HNB})	-0.058*** (0.005)	-0.048** (0.023)	-0.080*** (0.021)	-0.050 (0.027)	-0.017 (0.037)	-0.058*** (0.006)
Old, commensurate experience, bridge application, already bridged (O_{HB}^E)	-0.048*** (0.006)	-0.041** (0.017)	-0.100*** (0.022)	-0.026 (0.025)	…	-0.054*** (0.006)
Old, commensurate experience, bridge application (O_{HB}^L)	-0.055*** (0.006)	-0.052*** (0.017)	-0.074*** (0.019)	-0.030 (0.023)	…	-0.054*** (0.006)
Old, experience = young (O_L)	-0.057*** (0.005)	-0.038** (0.020)	-0.099*** (0.022)	-0.003 (0.035)	-0.094*** (0.031)	-0.062*** (0.006)
Tests of restrictions (p-value)						
A. Middle/old = young						
All middle resume types = 0 (middle=young)	0.00	0.02	0.02	0.15	0.57	0.00
All old resume types = 0 (old=young)	0.00	0.02	0.00	0.37	0.02	0.00
B. Resume types the same within age group						
All middle resume types equal	0.12	0.13	0.36	0.21	…	0.51
All old resume types equal	0.50	0.92	0.62	0.70	…	0.63
Joint (Table 13 restrictions)	0.26	0.10	0.60	0.48	0.12	0.70
C. Commensurate experience = low experience						
M_{HNB}=M_L	0.12	0.71	0.31	0.23	0.59	0.80
O_{HNB}=O_L	0.90	0.73	0.48	0.24	0.05	0.56
Joint	0.30	0.88	0.47	0.25	0.12	0.82
D. Bridge resumes = non-bridge resumes (all high experience)						
M_{HNB}=M_{HB}	0.71	0.02	0.80	0.53	…	0.38
O_{HNB}=O_{HB}^E	0.17	0.78	0.46	0.47	…	0.57
O_{HNB}=O_{HB}^L	0.69	0.89	0.76	0.51	…	0.61
Joint, older (O_{HNB}= O_{HB}^E, O_{HN}= O_{HB}^L)	0.38	0.85	0.54	0.74	…	0.83
Joint	0.56	0.10	0.73	0.80	…	0.76
Callback rate for young	14.41	20.89	28.68	24.28	32.08	18.69
N	24,350	5,348	4,707	4,138	1,680	40,223
Clusters	1,052	544	513	893	694	3,694

Notes: See notes to Table 10. Control variables correspond to second specification for each occupation (and combined occupations) in Table 10 (even-numbered columns). Standard errors are computed based on clustering at the resume level. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). There are not bridge resumes for janitors.

Table 12: Probit Estimates for Callbacks by Age, by City Demographics and State Age Discrimination Laws, All Occupations Combined, Marginal Effects

	City demographics			Damages		Firm-size cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Callback estimates	Young city	Middle-age city	Old city	Larger	ADEA	Lower	≥15
Middle	-0.039*** (0.010)	-0.028*** (0.006)	-0.027** (0.011)	-0.027*** (0.005)	-0.049*** (0.010)	-0.028*** (0.007)	-0.035*** (0.006)
Old	-0.067*** (0.010)	-0.054*** (0.006)	-0.067*** (0.011)	-0.058*** (0.005)	-0.074*** (0.010)	-0.056*** (0.007)	-0.065*** (0.006)
Cities (see Table 4)	Houston, Los Angeles, Salt Lake City	New York, Boston, Chicago, Charlotte, Phoenix, Birmingham	Sarasota, Miami, Pittsburgh	Sarasota, Miami, New York, Boston, Chicago, Houston, Los Angeles	Pittsburgh, Charlotte, Phoenix, Birmingham, Salt Lake City	New York, Boston, Los Angeles, Pittsburgh	Sarasota, Miami, Chicago, Houston, Charlotte, Phoenix, Birmingham, Salt Lake City
Hypothesis tests (p-value), pooled, age dummy interactions only/fully interactive models							
Middle x middle-age city=0	0.22/0.53						
Middle x old city=0	0.22/0.39						
Old x middle-age city=0	0.28/0.51						
Old x old city=0	0.68/0.95						
Middle x larger damages=0				0.48/0.32			
Old x larger damages=0				0.59/0.74			
Middle x smaller size cut-off=0						0.78/0.79	
Old x smaller size cut-off =0						0.91/0.85	
Joint	0.62/0.80			0.49/0.41		0.92/0.96	
Callback rate for young	20.52	17.05	20.16	16.60	24.28	17.31	19.54
Callback rate for middle	15.66	14.44	17.37	13.95	19.14	14.98	15.64
Callback rate for old	12.59	11.42	13.48	10.45	16.84	10.87	13.02
Tests of independence (p-value), young vs. middle vs. old	0.00	0.00	0.00	0.00	0.00	0.00	0.00
N	11,818	20,394	8,011	29,247	10,976	15,325	24,898

Notes: See notes to Table 10. Control variables correspond to second specification for combined occupations in Table 10 (column (12)). Standard errors are computed based on clustering at the resume level. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Hypothesis tests are from restricted models that add interactions between dummies for city demographics or state age discrimination laws and pool all observations.

