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# AGE DISCRIMINATION ACROSS THE BUSINESS CYCLE

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**ABSTRACT**

We test whether age discrimination rises during recessions using two complementary analyses. Confidential EEOC microdata reveal that age-related firing and hiring charges rise by 3.3% and 1.6%, respectively, for each percentage point increase in a state-industry's monthly unemployment. Though the opportunity cost of filing falls, the fraction of meritorious claims increases—a sufficient condition for rising discrimination under plausible assumptions. Second, we repurpose data from hiring correspondence studies conducted across different cities and time periods during the recovery from the Great Recession. Each percentage point increase in local unemployment reduces the callback rate for older versus younger women by 15%.

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# 1 Introduction

Age discrimination in the workplace is a serious concern, especially since a large and growing fraction of the U.S. labor force is older (Maestas et al., 2016). In 1990, 11.9% percent of the labor force was age 55 or older, and this fraction has steadily increased to 23.4% percent by 2019 (Bureau of Labor Statistics). The consequences of age discrimination are sizeable, as late-in-life involuntary job loss affects not only financial well-being and retirement readiness (Coile and Levine, 2010), but also physical and mental health (Gallo et al., 2000). In the aftermath of the Great Recession older workers have had a particularly difficult time getting rehired, potentially resulting in a costly long-term reduction in the labor force (Johnson, 2012; Neumark and Button, 2014). Likewise, evidence from the Displaced Worker Survey shows a large negative age gradient in the likelihood of re-employment and earnings losses following displacement (Farber, 2017).

Against this backdrop, we examine how age discrimination varies with the business cycle.[1] This is a challenging question, as measuring age discrimination, and indeed any type of workplace discrimination, is difficult. Direct, objective measures of discrimination are scarce and so scholarship has tended to lean on indirect ones, such as wage and employment gaps. Such outcomes, while likely to be adversely impacted by increases in discrimination, could also be due to productivity or costs which differ across age groups (Scott et al., 1995). This measurement challenge is complicated further by the presence of turbulent labor market conditions such as those engendered by the Great Recession. While unemployment spells lasted longer and hiring rates fell further for older workers during the Great Recession (Johnson and Butrica, 2012), the attribution of these adverse outcomes to discrimination is called into question by early claiming of Social Security (Hutchens, 1999) and early retirement (Bosworth, 2012).[2]

Our paper overcomes these challenges using two complementary analyses. The first inves-

[1]Related papers on discrimination and the macroeconomy include Thurow (1975); Ashenfelter (1970); Freeman (1973); Shulman (1987); Neumark and Button (2014); Knepper (2018); Boulware and Kuttner (2019).

[2]Neumark and Button (2014) recognize this complication in their work: "Of course we do not actually know whether age discrimination was or is occurring. But we can ask whether these state protections reduced the adverse effects of the Great Recession on older workers relative to younger workers."

tigation leverages novel and direct measures of employment discrimination: individual-level ADEA (Age Discrimination in Employment Act) charges filed with the Equal Employment Opportunity Commission (EEOC). In concert with these confidential microdata, we use state×industry×month variation in exposure to the Great Recession to test whether employers discriminate more against older workers when the labor market is weak. For each one percentage point increase in the unemployment rate, we find the volume of ADEA firing and hiring charges increases by 3.3% and 1.6%, respectively. The increase in the number of ADEA firing charges does not arise from the mechanical increase that might be expected given a surge in discharged workers and a constant charge filing rate among those displaced. Controlling for the number of layoffs and discharges in a state-month leaves the headline estimates materially unchanged, which suggests that a larger *share* of displaced older workers file discrimination charges during downturns.[3]

While these results point to an increase in the *reported* level of discrimination, they do not distinguish genuine employer misconduct from elevated employee incentives to file a case. The reason is that as outside labor market opportunities recede, the opportunity cost of pursuing a claim will fall, and hence more marginal cases will be filed with the EEOC even if actual discrimination has remained constant.

We address this potential confound by taking advantage of the EEOC's determination of whether a discrimination case has "merit" – a decision which involves a lengthy follow-up investigation as needed. Using this proxy for claim quality, we find the fraction of ADEA discrimination cases with merit *rises* in response to deteriorating labor market conditions. For each one percentage point increase in the unemployment rate, the probability a case has merit increases by a statistically significant 0.7%. With some assumptions, a sufficient condition to conclude that actual (as opposed to merely reported) age discrimination increased during the Great Recession is that the merit rate did not fall.[4] The small and statistically

[3]Only a small fraction of discriminatory employment actions result in a formal EEOC charge, and these are likely to be the more serious instances of age discrimination. Our estimates capture the effects for these more serious cases, and therefore may not reflect unreported discrimination more broadly.

[4]Specifically, we assume that holding constant the level of actual discrimination, the quality of marginally added cases falls during a recession. We also require the merit variable to only vary with the business cycle due to changes in the true quality of cases. We note that EEOC charges capture just a small fraction of all

significant increase in the average quality of cases leads to the conclusion that actual age discrimination rises during economic downturns. The higher volume of observed cases is driven both by an increase in actual discrimination combined with an increase in the likelihood of filing.

Our finding that actual age discrimination rises when economic conditions are poor is robust to alternative specifications, including different measures of labor market tightness. We also show that compositional effects are unlikely to explain our findings using a variety of empirical exercises: damages awarded in case do not vary systematically with the business cycle, the inclusion of case characteristics do not affect our estimates, and the findings are not driven by a shift to industries with larger expected differences in worker productivity. The results additionally appear not to be driven by changes in firm size, the use of legal representation by claimants, or the level of resources the EEOC has at its disposal for investigating claims. We note however, that the high costs of retaining legal represenation may be especially prohibitive during economic downturns

The second analysis uses a correspondence study. A large literature has used this type of design to study levels of hiring discrimination for different groups.[5] More germane to our analysis, two papers have looked at how labor market tightness across occupations affects callback rates for ethnic minorities. Baert et al. (2015) finds that occupations with shorter vacancy durations discriminate more in Belgium. As the authors recognize, an alternative interpretation is that occupations with difficult to fill vacancies are less desirable and hence ethnic minorities face less competition from natives (Bulow and Summers, 1986). Carlsson et al. (2018) finds the opposite result using the native female callback rate as a measure of labor market tightness in Sweden; since this measure is potentially endogenous, they also

cases, and so it is possible that this second assumption could be violated. For example, more serious cases may require a lawyer, but the costs of legal representation may be prohibitive during economic downturns, which could deter filing behavior and push merit in the opposite direction.

[5] For a sampling of correspondence studies using race, ethnicity, and immigration status, see Bertrand and Mullainathan (2004); Carlsson and Rooth (2012); Edo et al. (2019); Oreopoulos (2011); Rooth (2010). For age, see Bendick et al. (1997, 1999); Riach and Rich (2010), and for age by sex, see Farber et al. (2017); Lahey (2008); Neumark et al. (2019a,b). For broader surveys on labor market discrimination, see Bertrand and Duflo (2017); Neumark (2018); Baert (2018).

use the vacancy-unemployment ratio by occupation and find marginally significant effects.[6]

Our focus is on how the level of age discrimination varies over the business cycle. Fortuitously, we are able to repurpose data from a pair of correspondence studies conducted by Farber et al. (2017) and Farber et al. (2019) in the aftermath of the Great Recession. These authors sent out fictitious resumes of women applying to administrative support positions to examine how applicant characteristics affect the callback rate. The resumes were assigned older versus younger ages (and other characteristics, depending on treatment) and circulated across a panel of 8 different cities from 2012-2017, generating ample across-city and across-time variability in unemployment. Exploiting this rich variation, our analysis shows that the age callback penalty grows considerably in the presence of anemic labor market conditions: each one percentage point increase in the local unemployment rate reduces the callback rate for older women by 1.5 percentage points (off a baseline 10.2% callback rate), relative to younger women. We interpret this as evidence that firms discriminate more as the number of hiring options increases.