Table 13: Probit Estimates for Callbacks by Age, Old vs. Young Only, Effects of Skills and Interactions of Old with Skills, Marginal Effects

	Admin.	Sales-males	Sales-females	Security	Janitor	Combined
	(1)	(2)	(3)	(4)	(5)	(6)
Old	-0.074*** (0.008)	-0.062*** (0.017)	-0.087*** (0.020)	-0.045* (0.025)	-0.036 (0.040)	-0.062*** (0.008)
Common skills						
Spanish	0.003 (0.010)	-0.000 (0.023)	-0.025 (0.035)	0.076* (0.045)	-0.025 (0.045)	-0.002 (0.011)
Spanish x Old	0.017 (0.018)	-0.036 (0.032)	0.021 (0.053)	0.038 (0.060)	-0.011 (0.078)	0.008 (0.017)
Grammar	-0.019 (0.009)	-0.018 (0.020)	-0.008 (0.031)	0.023 (0.034)	-0.007 (0.045)	-0.014 (0.009)
Grammar x Old	0.031** (0.016)	0.042 (0.035)	-0.017 (0.042)	-0.014 (0.046)	0.021 (0.077)	0.011 (0.015)
College	0.024** (0.010)	0.005 (0.022)	0.022 (0.027)	0.029 (0.038)	0.117** (0.050)	0.027*** (0.010)
College x Old	-0.023* (0.012)	-0.005 (0.030)	-0.019 (0.038)	-0.002 (0.048)	-0.066 (0.072)	-0.016 (0.013)
Employee of the month	0.003 (0.009)	0.034 (0.027)	-0.020 (0.028)	-0.073** (0.035)	-0.062 (0.044)	0.002 (0.010)
Employee of the month x Old	0.003 (0.014)	-0.019 (0.034)	0.042 (0.043)	0.021 (0.053)	0.070 (0.079)	-0.001 (0.014)
Volunteer	0.015* (0.009)	-0.017 (0.023)	0.009 (0.031)	-0.023 (0.038)	-0.087* (0.045)	0.011 (0.010)
Volunteer x Old	-0.013 (0.012)	0.037 (0.037)	-0.014 (0.046)	-0.023 (0.050)	0.080 (0.080)	-0.003 (0.014)
Occupation-specific skills	*Skill 1: computer Skill 2: words per minute*	*Skill 1: computer Skill 2: customer service*	*Skill 1: computer Skill 2: customer service*	*Skill 1: CPR Skill 2: license*	*Skill 1: technical skills Skill 2: certificate*	
Skill 1	-0.011 (0.010)	0.004 (0.023)	0.019 (0.029)	-0.057 (0.034)	0.137** (0.061)	
Skill 1 x Old	0.031** (0.016)	0.039 (0.038)	-0.015 (0.041)	0.089 (0.058)	-0.142** (0.063)	
Skill 2	0.021** (0.010)	0.008 (0.023)	0.003 (0.028)	0.060 (0.038)	-0.012 (0.059)	
Skill 2 x Old	-0.024* (0.012)	0.006 (0.036)	-0.030 (0.039)	-0.047 (0.044)	0.017 (0.083)	
N	16,449	3,570	3,609	2,746	1,118	27,492
Number of clusters	717	359	386	599	462	2,522

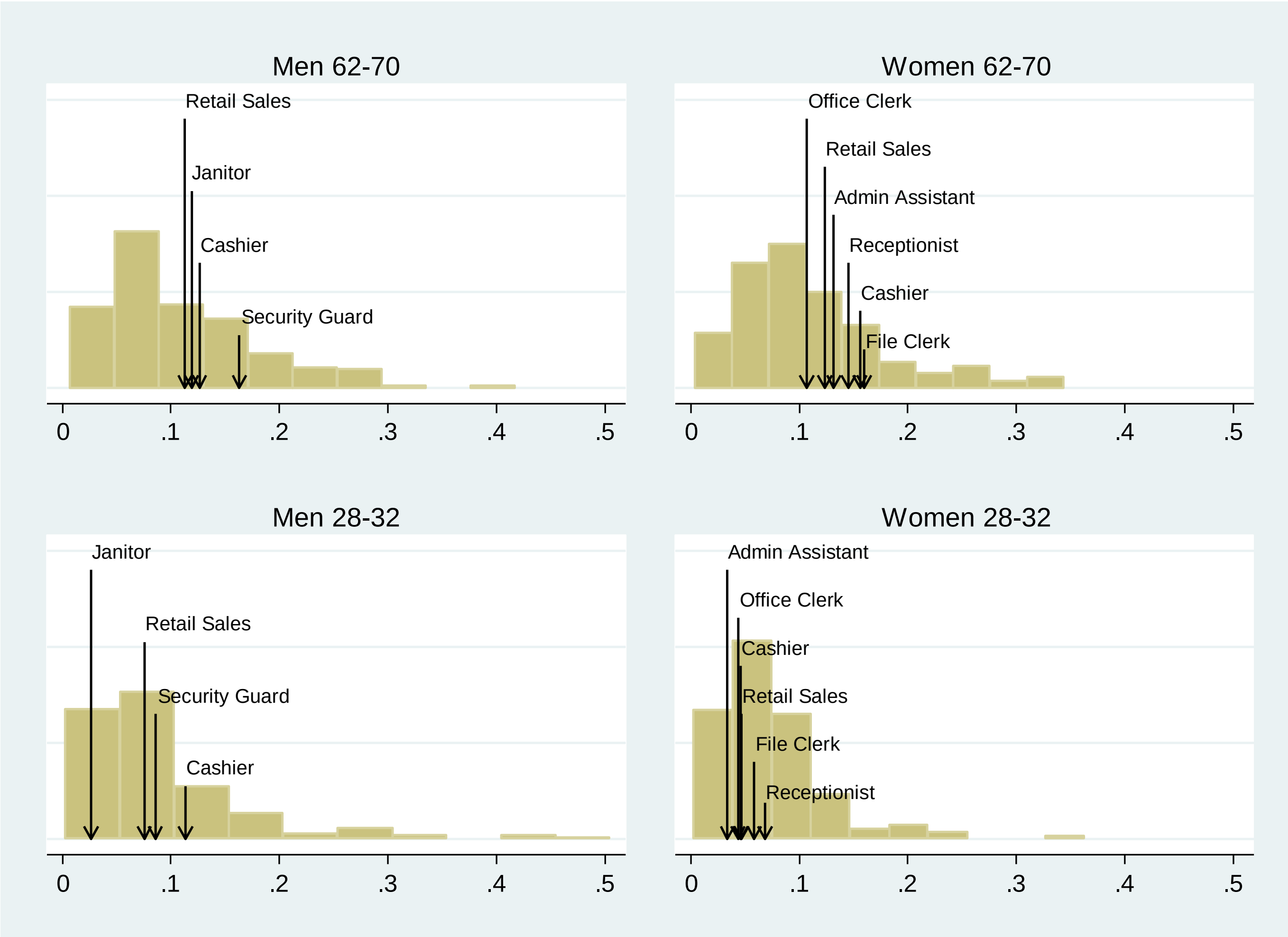
Notes: See notes to Table 10. Standard errors are computed based on clustering at the resume level. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Control variables correspond to first specification for each occupation in Table 10 (odd-numbered columns). Marginal effects are reported, computed as the discrete change in the probability associated with the dummy variable, evaluating other variables at their means. (See text for explanation.)

Table 14: Heteroscedastic Probit Estimates for Callbacks by Age, Old vs. Young Only (Corrects for Potential Biases from Difference in Variance of Unobservables)

	Administrative	**Sales-males**	**Sales-females**	**Security**	**Janitor**	**Combined**
	(1)	(2)	(3)	(4)	(5)	(6)
	All skills	All skills	All skills	All skills	All skills	5 common skills
A. Probit estimates						
Old (marginal)	-0.067*** (0.005)	-0.044*** (0.012)	-0.093*** (0.014)	-0.028 (0.017)	-0.062** (0.028)	-0.062*** (0.006)
B. Heteroscedastic probit estimates						
Old (marginal)	-0.068*** (0.006)	-0.049*** (0.012)	-0.074*** (0.015)	-0.022 (0.020)	-0.049* (0.029)	-0.060*** (0.006)
Overidentification test: ratios of coefficients on skills for old relative to young are equal (p-value, Wald test)	0.93	0.98	1.00	0.96	0.99	0.93
Standard deviation of unobservables, old/young	0.94	0.84	1.44	1.16	1.66	1.09
Test: ratio of standard deviations = 1 (p-value, Wald test)	0.61	0.23	0.07	0.35	0.35	0.41
Test: standard vs. heteroscedastic probit (p-value, log-likelihood test)	0.56	0.26	0.02	0.22	0.11	0.28
Old-level (marginal)	-0.054* (0.028)	-0.005 (0.039)	-0.161*** (0.034)	-0.058* (0.030)	-0.153* (0.082)	-0.080*** (0.022)
Old-variance (marginal)	-0.014 (0.029)	-0.043 (0.040)	0.086** (0.040)	0.036 (0.035)	0.104 (0.092)	0.020 (0.023)
N	16,449	3,570	3,609	2,746	1,118	27,492

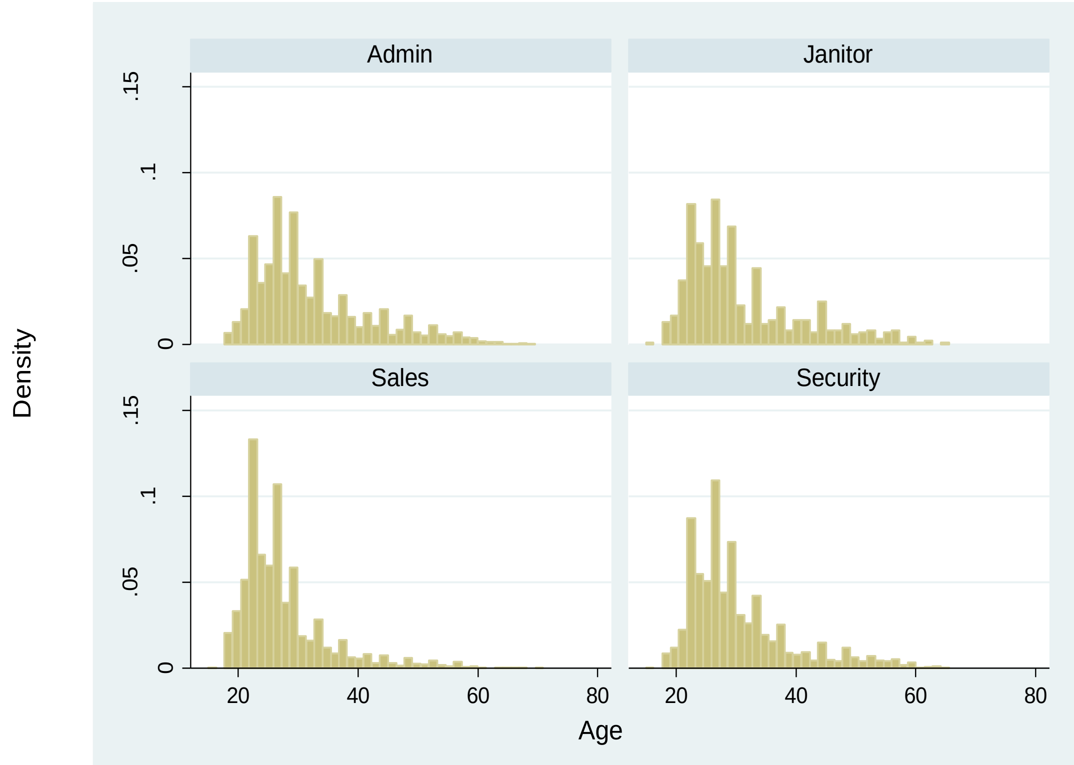
Notes: Marginal effects are reported, computed as the change in the probability associated with the dummy variable, using the continuous approximation, evaluating other variables at their means. Significantly different from zero at 1-percent level (***), 5-percent level (**) or 10-percent level (*). Control variables correspond to first specification for each occupation in Table 10 (odd-numbered columns), except that skill vector is as noted. Callback rates for young and old applicants are as in Table 8.