The EEOC and repurposed field experiment analyses complement each other well, as each has unique strengths. The EEOC data cover the entire U.S. and capture age discrimination borne by real people during a recession. Moreover, our EEOC analysis allows us to study changes in discrimination on the firing margin, something which is not possible with a correspondence or experimental study. The firing margin is particularly noteworthy, both because it constitutes the bulk of these types of age discrimination cases (85% firing versus 15% hiring), and because losing a job is likely to impose greater immediate costs compared to not being hired. The correspondence analysis has the advantage of random assignment of applicant age to otherwise comparable profiles, and requires no assumptions about reporting behavior during a recession.

Taste-based discrimination could explain our findings for the firing margin, since firms will have learned about productivity for already employed workers (Altonji and Pierret, 2001). But employers may also be statistically discriminating based on expectations of

[6] Also related is the correspondence study of Kroft et al. (2013), which shows that in tight labor markets employers lean more heavily on the length of unemployment spells to infer worker quality.

future productivity related to age. On the hiring margin, both taste-based and statistical discrimination could play important roles. While we cannot empirically distinguish between the two, we note that both are illegal, and from the worker's perspective, equally harmful.

Taken together, our two analyses provide compelling evidence that age discrimination rises as labor markets deteriorate. As far as we know, this is the first direct evidence for age discrimination varying with the business cycle, both for the firing and hiring margins. Our results complement related work on how education, skill, and experience requirements increase during recessions (Modestino et al., 2016, 2019; Hershbein and Kahn, 2018). Our findings accord with theories which predict that as competition for workers wanes, discrimination should increase, and against theories which predict the opposite. A related insight is that the extent of measured discrimination depends crucially upon the labor market context during which that measurement happened. This is relevant when interpreting and comparing research documenting discrimination in different time periods or labor markets.[7]

The next section describes federal ADEA laws and the EEOC's role in investigating employer misconduct. Section 3 describes both the EEOC and correspondence study data, followed by Section 4, which details our empirical methodology. Section 5 presents the main results for our two complementary analyses. Section 6 provides an overview of different theories for age discrimination across the business cycle and Section 7 concludes.

# 2 The EEOC and Discrimination Reporting

## 2.1 Federal Age Discrimination in Employment Act

The Age Discrimination in Employment Act (ADEA) was codified into federal law in 1967 with the explicit purpose of protecting workers against workplace discrimination on the basis of age. Issues covered include practices involving firing, hiring, promotion, layoff, compensation, harassment, and so forth. The youngest age above which an employee is eligible for protection under the ADEA is 40.

[7] For example, this is salient when comparing estimates of discrimination against ethnic minorities (Doleac and Hansen, 2020; Riach and Rich, 2002), women (Egan et al., 2017; Goldin and Rouse, 2000; Helleseter et al., 2020; Kuhn and Shen, 2012; Neumark et al., 1996), or workers whose nationality or race differs from that of their manager (Åslund et al., 2014; Giuliano et al., 2009, 2011).

A primary purpose of the ADEA is to help counter the perception among employers that age adversely impacts ability. But research has found that taste-based discrimination may also play an important role in explaining ageism; Neumark et al. (2019a) conclude that the callback deficit for older workers is more consistent with a model of taste discrimination after ruling out many common sources of statistical discrimination. Relatedly, Burn et al. (2019) find that ageist language related to personality traits and physical attractiveness in job ads predicts discrimination for both older men and women. Importantly, though, the EEOC does not distinguish between taste-based and statistical discrimination in its enforcement activities; both are considered to be illegal.

There is, however, an important question on the extent to which the law does or does not differentiate between cases arising due to age discrimination versus ability and/or costs. In the majority opinion written for the 2005 Supreme Court Case of *Smith v. City of Jackson, Miss.* (544 U.S. 228 (2005)), Justice Sandra Day O'Connor unequivocally asserts the right of employers to lawfully take actions that are inimical to the class of older employees, so long as they are based on a "reasonable factor other than age." Specifically, she writes:

> *...the Wirtz Report correctly concluded that–unlike the classifications protected by Title VII–there often is a correlation between an individual's age and her ability to perform a job. Wirtz Report 2, 11-15. That is to be expected, for "physical ability generally declines with age," Murgia, supra, at 315, and in some cases, so does mental capacity, see Gregory v. Ashcroft, 501 U. S. 452, 472 (1991)....Beyond these performance-affecting factors, there is also the fact that many employment benefits, such as salary, vacation time, and so forth, increase as an employee gains experience and seniority. See, e.g., Finnegan v. Trans World Airlines, Inc., 967 F. 2d 1161, 1164 (CA7 1992) ("[V]irtually all elements of a standard compensation package are positively correlated with age"). Accordingly, many employer decisions that are intended to cut costs or respond to market forces will likely have a disproportionate effect on older workers. Given the myriad ways in which legitimate business practices can have a disparate impact on older workers, it is hardly surprising that Congress declined to subject employers to civil liability based solely on such effects....*

This ruling is important for interpreting our findings. The ADEA allows for disparate impact in the hiring and firing of older workers, in contrast to other protected classes such as race or sex where it would be illegal.[8] The implication is that an ADEA claim in which

[8] Disparate *treatment* is illegal under the ADEA, but disparate *impact* claims are not. As explained

older employees are fired based on cost or productivity considerations will not be considered meritorious, at least under the post-2005 interpretation of the law.[9] It is for this reason as well that our EEOC analysis sample begins in 2005.

## 2.2 Process for Filing and Resolving Discrimination Charges

The Equal Employment Opportunity Commission (EEOC) enforces prohibitions against age discrimination for private and public employers with over 20 employees (approximately 85% of all U.S. workers).[10] Figure 1 lays out the process for filing and resolving discrimination charges. Individuals are typically required to file a charge with the EEOC within 180 days of the alleged discriminatory action. The employer is then notified of the receipt of the charge within 10 days of the filing date. Normally the case is first referred to mediation, during which a neutral third party will attempt to assist the two parties in reaching a voluntary resolution. The average time to resolution for mediated cases is less than three months.

If instead either the employer or employee decides against mediation, the EEOC begins its investigation by first asking the employer to provide a written answer to the discrimination charge, after which the EEOC may hold interviews, gather documents, and interview witnesses. This process takes approximately 10 months on average. At any time during the investigation, the charging party and respondent may reach a negotiated settlement or the charging party may withdraw the case after receiving desired benefits from the employer. These are both considered to be "merit resolutions" by the EEOC, as they imply an outcome favorable to the charging party.

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by Rozycki and Sullivan (2010), in disparate treatment cases, "[t]he employee must prove through direct or circumstantial evidence that the discrimination was intentional. In contrast, a disparate-impact claim does not require proof of an intention to discriminate. Instead, showing that a facially neutral employment practice has a disproportionately adverse impact on a protected group states a prima facie case of unlawful disparate-impact discrimination."

[9]Section 4(f)(2) of the current version of the ADEA confirms that reasonable factors other than age are allowable.

[10]Certain states, counties, towns, and cities have implemented their own anti-discrimination laws. The organizations responsible for enforcing these laws, Fair Employment Practice Agencies (FEPAs), often have worksharing agreements with the EEOC so that whenever the charge's allegation is also covered by an EEO law, the FEPA will dual file the charge with the EEOC. To control for heterogeneity in the coverage and intensity of employment discrimination enforcement across states, we include state fixed effects and exclude FEPA charges from the analysis.

Following the investigation, the EEOC determines whether they have reasonable cause to believe that the alleged discrimination occurred according to the evidence collected. If no reasonable cause is determined, the charging party may still exercise the right to sue. If instead a reasonable cause is determined (i.e., the case has "merit"), the EEOC will again attempt to negotiate a voluntary agreement with the employer and charging party, resulting in either a successful or unsuccessful conciliation. If efforts to conciliate the charge are unsuccessful, the EEOC will then refer the case to its legal staff to determine its suitability for litigation.

Figure 1 displays the fraction of cases with different outcomes for ADEA hiring plus firing charges in our baseline sample. Combined merit resolutions are significantly rarer (17%) than are cases dismissed due to not having had reasonable cause (68%). The remaining category is administrative closures, which are charges for which the resolution cannot be determined (15%).[11] Only a small number (0.3%) of all initially filed charges are litigated.