Figure 1: Histograms of Shares of Recent Hires (< 5 Years of Tenure) in Age Group Relative to All Hires of Same Sex in Occupation, Chosen Occupations and All Occupations for Men, 2008 and 2012 CPS Tenure Supplements



Notes: Histograms are created for all occupations with non-empty cells for both age groups. There are 203 for men and 150 for women.

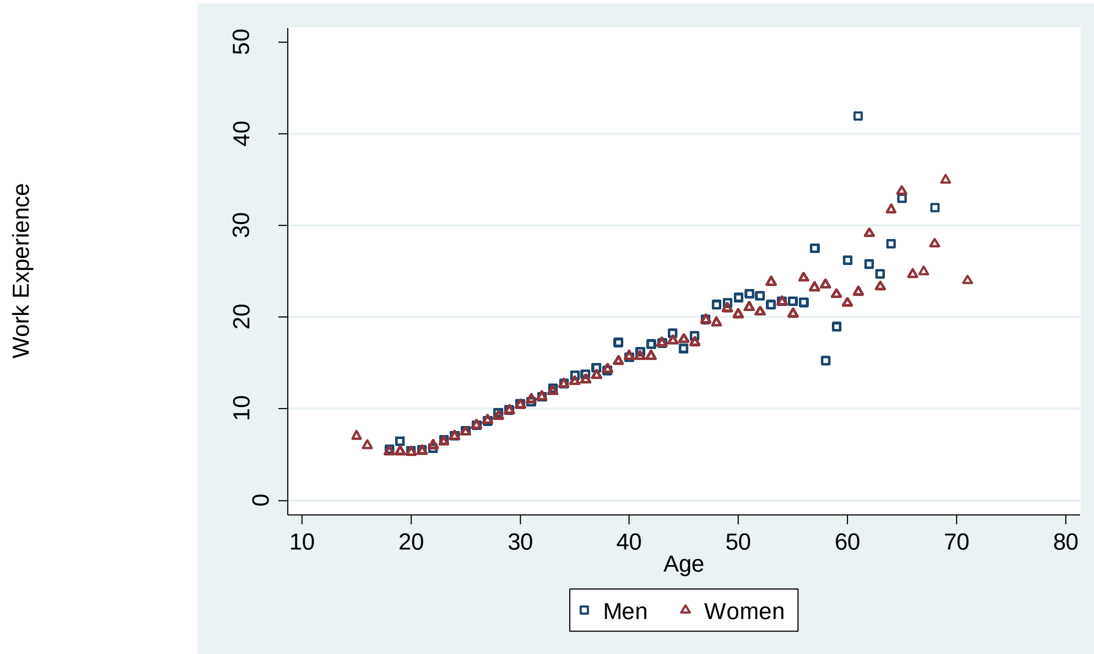
Figure 2: Histograms of Resumes by Age, Resume Website



Notes: Based on sample of resumes extracted from website, as described in text.

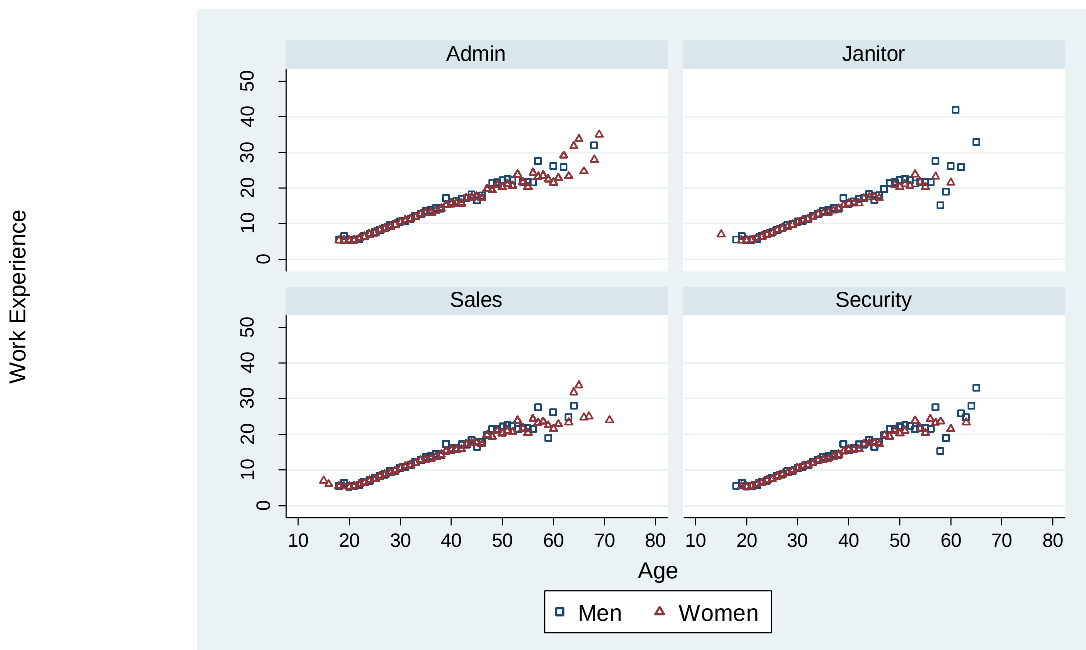
Figure 3: Age and Experience in Resume Sample