In 2005, the Gallup poll conducted a national survey with input from the EEOC in conjunction with the EEOC's 40th anniversary. The poll sampled adults who were either currently employed, actively seeking employment, or had been employed in the past 2 years. Among older workers, 4% reported being discriminated against in the workplace on the basis of age. Comparing this fraction with the fraction of older workers who file an EEOC age discrimination charge, we calculate that 0.53% of all alleged cases are reported to the EEOC.

# 3 Data

This paper combines a unique source of EEOC administrative data on charges with local area estimates of employment produced by the Bureau of Labor Statistics (BLS). This enables us to examine the relationship between the Great Recession and self-reported, but quality-validated, workplace discrimination charges filed by older employees. Additionally, we use data from a correspondence study of job applications for older and younger women to assess whether increases in local unemployment rates exacerbate age discrimination. In

[11] These include scenarios where the charging party fails to respond to the letter, the EEOC does not have jurisdiction, the charging party files a private lawsuit, there is a failure to locate the charging party, etc.

the following subsections, we describe the primary data elements subsumed in each.

## 3.1 EEOC Charge Data

Our EEOC analysis uses a total of 80,000 ADEA firing and hiring charges filed with the EEOC from 2005 through 2015 for which the industry code is available (approximately 8,000 annual filings).[12] These are further partitioned into 46 issues (e.g., Sexual Harassment, Discharge, Hiring, etc.) and 80 bases (e.g., Sex-Female, Race-Black, Age, etc.). Each observation in the EEOC dataset corresponds to a particular charge, which may include multiple claims of types of discrimination. The average number of claims per charge is just over 4 for ADEA firing charges and just over 3 for ADEA hiring charges. For the purposes of this paper, we classify an observation as a firing charge if one of the issues was coded as "discharge" or "layoff" and as a hiring charge if one of the issues was "hiring." These charge types account for approximately 73% of all ADEA filings. Appendix Table A1 lists the most common basis categories in addition to age, and the most common additional issue categories. The table shows that retaliation and disability claims are commonly included bases in ADEA firing and hiring charges, and that terms and conditions and harassment are commonly included as additional issues.[13]

Appendix Table A1 also reports selected characteristics of the workers and firms. For both firing and hiring, the average age of the charging party is 56 years old, with over half being white and roughly one-quarter black. Interestingly, the gender composition skews more male in hiring charges. The share of plaintiffs retaining private legal counsel is far greater in ADEA cases of firing discrimination than in hiring discrimination (17% versus 7%), likely reflecting the monetary stakes being greater in grievances involving a discharge. Finally, the composition of private versus public firms differs by the type of charge filed; nine out of ten firms accused of firing discrimination are private, compared to just three-quarters of those accused of hiring discrimination.

[12] Data are unfortunately missing from October 2010 through September 2011 in the file we received from the EEOC; while the data do exist at the EEOC, they are not currently releasing any new data to researchers. We also drop 1.4% and 0.1% of observations for which sex and age are missing, respectively.

[13] Note that a charge including a discharge can also involve a hiring issue, for example if an employee feels they were wrongly fired and not rehired for another position.

Particularly important for our study, the EEOC data include information on how the charges are resolved, which results from a follow-up investigation conducted by the local EEO office. We transform the resolution into a binary variable that indicates if the agency determined the case to have had "merit," which serves as a useful proxy for the quality of the charge filed. The EEOC classifies as meritorious those cases resulting in settlements, withdrawals with benefits, and reasonable cause findings. Cases determined to have merit include both successful and unsuccessful final conciliation attempts. In general, merit resolutions are those charges for which the outcome is favorable to the charging party, either by way of monetary damages being exchanged or the EEOC concluding that the charge had reasonable cause following its lengthy investigation.

Table 1 provides a detailed breakdown of case resolutions for the ADEA firing and hiring charges separately. Notable differences between the two types of charges emerge; namely, ADEA firing cases are six times as common, 22% more likely (17.2% versus 14.1%) to have had merit, and generate larger monetary damages. This makes sense in light of the fact that hiring discrimination is notoriously difficult to prove.

There are several advantages of using the confidential EEOC microdata compared to the publicly available EEOC data,[14] which is nationally and annually aggregated, as others have used in various settings (e.g., Donohue and Siegelman, 1992, 2005; Griffin, 2016; Wakefield and Uggen, 2004). First, we can leverage information on the industry, state, and month of a charge to generate far richer variation in unemployment. We can further combine this information with a measure of the quality of each individual case. Moreover, we can separate out hiring and firing discrimination from "on-the-job" discrimination, for which there may be different reporting incentives during a recession.

### 3.2 State and Industry Unemployment Data

To measure local exposure to the Great Recession and subsequent recovery over the 2005-2015 period, we first calculate the number of unemployed individuals at the state-month level using the Local Area Unemployment Statistics series produced by the BLS. We also

[14]We received access to these data by submitting and receiving approval for a project proposal, though access to such data has tightened recently.

use variation in industry-specific exposure using unemployment statistics tabulated at the industry-month level by the BLS.[15] We then impute state-by-industry specific unemployment rates at the monthly level, the details of which appear in Section 4.2.

Appendix Figure A1 shows state variation in the unemployment rate at the height of the Great Recession (December 2009). There exists considerable cross-sectional variation: unemployment rates ranged from just over 4% in North Dakota to nearly 14% in Michigan. Appendix Figure A2 documents similarly wide variation across industries. Construction was shocked particularly hard, seeing a peak unemployment of 22%, whereas Education and Health Services were relatively insulated, with unemployment reaching only 6%. While not shown, there is also variation in the speed of recovery across both states and industries.

## 3.3 Correspondence Study Data

Our second analysis uses data from Farber et al. (2017) and Farber et al. (2019), who generated thousands of fictitious resumes and submitted them to 8 different cities over five time periods between 2012 and 2017. They explored how applicant characteristics (age, unemployment duration, recent unemployment, unemployment status, and low-level interim job) affected the callback rate for administrative support jobs. We repurpose their data to see how differences in local unemployment affect callback rates of older workers, relative to younger ones. For context, all of the artificial applicants were college-educated females with significant work experience.

Consistent with standard methods used in correspondence studies, for each city and in each application wave, paired applications were submitted with just one differing characteristic in Farber et al. (2017). Importantly, in rounds 1-3, either two younger [35, 36, 37, 40, 41, or 42] or two older [55, 56, 57, or 58] applications were sent to each job posting, and so variation in age is generated across rather than within job postings. Round 1 compared the newly unemployed with those who had been randomly assigned an unemployment spell of 4, 12, 24, or 52 weeks. Round 2 was identical except that each applicant was randomly assigned an unemployment duration of 0, 4, 12, 24, or 52 weeks. Round 3 precisely mirrored

[15] These estimates are, in turn, derived by combining data from the Current Population Survey (CPS), the Current Employment Statistics (CES) Survey, and state unemployment insurance (UI) systems.

round 2 except that a low-level interim job was assigned randomly at the application level, within matched-pairs. Finally, round 4 added the modification that each posting received an application from both a younger and older pair of workers.

In Farber et al. (2019), the authors sent out an additional round of resumes to the same set of cities a few years later to further study the role of recent employment and unemployment status by age; we label this as round 5. As in round 4 of their prior study, they send four applications to each job posting. But this time they used a broader range of ages, including very young individuals [22-23, 27-28], younger individuals [33-34, 42-43], and older individuals [51-52, 61-62]. While the age groupings are somewhat different across the two papers, the younger and older categories are roughly comparable. We therefore limit data from round 5 to job postings where there are at least two older applicants and at least 1 younger applicant. Hence, rounds 4 and 5 differ from the first three rounds in that older applicants directly compete against their younger counterparts.

Table 2 shows the number of applications submitted during each round of applications–3/2012-5/2012, 7/2012-9/2012, 11/2013-4/2014, 4/2014-8/2014, and 3/2017-8/2017–and for each of the 8 cities–Charlotte, Chicago, Dallas, Omaha, Pittsburgh, Portland (ME), Sacramento, and Tampa Bay.[16] As Farber et al. (2017) note, they intentionally picked 4 low unemployment cities and 4 high unemployment cities. This feature, along with the fact that each successive round of applications was submitted as labor markets continued to recover from the Great Recession, generates meaningful across-city and across-time variation, as illustrated in Figure 2. Unemployment rates can be twice as large for some cities versus others, even in the same time period. Moreover, unemployment rates fall by roughly half for most cities from the beginning to the end of our sample period.