A. Overall averages by age



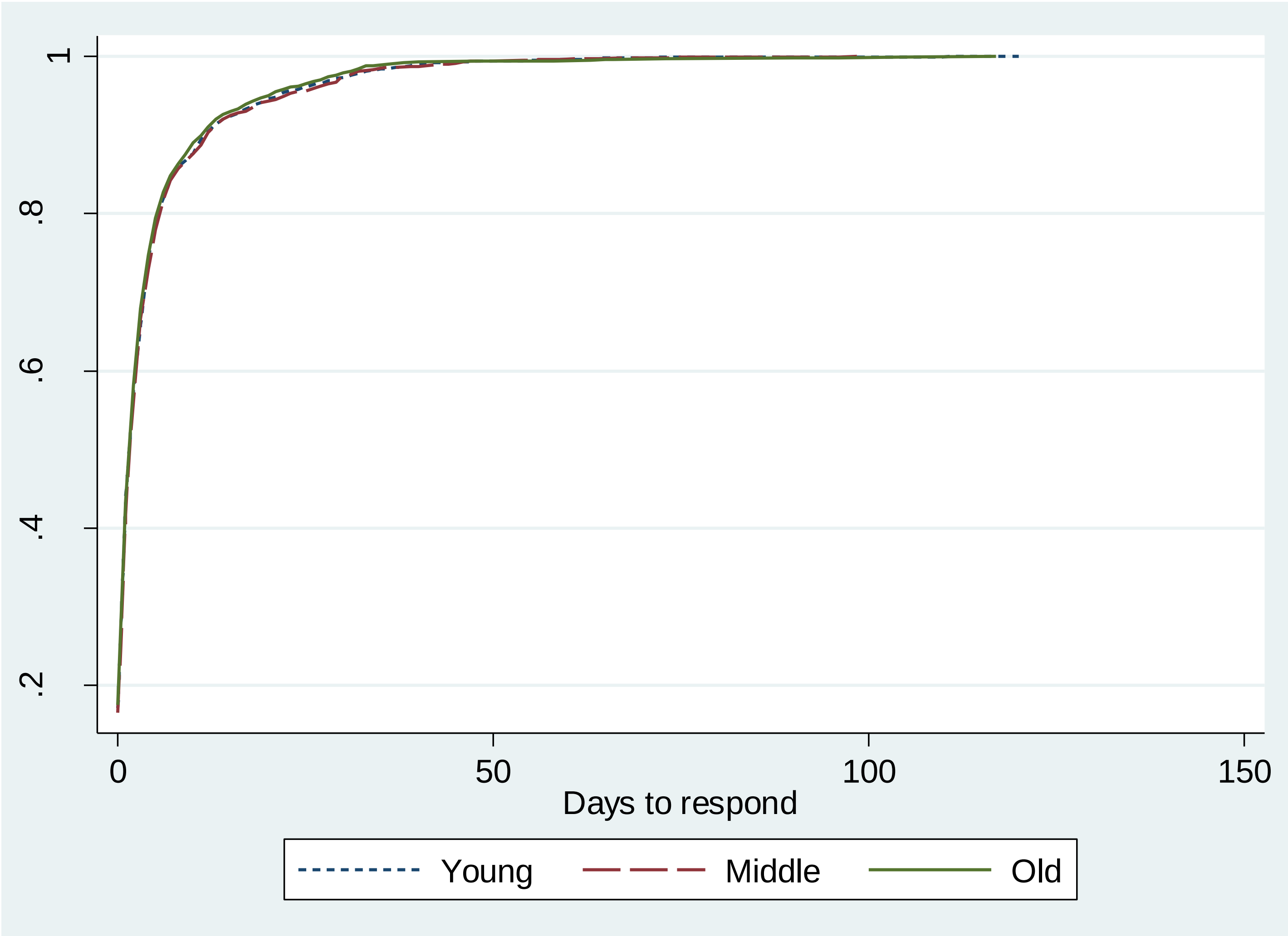
Notes: In the individual-level data, the correlation between age and computed experience is 0.77.

B. By job



Notes: Based on sample of resumes from a resume-posting website, as described in text.

Figure 4: Cumulative Distributions of Days to Respond for Young, Middle, and Old Resumes with Callbacks



Notes: Sample sizes are as in Table 9. Note that responses that could not be matched to a specific job, but only to a resume, are not included in this analysis.

Appendix Table A1: Median Hourly Wages for Low-Tenure (< 5 Years) Workers in Targeted Jobs, 2008 and 2012 CPS Tenure Supplements

	Men			Women		
Occupation	**Age 28-32**	**Age 48-52**	**Age 62-70**	**Age 28-32**	**Age 48-52**	**Age 62-70**
Retail salespersons and cashiers	12 [29]	10.1 [17]	9.62 [13]	9 [44]	8.87 [25]	9 [18]
Janitors and building cleaners	9 [11]	16.4 [11]	8.5 [6]	…	…	…
Security guards and gaming surveillance officers	9.5 [6]	10 [3]	10.75 [4]	…	…	…
Secretaries and administrative assistants; office clerks, general; receptionists and information clerks; and file clerks	…	…	…	14 [49]	13 [53]	12.5 [23]
Over all occupations, including those not shown	15 [828]	18 [444]	13.46 [142]	13.78 [805]	14.1 [520]	12 [163]

Notes: Cell sizes are shown in square brackets.

Appendix Table A2: Coding of Jobs for Construction of Bridge Resumes

	Retail sales	**Administrative assistant**	**Security guard**
1	Sales associate, cashier, customer service	Receptionist, front desk secretary, secretary	Security guard, security patrol
2	Customer service team leader		
3	Department team leader, shift supervisor	Administrative assistant	Security shift supervisor
4	Assistant manager, department manager		
5	Store manager	Office manager, executive assistant	Director of security

Notes: Each job used in the creation of the resumes was coded using this numeric scale. Using the codes, every resume was coded to create a level of responsibility over time. The three job histories (A, B, and C) for each type of resume were averaged together to create the average responsibility profile over time for the resume type. There was one type of young resume (Y), three types of middle-aged resumes (M_L, M_{HB}, and M_{HNB}), and four types of old resumes (three middle-aged resumes (with B and NB denoting bridge and non-bridge), and {O_L, O_{HB}^E, O_{HB}^L, and O_{HNB}). Appendix Figures A1 and A2 illustrate the responsibility profiles over time for the different middle-aged and older resumes.

Appendix Table A3: Job Search Methods of the Unemployed, 2014 CPS Monthly Files

	Age 28-32		Age 48-52		Age 62-70	
	Men	**Women**	**Men**	**Women**	**Men**	**Women**
Contacted employer directly/interview	52.7%	50.8%	53.8%	49.3%	45.6%	44.0%
Contacted public employment agency	21.1%	20.9%	25.0%	21.0%	15.1%	15.7%
Contacted private employment agency	10.2%	9.4%	12.5%	10.1%	11.9%	7.9%
Contacted friends or relatives	31.7%	25.6%	33.6%	30.7%	32.5%	29.5%
Contacted school/university employment center	4.2%	3.9%	3.5%	3.7%	3.9%	4.6%
Sent out resumes/filled out applications	55.5%	61.4%	53.1%	58.9%	46.3%	48.9%
Checked union/professional registers	4.2%	2.9%	6.6%	3.5%	7.0%	2.8%
Placed or answered ads	19.3%	15.2%	17.7%	19.3%	19.0%	18.1%
Other active	8.1%	6.7%	8.8%	9.6%	11.9%	12.1%
Looked at ads	31.8%	31.5%	32.0%	34.0%	30.0%	33.6%
Attended job training programs/courses	1.2%	2.4%	1.8%	2.0%	1.2%	2.0%
Other passive	3.0%	2.9%	3.5%	4.4%	5.9%	8.9%
Nothing	5.0%	3.3%	4.3%	4.8%	4.9%	4.6%
N	2,172	2,098	1,683	1,565	1,143	921

Notes: These estimates are derived from the Current Population Survey (basic monthly) for the year 2014. The sample includes all individuals who were unemployed and thus were asked about their job search methods. Population weights are used to generate estimates that are population representative. The proportions do not sum to one because respondents could list up to six job search methods.

Appendix Table A4: Skills on Resumes, by Occupation

	Searched for	**Admin**	**Janitor**	**Sales**	**Security**	**Total**
All	Bilingual, fluent	19%	12%	17%	13%	17%
All	Spanish	18%	10%	15%	10%	15%
Admin, Sales	Microsoft Office (Word, Excel, PowerPoint)	75%	33%	56%	47%	59%
Admin, Sales	QuickBooks	9%	0%	2%	1%	3%
Admin, Sales	POS software, inventory management	2%	2%	3%	1%	2%
Admin, Sales	Quick Learner	3%	3%	4%	3%	4%
Admin	Typing, WPM	29%	6%	15%	12%	18%
	Email, internet	13%	5%	9%	10%	10%
Sales	Communication	25%	18%	28%	23%	26%
Sales	Customer service	31%	22%	37%	26%	33%
Sales	Interpersonal	9%	6%	8%	9%	8%
	Other buzzwords	31%	29%	34%	28%	32%
Security	Security license, guard card	0%	2%	1%	10%	2%
Security	CPR, first aid	7%	4%	6%	13%	8%
Janitor	Certificate in/of Custodial Maintenance	0%	2%	0%	1%	0%
	Cleaning	1%	16%	3%	4%	3%
	Technical cleaning skills	0%	4%	1%	2%	1%
N		4,425	663	8,467	2,938	16,493

Notes: "Other buzzwords" includes: dependable, reliable, flexible, hardworking, attitude, team player, attention to detail, independent, and/or time management. "Cleaning" includes: cleaning, mopping, sweeping, trash, sanitizing, and/or housekeeping. "Technical cleaning skills" includes: plumbing, pest management, hazardous waste management, and/or knowledge in green cleaning/products. "Certificate in/of Custodial Maintenance" is defined as a certificate in janitorial or custodial work, or in any of the above technical cleaning skills.

Appendix Table A5: Zip Codes Used for Each City and Sub-Market

	ZIP	City	State
Birmingham:			
	35023	Hueytown	AL
	35094	Leeds	AL
	35118	Sylvan Springs	AL
Boston:			
General, Boston, Cambridge, Brookline	02152	Winthrop	MA
	02170	Quincy	MA
	02171	Quincy	MA
South Shore	02132	Boston	MA
	02170	Quincy	MA
	02171	Quincy	MA
North Shore, Northwest Suburbs	02152	Winthrop	MA
	01906	Saugus	MA
	01906	Saugus	MA
Western Suburbs	02132	Boston	MA
	02152	Winthrop	MA
	02026	Dedham	MA
Charlotte:			
	28105	Matthews	NC
	28120	Mount Holly	NC
	28210	Charlotte	NC
Chicago:			
General, Chicago, Northern Suburbs	60631	Chicago	IL
	60656	Chicago	IL
	60706	Norridge	IL
Southern Suburbs	60452	Oak Forest	IL
	60453	Oak Lawn	IL
	60655	Chicago	IL
Western Suburbs	60513	Brookfield	IL
	60516	Downers Grove	IL
	60148	Lombard	IL
Houston:			
	77009	Houston	TX
	77018	Houston	TX
	77055	Houston	TX
Los Angeles:			
General, Central Los Angeles	90027	Los Angeles	CA
	90039	Los Angeles	CA
	91202	Glendale	CA
Westside, South Bay	90501	Torrance	CA
	90504	Torrance	CA
	90066	Los Angeles	CA
San Fernando Valley	91505	Burbank	CA
	91324	Northridge	CA
	91356	Los Angeles	CA
San Gabriel Valley	90041	Los Angeles	CA
	91016	Monrovia	CA
	91754	Monterey Park	CA
Long Beach, Area Code 562	90241	Downey	CA
	90242	Downey	CA
	90650	Norwalk	CA