# 4 Empirical Models

## 4.1 Distinguishing Actual versus Reported Discrimination

To ascertain whether discrimination varies with the business cycle, one needs to distinguish between actual and reported discrimination. Older workers who are fired from their jobs

[16] Portland, ME was omitted in round 3 of the survey.

(or not hired in the first place) have the option to pursue a legal claim of discrimination to receive both monetary payments and a potential restoration of their job. A worker's firing may have been linked to age discrimination, but it could also have been due to lower productivity/higher costs relative to retained workers and hence perfectly legal. How strong a case the worker has influences their chances of winning, and therefore their likelihood of filing an EEOC charge.

The worker's reporting threshold will also respond to labor market conditions, due to the time costs of filing a charge and the difficulty of finding new employment at a similar wage. During good economic times, the opportunity cost of filing a claim is higher and the potential benefits lower, as it is easier to find a new job quickly. Conversely, as the job market weakens, workers have an incentive to file more marginal claims. Hence we make the assumption that holding actual discrimination constant, more marginal claims will be reported during a weak labor market. This implies that for a given level of discrimination, the volume of charges will be higher while average claim quality will be lower during a recession.

In other words, during a recession the volume of hiring and firing charges could go up for two different reasons: an actual increase in discrimination and a reported increase in discrimination. Therefore, a jump in EEOC charges during a recession does not, by itself, necessarily imply a rise in actual discrimination. We can, however, arrive at such a conclusion if the quality of discrimination charges filed weakly increases. In this scenario, the rise in actual discrimination more than offsets the increased filing of weaker cases. This interpretation is valid so long as (i) holding constant the level of actual discrimination, the merit of marginally added cases falls during a recession, and (ii) the merit variable is an objective measure of case quality whose mean varies with the business cycle only due to changes in the true quality of cases. As robustness checks, later in the paper we explore and rule out several alternative explanations for why merit might change during a recession.

The benefit of our EEOC data is that it contains an independent measure of the quality of a case, which allows us to test whether the sufficient condition holds. Importantly, we are able to measure discrimination on the firing margin, something not possible with a correspondence study. The benefit of the correspondence data is that the issue of reporting does not even arise.

## 4.2 EEOC Charge Model

To identify the effect of unemployment on discrimination, we adapt the empirical model outlined in Maestas et al. (2021). That paper leverages variation in unemployment across states and over time to study disability insurance claims. We augment their formulation by including imputed measures of monthly state exposure by industry. Such an enhancement is possible because industry codes are included in the EEOC data and useful because of the rich variation in unemployment exhibited across industries during the Great Recession (see Appendix Figure A2).

To exploit this additional layer of heterogeneity, we impute monthly industry-specific unemployment at the state level using weighted national unemployment shares by industry. Specifically, we first recognize the number of unemployed individuals in each state $s$ in time period $t$, $U_{st}$, equals the weighted sum of industry-specific unemployment $j$ within that state:

$$U_{st} = \sum_{j=1}^{J} w_{jst} U_{jst} \tag{1}$$

where the subscripts denote industries and the weights, $w_{jst}$, represent each industry's share of total state employment in a period. These employment shares can be directly calculated at the state-month level from the Quarterly Census of Employment and Wages (QCEW).

To impute $U_{jst}$, we assume that industry $j$'s employment-weighted share of overall unemployment in state $s$ in period $t$ is equal to the corresponding employment-weighted share at the national level $n$. This assumption can be expressed as:

$$\frac{w_{jst}}{w_{jnt}} = \frac{U_{jnt}/U_{nt}}{U_{jst}/U_{st}} \tag{2}$$

This is a proportionality assumption relating employment shares to unemployment shares. This requires unemployment ratios at the state versus national level to be proportional to the correponding employment ratios. As all other variables are available from the QCEW or BLS's Current Employment Statistics, $U_{jst}$ can be imputed by solving as a function of these known quantities.

Armed with these monthly state-industry measures of labor market tightness, we estimate two different types of models, one for hiring and firing volume, and one for hiring and firing merit. Our baseline model for the volume regressions collapses the number of ADEA charges to the industry-state-month level and takes the following form:

$$volume_{jst} = \beta U_{jst} + \gamma_j + \alpha_s + \theta_t + \epsilon_{jst} \tag{3}$$

where $volume_{jst}$ is the number of ADEA hiring or firing discrimination reports filed with the EEOC in a state-industry-month and $\gamma_j$, $\alpha_s$, and $\theta_t$ are fixed effects for industry, state, and time. As in Maestas et al. (2021), we use the number unemployed as our measure instead of the unemployment rate to eliminate the confound introduced by industry-state-time differences in the size of the labor force on our outcome measures. The coefficient $\beta$ can easily be rescaled to estimate the effect of a one percentage point increase in the national unemployment rate on the change in the number of ADEA discrimination claims filed. In robustness checks, we explore alternative measures for labor market slackness, and find similar results. Our main estimates weight by the size of the state-industry-month labor force, so as to make the estimates representative of the entire U.S. population (see Solon et al., 2015); as we show in the Appendix, unweighted estimates yield similar findings.

This baseline model implicitly assumes that past changes in unemployment do not induce contemporaneous discrimination charges. As an alternative, we allow for the possibility that discrimination charge filing behavior responds not just to current movements in the unemployment rate but to previous changes as well. In particular, we implement a polynomial distributed lag model similar to that in Maestas et al. (2021):

$$volume_{jst} = \beta(L) U_{jst} + \gamma_j + \alpha_s + \theta_t + \epsilon_{jst} \tag{4}$$

where the function $\beta(L)$ is a lag polynomial that measures the effects of current and past values of unemployment on volume. The sum of the individual lag weights represents the cumulative number of discrimination reports induced by current and previous changes in unemployment. The appropriate polynomial degree and number of lags are chosen by minimizing the Akaike Information Criteria (AIC).

Our baseline model for the merit regressions uses noncollapsed data at the individual case level, so that we can control for relevant case characteristics. We model the dummy variable for whether case $i$ was determined to have merit as:

$$merit_{ijst} = \beta U_{jst} + \gamma_j + \alpha_s + \theta_t + \pi X_i + \epsilon_{ijst} \tag{5}$$

where $X_i$ is a vector of control variables associated with a case. We include the race, age, and sex of the charging party, along with the firm's sector (public or private).[17] Time fixed effects implicitly account for the potential impact of changing resource constraints at the EEOC on case success rates. In a robustness check, we additionally include controls for the type of claim being filed (e.g., sexual harassment, wages, suspension) and the class of protected employees involved (e.g., race, sex, disabled); the results are similar, suggesting compositional changes are not driving our results. We also consider polynomial distributed lag models for merit which are analogous to equation (4).

Though state-industry-time differences in local labor market conditions constitute the source of identifying variation, we conservatively cluster our standard errors at the state level. Since charges are filed with one of the 53 local EEO offices, this also allows for arbitrary correlation across the decisions reached by any one local office over time. Finally, note that there is no reason to weight the merit regressions, as they use individual-level data for the entire U.S.

## 4.3 Correspondence Study Model

For our correspondence study analysis, we estimate two types of regressions, one for rounds 1-3 and another when including rounds 1-5. As a reminder, rounds 1-3 sent either 2 older or 2 younger applications to each job posting. To estimate the effect of unemployment on callback rates for older female applicants using rounds 1-3 of the Farber et al. (2017) study, we use the following specification:

$$callback_{ict} = \beta_1 UR_{ct} + \beta_2 older_i + \beta_3 (older_i \times UR_{ct}) + \alpha_c + \theta_t + X_i + \epsilon_{ict} \tag{6}$$

where $callback_{ict}$ is an indicator for whether resume $i$ in a given city $c$ at time $t$ received a callback, $UR_{ct}$ denotes the unemployment rate, and $older_i$ indicates whether the applicant

[17] We note that our estimates are robust to the exclusion of these covariates.

is over age 50. Additionally, $\alpha_c$ and $\theta_t$ represent city and time fixed effects, respectively, and $X_i$ is a vector of other characteristics assigned to the resume (e.g., length of unemployment spell).

The coefficient of interest here is $\beta_3$, which tells us how much the callback rate changes for older applicants, relative to younger ones, for a one percentage point increase in the local unemployment rate. A negative coefficient would suggest that recessions exacerbate age discrimination on the entry margin. We cluster the standard errors at the city-round level, since that is the level of randomization. We follow Neumark et al. (2019b) and weight observations by the ratio of the share of employment in Office and Administrative support occupations at a national level to the share based on postings in the Farber et al. (2017) dataset. We calculate the national shares using the Occupational Employment Statistics (OES) data. As we show in the Appendix, unweighted estimates yield similar findings, though with less precision. We use a linear probability model; results are virtually identical if we instead use a probit or logit.

When estimating regressions which include rounds 4 and 5, we add both an indicator for being in one of these two rounds and its interaction with whether the fictitious resume was assigned an older age. We do this because, unlike in rounds 1-3, each employer receives a total of four applicants: at least two older and at least one younger, rather than just a single pair of either type. Thus, the interaction term captures how older female applicants fare when they are in direct competition with at least one additional younger female applicant.[18]

A negative coefficient on this interaction is consistent with the idea that increasing the number of younger employees applying to a firm increases the extent to which a firm can be selective/discriminatory without bearing as much of a cost. One way to increase the number of options an employer has is to increase the unemployment rate, since more individuals will be looking for a job; this is the source of variation we exploit for both rounds 1-3 and the EEOC analysis. In rounds 4 and 5 of the correspondence study, the options an employer has to choose from is experimentally increased by 1 or 2 additional younger applicants.

[18] Phillips (2019) makes the general point that spillovers can occur when multiple applications of different types are sent to the same job posting.

# 5 Results

In this section, we first present results using the EEOC data. We begin with a graphical overview, followed by our regression models and several robustness checks. We then report our findings using the correspondence data.

## 5.1 EEOC Charge Results

**Graphical overview.** Figure 3 provides an initial look at how the combined number of monthly ADEA hiring and firing discrimination charges evolves over the business cycle. Total charges and merit charges increase by roughly 70% and 55% as unemployment rises from a low of 4.5% to a peak of 10%.[19]

The aggregate trends in Figure 3, while informative, mask underlying heterogeneity by geography and sector. Figure 4 provides a state-level view of how both the volume and quality of ADEA hiring and firing claims moved during the pre versus post-recessionary periods, using the official NBER recession dates. Panels (a) and (b) show that the economies hit hardest by the contraction between 2005 and 2009 were also the ones for which total discrimination charges and their average quality increased most sharply. Likewise, in panels (c) and (d), each state's unemployment rate change between 2009 and 2015 is plotted against the corresponding change in the volume and quality of ADEA charges, respectively. It is clear from the graphs that the state economies that recovered least from the Great Recession were also the ones that sustained the largest increase in volume and claim quality.

Figure 5 presents a similar set of graphs, but this time using industry-specific changes in the unemployment rate. Industries more susceptible to the negative labor demand shocks perpetuated by the Great Recession, such as mining and construction, were also those that experienced the largest increase in the volume of charges between 2005-2009 (panel a). Conversely, industries recovering more fully exhibited the largest reduction in charges from 2009-2015 (panel c). And as with the nature of the geographic shocks, the countercyclical relationship of merit is borne of both an increase during bad times (panel b) and a curtailment

[19]The number of merit charges drops following the break in the data near the end of 2011. We include time fixed effects in all of our regressions, which should capture this level difference. Our results are also robust to only using the period prior to the break in the data.

in the wake of the recovery (panel d). The differently-sized responses to these shocks indicate that the state and industry sources of variation are unique from one another. Combining both types of shocks, then, generates even richer variation in labor market conditions, and should yield greater precision in estimation.

To visualize the dynamics in an "event-time" framework, similar to Lamadon et al. (forthcoming), in Figure 6 we plot how the timing of changes in volume and merit vary with the timing of the sharp change in unemployment due to the recession. Since the underlying unemployment shocks are continuous, we first define two groups: state-industry cells with above-median versus below-median unemployment shocks. The shocks are defined based on the percent difference in unemployment between the first and last months of the Great Recession. The graph plots the difference in the magnitude of the unemployment shocks between the two groups, as well as the percentage difference in both total and merit charges between these two groups. The spike in charges occurs just after the sharp increase in unemployment during the Great Recession.

**Volume regressions.** We now turn to regression results for the volume of discrimination charges at the industry-state-month level in Table 3. Start with column (1), which regresses the combined number of charges (firing and hiring) on the contemporaneous number of unemployed individuals as described in equation (3). The point estimate reveals that when the number of unemployed persons rises by 100,000, there will be 1.31 more age discrimination charges. This coefficient can be easily rescaled to estimate the effect of a one percentage point increase in the national unemployment rate.[20] The rescaled estimate, which we label in bold as the "effect of 1 pp ↑ unemp" in our tables, reveals that each one percentage point increase in the national unemployment rate generates 20.2 additional monthly ADEA discrimination charges off a baseline of 665.0 charges, or a 3.0% increase. Alternatively, the elasticity of charges with respect to the unemployment rate is 0.21.

Splitting the sample into firing and hiring cases makes clear that most of the increase in age discrimination is driven by the firing margin. This makes sense, as firing cases are

[20]Specifically, we multiply the coefficient by 1 percent times the average size of the labor force over the sample period (154 million workers).

much more common (85% of the sample). Table 3 indicates that a one percentage point increase in national unemployment leads to 18.6 additional monthly firing charges (column 3), compared to just 1.5 additional hiring charges (column 5). In percent terms, this is a 3.3% increase in firing charges and 1.6% in hiring charges.

It is possible that discrimination effects are dynamic, growing over the life-cycle of the recession. To allow for this, we turn to the polynomial distributed lag model of equation (4). The AIC always selects an optimal lag length of 6 and a quadratic polynomial. Column (2), (4), and (6) report the cumulative effects over all periods, and finds total effects which are similar to the contemporaneous model results in columns (1), (3), and (5). Appendix Table A2 reports all of the PDL coefficients. Most of the effect shows up contemporaneously, with little evidence of lagged unemployment mattering.

Thus, we can use either the contemporaneous or polynomial distributed lag estimates to make in-sample predictions for how the Great Recession induced hiring and firing discrimination reports. During that period, the national unemployment rate more than doubled from 4.5% to 10%, suggesting that ADEA firing discrimination claims increased by 102 per month, an 18% jump relative to the mean. ADEA hiring discrimination reports, on the other hand, are predicted to have increased by 8.3 per month, a 9% increase.

Lastly, we test the extent to which our volume results are purely mechanical. That is, if a fixed fraction of all discharged workers file an ADEA discharge claim with the EEOC, any increase in displaced workers would necessarily raise the number of discharge discrimination claims we observe. However, when we directly control for the number of discharges and layoffs in a state-month,[21] the measured effect of unemployment on the number of ADEA discharge claims filed is materially unchanged.[22] This suggests that the number of complaints is increasing not simply because the number of fired workers has increased but rather because the share of fired workers who filed charges has increased.

[21] We use BLS JOLTS state experimental estimates, which combine the available sample from JOLTS microdata with model-based estimates, and are smoothed with a 3-month moving average.

[22] The coefficient (standard error) on the scaled unemployed coefficient changes from 41.4 (8.1) to 41.3 (8.0) charges per percentage point increase in the national unemployment rate.

**Merit regressions.** While the increases in volume are important in their own right, in isolation they do not reveal whether actual employer misconduct rose, or whether the increase is driven by lower quality filings in the midst of a weak job market. A sufficient condition for elevated age discrimination during a recession is that average case quality does not decrease, a condition we discuss in Section 4 and test for using our merit variable.

Table 4 estimates the relationship between the number unemployed in a state-industry-month and the quality of ADEA firing and hiring charges. The dependent variable is whether a claimant's case was found to have merit. Both age and female are positive predictors of the success of an ADEA discrimination claim. Charges are also 4 percentage points more likely to be meritorious when filed against private versus public firms.