	ZIP	City	State
Miami:			
General, Miami, Dade County	33134	Coral Gables	FL
	33145	Miami	FL
	33166	Miami Springs	FL
Broward County	33014	Miami Lakes	FL
	33016	Hialeah	FL
	33055	Miami Gardens	FL
New York:			
General, Manhattan, Queens, the Bronx	11358	Flushing	NY
	11364	Bayside	NY
	11379	Flushing	NY
Brooklyn	11209	Brooklyn	NY
	11228	Brooklyn	NY
	11379	Flushing	NY
Staten Island	10306	Staten Island	NY
	10307	Staten Island	NY
	10310	Staten Island	NY
New Jersey	07605	Leonia	NJ
	07070	Rutherford	NJ
	07110	Nutley	NJ
Phoenix:			
General, Central Phoenix, South Phoenix	85283	Tempe	AZ
	85013	Phoenix	AZ
	85044	Phoenix	AZ
East Valley	85283	Tempe	AZ
	85206	Mesa	AZ
	85202	Mesa	AZ
West Valley	85323	Avondale	AZ
	85338	Goodyear	AZ
	85345	Peoria	AZ
North Phoenix	85032	Phoenix	AZ
	85023	Phoenix	AZ
	85053	Phoenix	AZ
Pittsburgh:			
	15209	Pittsburgh	PA
	15223	Pittsburgh	PA
	15234	Pittsburgh	PA
Salt Lake City:			
	84106	Salt Lake City	UT
	84107	Murray	UT
	84117	Salt Lake City	UT
Sarasota:			
	34231	Sarasota	FL
	34232	Sarasota	FL
	34239	Sarasota	FL

Notes: For six of the 12 cities (Boston, Chicago, Los Angeles, Miami, New York, and Phoenix), the job posting website contained "Sub-Markets" that covered different parts of the metropolitan area. When applying to jobs in each sub-market, we use addresses located within these markets. For job ads where it is unclear in which sub-market the job is located, the set of addresses for "General" are used. For Boston: North Shore, Northwest Suburbs, we use the same zip code twice (but still different street addresses) because there were not good alternatives after applying our filters.

Appendix Table A6: Reasons Applications Not Submitted in Response to Job Ads

Reason for dropping	**Admin.**	**Sales**	**Security**	**Janitor**	**All**
College requirement	8%	5%	1%	0%	5%
Already applied	6%	8%	10%	6%	7%
Spam	5%	4%	0%	0%	4%
Same company posting different jobs	3%	5%	4%	0%	4%
CPR (security)	2%	3%	4%	0%	2%
Outlook, QuickBooks, POS program, or given tying speed required	11%	5%	1%	0%	7%
Bilingual requirement	9%	6%	4%	2%	7%
Salary history/requirements, answer questions, submit references, security license number	9%	7%	6%	5%	8%
Need photo	4%	4%	3%	0%	4%
Apply in person, online, or phone call	11%	12%	30%	43%	16%
Temporary, seasonal, or internship	6%	6%	3%	1%	5%
Doesn't fit in job description (e.g., truck driver listed in sales)	11%	12%	16%	6%	11%
Duplicate posting	5%	7%	8%	27%	8%
Wrong market	4%	4%	2%	5%	4%
Managerial/supervisor	6%	10%	6%	3%	7%
Other	1%	2%	0%	0%	1%