Somewhat remarkably, the quality of combined age discrimination charges (firing+hiring) *increases* during the Great Recession. The implied effect in column (1) is that each one percentage point increase in a state-industry's monthly unemployment rate engenders a 0.0012 increase in the fraction of claims with merit.[23] This is relative to an average merit rate of 0.167, and so translates to a 0.7% increase.[24] Looking at separate merit regressions for firing and hiring in columns (3) and (5), a similar pattern emerges, although only the firing estimate is statistically significant. The polynomial distributed lag models yield similar total implied effects; estimates for individual lags are found in Appendix Table A3.

Combining the volume and merit results, we conclude that the level of actual discrimination rose during the Great Recession. During that period, the national unemployment rate rose by 5.5 percentage points from trough to peak, implying the fraction of cases with merit rose by 0.67 percentage points, or a 4% increase relative to the mean.

## 5.2 EEOC Charges: Robustness, Composition, and Heterogeneity

**Robustness.** Table A5 reports a variety of specification checks, both for the volume (top panel) and merit regressions (bottom panel). Mirroring the graphical analysis of Figures 4

[23]To calculate the implied effect, we multiply the estimated coefficient by 1% times the average size of a state-industry's labor force (681,000).

[24]For robustness, we re-run the specification excluding all individual covariates (i.e., age, race, sex, etc.). Our main coefficient estimate and precision are reduced slightly to 0.0010 (0.0030), a 0.6% increase.

and 5, in columns (1) and (2) we separately test for effects during the run-up (2005-2009Q2) and recovery from (2009Q3-2015) the Great Recession. While the volume effects in terms of percent change nearly double in the latter period, the countercyclical response of merit is two and a half times stronger in the first half of the sample.[25] Column (3) demonstrates that the volume and quality results are nearly the same when the sample is restricted to workers over the age of 50. In column (4), we replace the date of filing with the self-reported date of the discriminatory event; while the direction is the same and precision similar, the coefficients are smaller.

The results are likewise robust to only using variation in unemployment at the state level in column (5), with even larger percent changes for both volume and merit. These state level regressions do not require the imputation described in equation 2, yet reach similar conclusions and remain statistically significant. In column (6), we use the unemployment rate as the independent variable and for volume use the number of charges filed in a state-month divided by the size of the relevant labor force as the dependent variable (the merit variable remains the same as before). This rate-on-rate specification weights the data by the size of each state's labor force, and produces somewhat larger estimated effects than the baseline specifications. Column (7) uses employment to population ratios instead of unemployment rates as a measure of state labor market tightness, and finds qualitatively similar results.[26] But we caution that these last two sets of estimates could suffer from division bias.

We further assess the sensitivity of our results to two additional measures of market tightness in Table 5. We first use a measure of log tightness (that is, log job openings – log unemployment). To do this, we use JOLTS data (Job Openings and Labor Turnover Survey), which provides estimates of both state-level job openings and national-industry job openings. This means that we are able to apply the same imputation procedure as we do in

[25]If we instead use the time period before the missing-data break period (2005-October 2010), the estimates imply a 2.8% change effect for volume and a 0.9% change effect for merit.

[26]To convert the volume of charges filed to a rate, we divide the number of charges by the size of each state's population. The merit variable remains the same as before but both estimates weight observations by the size of each state's working-age populaton.

our main estimation approach to impute job openings at the state-industry level. Columns 1 and 3 show that a 1 standard deviation increase in log tightness reduces combined hiring and firing charges by 0.435 and decreases merit by 0.009 percentage points, respectively. Figure A4 displays corroboratory graphical evidence that sharp decreases (increases) in log tightness over the study sample period were associated with increases (decreases) in charge volume and merit.

Following Landais et al. (2018) and Kroft et al. (2019), we construct the state analog to their national recruiter-producer ratio, which captures the share of a firm's workforce devoted to recruiting. The measure is defined as $\tau = \frac{\rho \times rec}{l - \rho \times rec}$, where $rec$ is the seasonally-adjusted number of workers in the industry with NAICS code 56131, which corresponds to "employment placement agencies and executive search services," $l$ is total state-level private employment, and $\rho$ is a scaling factor which captures recruiters that operate outside of the recruiting industry. We set $\rho$ equal to 8.4 to match Landais et al. (2018), who do so based on US survey evidence. Because, like log tightness, this is a procyclical measure, we expect to see that increases are associated with decreases in both volume and merit. Indeed, columns 2 and 4 of Table 5 report that a one standard deviation increase in the recruiter-producer index reduces state-level ADEA charges and reduces merit, though with limited precision.

The volume estimates in Table 3 weight by the size of the state-industry-month labor force to make the estimates nationally representative. Appendix Table A4 reports unweighted volume regressions, with similar qualitative results. Appendix Table A6 assesses the sensitivity of the results to the exclusion of the construction industry, which was hit particularly hard by the housing crisis. However, this has virtually no effect on the main results, which is unsurprising given that just 2.2% of all ADEA charges are filed against firms in the construction industry. Appendix Table A7 show that the volume and merit results are similar if we include two lags of unemployment instead of using the PDL model.

**Changes in the composition of filers.** The merit and volume results are consistent with age discrimination increasing when economic conditions are poor. A possible confounder is that the composition of workers reporting age discrimination changes during recessions. On the one hand, if better workers are laid off during recessions and these workers are more adept at filing and arguing their case, then our measures of volume and merit would rise even if actual age discrimination remained constant. Consider, for example, a scenario in which only bad workers file claims when jobs are plentiful whereas more skilled workers simply switch jobs when they are terminated illegally due to age considerations. In the midst of a recession, however, even high-skilled older workers may fail in their job search and so would be more inclined to file discrimination claims. It is also possible that resourceful workers exert more effort during recessions to avoid being laid off (Lazear et al., 2016), with workers who are less competent at filing and winning cases being laid off instead. In this case, our measures of volume and merit would be biased in the other direction.

We conduct several empirical exercises to probe compositional effects in Table A8. We first explore whether merit increases countercyclically due to changes in the skill level of workers. The compensation awarded to successful claimants should, in theory, equal the value of the lost wages due to a firm's discriminatory firing. If wages are a reasonably good proxy for skill, the positive selection story would imply that the average compensation won by illegally discharged employees would be countercyclical over the business cycle. But column (1) finds a negligible impact of unemployment on the average damages awarded among those cases for which any compensation is provided, suggesting no change in the composition of cases by benefit level.

Another possible compositional change is that the characteristics of EEOC filers in cases involving age discrimination varies over the business cycle. When a person files an ADEA age discrimination charge, they can also include other additional protected classes (i.e., bases) in their charge, such as sex, race, or disability status. Likewise, the issues raised in the case can vary, such as whether harassment was involved. Column (2) controls for the presence of all other bases and issues raised (see Table A1) and finds the headline merit estimate unchanged.

We also examine whether merit rulings reflect economic considerations such as salary, benefits, and productivity. As a reminder, reasonable economic factors other than age are perfectly legal grounds for dismissal, even if they have a disparate impact on older workers (see Section 2.1), and so should not result in a merit ruling. To explore this empirically, we first make the observation that if workers are paid their marginal product, wage dispersion in an industry should reflect the underlying productivity distribution of its workers. This is especially true the more decentralized is the prevailing wage-bargaining system (Dahl et al., 2013). The relative absence of intra-industry wage dispersion then implies either that productivity is uniform or that differences in productivity are not easily observable. Thus, if recession-generated increases in merit rulings are driven by high wage-dispersion industries, this would raise the specter that our measure of quality in firing discrimination charges is contaminated by productivity considerations.

To test this, we use the 2004 BLS Occupational Employment Statistics (OES) to construct a measure of industry wage dispersion: the quartile coefficient of wage dispersion.[27] Among the low wage-dispersion industries are food services and accommodation, retail, and transportation and utilities. We modify our merit regression to include a measure of wage dispersion at the 4-digit industry level (290 industries) and its interaction with the level of unemployment.[28] Column (3) of Table A8 finds a sizable negative coefficient on the interaction term, providing evidence that in slack labor markets, meritorious ADEA discharge claims are being filed in industries for which differences in output across workers are less, rather than more pronounced.[29] The implication is that the recessionary uptick in merit is unlikely to have resulted from age-blind economic calculations.

A different compositional explanation is that larger firms, against which discrimination claimants have less success (see Appendix Figure A5), are less likely to have been accused

[27] The quartile coefficient of wage dispersion is defined as $(P_{75} - P_{25})/(P_{75} + P_{25})$. We obtain similar results if we use the 90th and 10th percentiles instead. We use measures of wage dispersion from the year 2004 so that they are uncontaminated by any recession-induced compression.

[28] In 7.5% of observations, the quartile coefficient of wage dispersion is not available at the 4-digit industry level either because the employment cell is too small to compute percentile wages or because the percentile wage is top-coded at $145,600 (in 2004 dollars). In either case, we replace the missing value with that of its 2-digit industry measure of wage dispersion.

[29] We limit the sample to firing cases for this analysis since productivity is more likely to be observed for those already employed. However, the results are robust to including hiring cases as well. To rule out the possibility that the dispersion interaction is instead capturing low-wage industries, which tend to have more compressed wage distributions, we additionally tried interacting unemployment with median industry wages. The measured wage dispersion interaction effect is insensitive to this modification.

during recessions. However, we find that whereas the fraction of charges accounted for by larger firms was less than proportional to the share of workers employed by such firms prior to the Great Recession, larger firms contribute a more than proportionate share of ADEA charges in its aftermath.[30]

Another possibility is that claimants employ more resources to improve their chances of winning when the job market languishes. While legal representation increases the chance a claimant receives a merit ruling by 4 percentage points, the unemployment coefficient remains virtually unchanged. Neither is it the case that the retention of legal representation is more common nor statistically more likely to elicit a merit designation during recessions.[31]

Relatedly, the level of resources the EEOC has at its disposal for investigating claims both over time and across geography is ruled out as an explanation with the inclusion of month and state fixed effects in our regressions. It is possible that firms are more willing to settle a case during recessions, even after netting out time, state, and industry fixed effects, but this is not something we are able to analyze formally.

**Heterogeneity by gender.** Recent work has highlighted the extent to which age discrimination is intersectional with sex. Older women receive less protection under the ADEA (Button, 2020; McLaughlin, 2020), there is more evidence of age discrimination against females in correspondence studies (Lahey, 2008; Farber et al., 2017; Neumark et al., 2019a,b), and there are different patterns of explicit age discrimination against women versus men (Helleseter et al., 2020). In Appendix Tables A9 and A10 we repeat our main analyses for volume and merit, but only for the sample of women, and find similar results (once volume is scaled by the fraction of women in the labor force).

Pushing further, in Appendix Table A11, columns (1) and (3), we test whether recessions affect the quality of age discrimination firing and hiring charges differently by sex. We find

[30] We further find that the effect of recessions on merit is not monotonically increasing in firm size. Relative to firms having 201-500 employees, the effect of a one pp increase in the unemployment rate on merit for the largest firms is 0, as compared to a positive 2.8 pp for firms having 101-200 employees and a negative 1 pp for firms with fewer than 100 employees.

[31] We estimate that each one percentage point increase in the national unemployment rate increases the fraction of charging parties that privately obtain legal representation by a statistically insignificant 0.8 percentage points, off a 15.7% baseline.

no detectable difference. It might, however, be the case that older females face increased age discrimination specifically when they are competing against younger women. Because gender concentrations vary substantially across industries, ranging from 10% female in Construction to over 78% female in Health Care and Social Assistance, we can leverage this variation to estimate whether recessions raise the quality of claims for older women more in industries employing a higher fraction of women. Indeed, column (4) of Table A11 indicates that labor market slackness increases the quality of ADEA hiring claims filed by older women more, relative to older men, the higher is the ratio of female employees in an industry. While no such effect emerges for firing discrimination, this exercise provides suggestive evidence that competition for work, and the associated relaxation of hiring discrimination constraints, may be gender-specific.

## 5.3 Correspondence Study Results

We now shift focus to our complementary analysis using the correspondence study data, where we test whether older women have a harder time finding a job as the labor market deteriorates. We begin with a graphical view of the data. Figure 7a plots regression-adjusted callback rates for applications assigned older versus younger ages–i.e., the age penalty– against the local unemployment rate within each city and time period for rounds 1-3 of the Farber et al. (2017) data.[32] There is a clear negative slope, implying that weak labor markets exacerbate age discrimination.

Figure 7b graphs a similar relationship, but in differences rather than levels. The circles plot the change in the old minus young callback rate against the change in unemployment rate between rounds 3 and 1. The negative slope approximately matches the slope using levels in panel (a). Similar negative slopes are found using changes between rounds 3 and 2 or between rounds 2 and 1.

For a more precise estimate of the relationship between recessions and the intensity of age discrimination against women, we present regression results based on equation (6). The

[32]The regression-adjustment controls for other characteristics found on the resume, such as the length of the applicant's listed unemployment spell and whether or not the applicant held a low-level interim job. Also included is the size of the public sector in each city-year.

key coefficient on the interaction term, $older_i \times unemployment\ rate_{ct}$, tells us how much the callback rate changes for older applicants, relative to younger ones, for each one percentage point increase in the local unemployment rate. Because federal, state, and local government employers are bound by additional regulations stipulating that all applicants receive a fair chance at employment, they are likely to have less discretion to respond to job inquiries in a discriminatory fashion. To account for this, we control for the fraction of public employment in a city as well as an interaction term, $older_i \times public_{ct}$. This mainly impacts Sacramento, which is a state capital, and has a public sector which is approximately twice as large as the remaining 7 cities (see Appendix Figure A6).

The first column of Table 6 reports estimates without city or time fixed effects. The second column adds these fixed effects into the regression, and shows the estimates are similar. Each one percentage point rise in the local unemployment rate reduces the callback rate for older applicants by 2.3 percentage points in the first three rounds of the correspondence study, off a baseline 11.6% callback rate. This translates to a 21% decrease in the number of callbacks for older applicants. As anticipated, increases in the size of the public sector appear to reduce age discrimination as well. In column (3), we add an additional control for $public \times unemployment\ rate_{ct}$, which while an important callback predictor, has almost no effect on our coefficient of interest.

Next, we add to our analysis the fourth and fifth rounds of the study. Recall these rounds differ from the first three as there are now 4 applications submitted to each posting, at least two of which are older applicants and at least one of which is younger. In rounds 1-3 either two older or two younger applicants were sent to each job posting. Whereas the variation in the first three rounds emanates from differences in local labor market conditions over time and across cities, rounds 4-5 additionally introduce within-job posting variation in age. Therefore, we include an interaction term for $older_i \times competing_i$, where $competing_i$ is a dummy variable for being an observation from the 4th or 5th rounds (and hence competing against additional applicants of different ages).

The first column of Table 7 does not include city or time fixed effects, while the latter two columns do. Focusing on the second column, similar to what we found in Table 6, older applicants are relatively less likely to receive a callback in cities that recovered less successfully from the Great Recession. In percent terms relative to the mean, the effect size of -1.5 percentage points represents a 15% drop. The specification in column 3 yields virtually identical estimates. Unweighted estimates corresponding to Tables 6 and 7 are also similar, though lose some precision (see Appendix Tables A12 and A13).

The interaction term $older_i \times competing_i$ tells us how older workers fare when they are in direct competition with one or two additional younger workers. The estimate in column (3) provides suggestive evidence that, all else equal, an older female applying to an administrative support position is less likely to receive a callback when she is competing directly with otherwise similar younger applicants. While not statistically significant, the 1.9 percent point is sizable, indicating that when an employer faces a lower search cost to hire younger workers, they tend to disfavor older applicants. The relatively large magnitude may partly be driven by the fact that the additional younger applicants in the last two rounds are near-perfect substitutes for their older counterparts except for age on their resumes.

When we use logit models to estimate callback probabilities, as in Farber et al. (2017), the estimated odds ratios for $older_i \times unemployment\ rate_{ct}$ are 0.788 (s.e.=0.050) in rounds 1-3 and 0.845 (s.e.=0.039) in rounds 1-5. The estimated odds ratio for $older_i \times competing_i$ is 0.826 (s.e.=0.146). Either set of estimates is nearly identical to the effect sizes implied by the corresponding linear probability model coefficients.

A caveat with our correspondence study is that it only measures age discrimination in the hiring of female applicants for administrative support positions. Hence, our findings may not be externally applicable to other populations, such as for males or for other occupations.