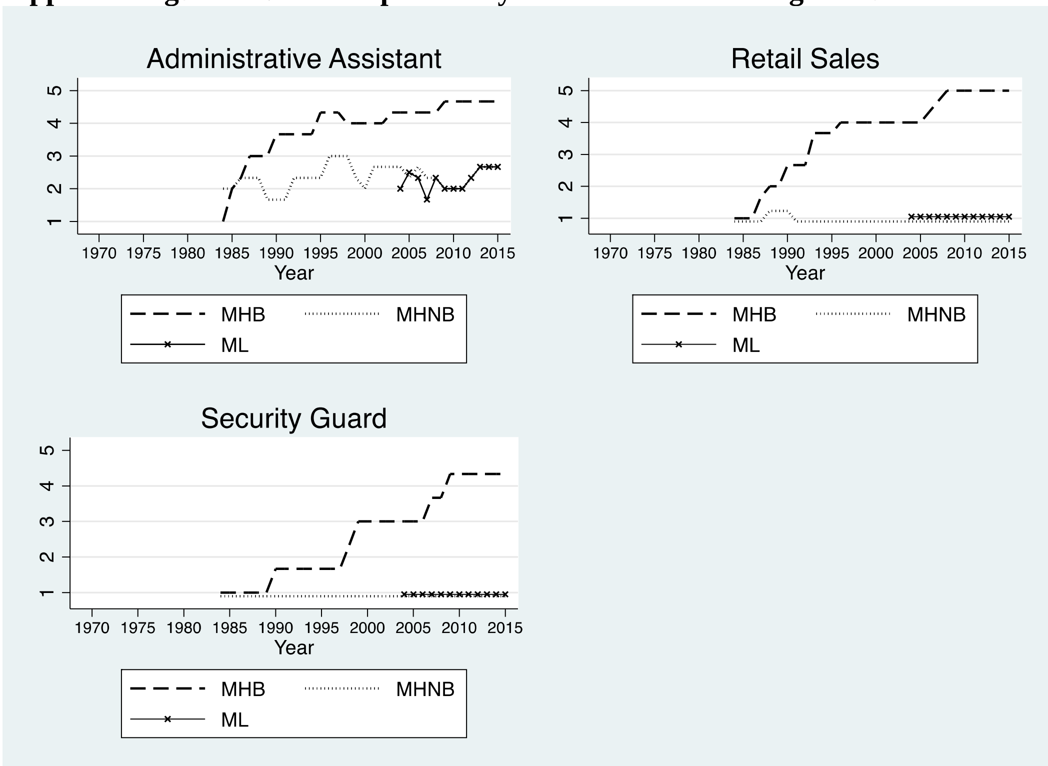
Notes: Research assistants did not apply for a job if it did not fit the description of the occupation, was not low skilled, asked for skills that were randomized onto the resumes, or if they required documents that we had not prepared. A job could be dropped for one or more reasons. The numbers reported in this table represent the share of total reasons for dropping, not the percentage of ads that were dropped for that reason. The "spam" ads noted here were identified by research assistants when reading the ads. Many more spam ads were identified after applying; see the text for discussion.

Appendix Table A7: Errors in Applying to Job Ads

Error	**Occurrences**	**Callbacks**
Sent resumes at wrong time	205	26
Sent only some resumes	12	2
Sent from the wrong part of the city	14	6
Applied using the wrong triplet	81	17
Sent resumes in the wrong order	730	148
Sent email with error in the script	4	1
Sent the wrong resume*	10	4
Sent resume from the wrong city*	8	0
Sent resume using the wrong email	6	0
Sent resume from the wrong occupation*	2	0
Sent email when should have applied in person*	6	0
Applied to the same job less than a month apart	29	2
Applied when the job required a skill*	6	2
Applied with men when it asked for only women*	3	0
Sent multiple applications to same job	9	3
Applied to a job that required a salary history*	3	0
Applied to an internship*	3	0
Applied when they required extra information*	3	0

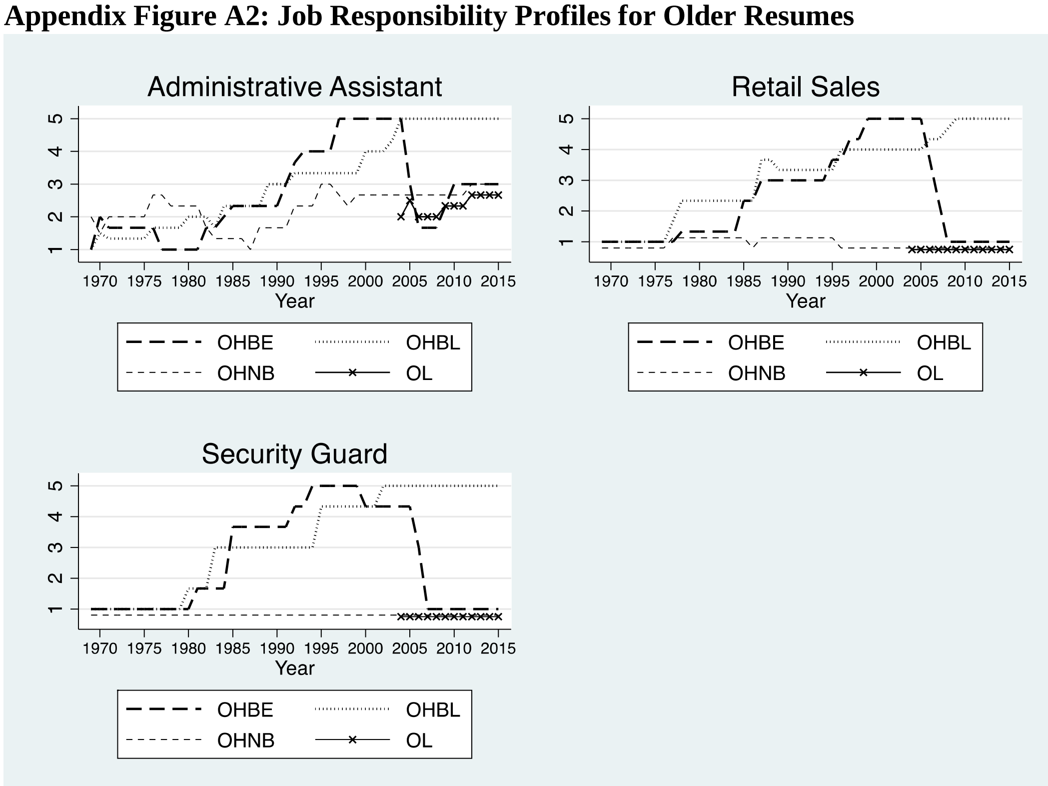
Notes: These errors were reported by research assistants or detected by monitoring. * indicates cases where the error violates the protocol in a way that could invalidate the data. Note that many, but not all, of these cases generate no callbacks.

Appendix Figure A1: Job Responsibility Profiles for Middle-Age Resumes



Notes: See explanation in notes to Appendix Table A2.

Appendix Figure A2: Job Responsibility Profiles for Older Resumes



Notes: See explanation in notes to Appendix Table A2.