It would be interesting to extend our analysis to use data from other existing correspondence studies on age discrimination. Unfortunately, either sample sizes are too small (Bendick et al., 1997, 1999; Riach and Rich, 2010), the number of cities across which the resumes were sent is too small (Lahey, 2008), or the variation in unemployment is too limited (Neumark et al., 2019a,b). While Neumark et al. (2019a) conducted a correspondence study across twelve different cities and Neumark et al. (2019b) across 50 states, they did so during 2015 or 2016, when even the hardest hit labor markets were mostly recovered from the Great Recession. Hence, these two studies occurred during relatively tight labor markets and provide substantially less variation in unemployment. In the Farber et al. (2017) and Farber et al. (2019) data we analyze, unemployment rates can be twice as large for some cities versus others, and fell by half for most cities from the beginning to the end of our sample period. In the Farber data the mean unemployment rate across cities and time is 6.5%, with a standard deviation of 1.9% (see Figure 2). In comparison, Neumark et al. (2019a) has a mean of 5.3% and a standard deviation of 0.8%, and Neumark et al. (2019b) has a mean of 4.4% and a standard deviation of 0.9%. When we attempted to use the data from either Neumark et al. (2019a) or Neumark et al. (2019b), the estimates were too noisy to be informative and not statistically different from zero.

# 6 Discussion

Using two complementary analyses, we find evidence that age discrimination rises when labor markets are slack. This novel empirical finding is the main contribution of the paper. A priori, there are some theories which predict a rise in age discrimination as labor market deteriorate, and other that predict a fall. In this section we discuss a variety of these theories. We recognize that several mechanisms could be in play simultaneously, and that the net effects are ultimately an empirical question.

**Models of taste-based discrimination.** The search cost model developed by Biddle and Hamermesh (2013) and used by Baert et al. (2015) predicts that taste-based discrimination will increase as unemployment rises. The insight is that employer search costs create a market penalty for passing on qualified, but disfavored workers. During recessions, the emergence of a larger pool of job seekers reduces this market penalty. Discriminatory firms will fire their older workers first and have an easier time finding preferred younger workers. Relatedly, in a model with downwardly rigid wages which necessitates layoffs, discriminatory firms can more easily fire equally productive but less preferred workers without harming profits.

Another reason recessions could affect taste-based discrimination is related to the seminal contribution of Becker (1957) whose insight is that discrimination is unsustainable in a perfectly competitive product market (see Black and Strahan, 2001; Black and Brainerd, 2004). If recessions cause competitors to go bankrupt, then taste-based discrimination would be easier to sustain. However, if product markets become more competitive during recessions, this would have the opposite effect. For example, during recessions, firms facing significant credit constraints and on the brink of survival would have a stronger incentive to fire unproductive workers instead of disfavored older workers. Of course, taste-based discrimination need not be driven by employers' personal preferences. It could also be the case the customers have discriminatory tastes to interact with younger workers, in which case profits could be higher for discriminatory firms which do not hire older workers. During a recession, otherwise ethical firms may have an incentive to compromise age-blind hiring practices. Similar stories could be told for coworker taste-based preferences.

**Models of statistical discrimination.** Search cost models can readily be extended to statistical discrimination. When the supply of job applicants is plentiful, firms could have less incentive to invest time and effort into disentangling individual productivity from group averages and instead rely on age as a screening mechanism. In this model, if older workers are less productive on average, age discrimination will be countercyclical. However, the benefits to discerning individual productivity could also go up during a recession, for example if the distribution of worker quality among those seeking a job includes more high-skilled workers (Modestino et al., 2019).

Countercyclical discrimination would also be consistent with the ranking model of Blanchard and Diamond (1994). In a slack labor market, the size of the applicant pool is larger. If age is a signal of low unobserved productivity, then older workers will be less likely to be ranked as the top applicant (or among the top applicants) for a job when there are many workers vying for the same position.

As pointed out by Carlsson et al. (2018), the screening models developed by Vishwanath (1989) and Lockwood (1991) can lead to procyclical statistical discrimination. Even if the unconditional productivity for older and younger workers are the same on average, if older workers have a higher variance in unobserved productivity,[33] age discrimination will fall during a recession. The idea is that in a tight labor market, unemployed workers looking for jobs are drawn from the far-left tail of the skill distribution. The expected productivity conditional on being unemployed will be lower for the group with the higher variance, creating an incentive for firms to statistically discriminate against older workers.

In our setting, it is difficult to differentiate between the various models. It is likewise hard to separate taste-based from statistical motivations. However, regardless of the mechanism or type of discrimination, we note that all forms are illegal, and from the worker's perspective, equally harmful. Note that models which predict changes in hiring or firing of older workers based on current productivity are not illegal, and should not appear as meritorious cases in our EEOC data. For example, signal jamming models – where firms use recessions as cover to fire older workers who are less productive or more costly – are perfectly legal, even if there is an implicit commitment to overpay older workers, as in a deferred compensation model (Lazear, 1979; Schleifer and Summers, 1988).[34]

# 7 Conclusion

The Great Recession had two countervailing forces affecting older individuals (Bosworth, 2012; Munnell and Rutledge, 2013). On the one hand, there was a loss in housing wealth and a drop in defined contribution plan balances which pushed older workers to remain in the labor force. On the other hand, job loss coupled with the difficulties older workers had becoming re-employed pushed them towards early retirement. As Munnell and Rutledge document, the Great Recession was different from the earlier recessions of the 1970s, 1980s, and 1990s. The labor force participation of older individuals went up instead of down, and unemployment rates exceeded those in prior recessions. While evidence shows that the proportion of workers expecting to retire before age 65 fell, this did not translate to an

[33]Several papers argue that older workers have a higher dispersion in human capital investments (e.g., Mincer, 1974; Heckman et al., 2006).

[34]It would still be illegal to fire a worker based on expected future declines in productivity based on their age as this is a form of statistical discrimination.

increase in actual retirement. Munnell and Rutledge argue that this was due to the difficulty older workers had in finding a job.

With this context in mind, we estimate how age discrimination responded to the Great Recession using two complementary analyses. In the first analysis, we deploy individual-level data on discrimination charge filings with the EEOC before, during, and after the Great Recession. Our estimates imply that from the trough to the peak in unemployment, age-related firing and hiring discrimination charges increased by 18% and 9%, respectively. We use a proxy for the quality of a claim to disentangle countercyclical employee filing incentives and genuine employer misconduct. We estimate that the Great Recession induced a 4% increase in the quality of firing and hiring discrimination claims. Under certain assumptions, these results are sufficient to conclude that both actual and reported discrimination against older workers increased during the Great Recession.

In our second analysis, we repurpose data from the correspondence studies of Farber et al. (2017) and Farber et al. (2019) to examine how older female job applicants fare when unemployment is higher. We find that a one percentage point increase in unemployment leads to a 15% decrease in the relative likelihood of receiving a callback.

Combined, these two analyses provide compelling evidence that negative labor demand shocks increase employment discrimination against older employees. The findings suggest that whatever power disparities exist between an individual and her employer, they grow during recessions so that firms can engage in increased discrimination. From a policy perspective, this argues for increased support of deterrence efforts by guardians against corporate malfeasance–like the EEOC–during periods of economic malaise. A similar conclusion could be extrapolated to other federal watchdog agencies, such as the Occupational Safety and Health Administration (OSHA), as worker injury risk has been shown to increase during economic contractions (Boone and Van Ours, 2006; Boone et al., 2011). Given our findings, it is not surprising that other levers firms have at their disposal to exploit a worker's reduced bargaining power, such as upskilling (Hershbein and Kahn, 2018; Modestino et al., 2016, 2019) and the implementation of non-compete agreements (Johnson and Lipsitz, 2020), have been found to proliferate during recessions.

In future work, it would be interesting to study how discrimination for other classes of workers evolves over the business cycle. However, one challenge to studying classes protected by Title VII (e.g., race or sex) or the ADA, is that employment practices that generate a disparate impact are illegal, complicating the interpretation of any findings. In contrast, the ADEA allows for firings and hirings based on cost or productivity considerations, even if they disproportionately affect older workers.

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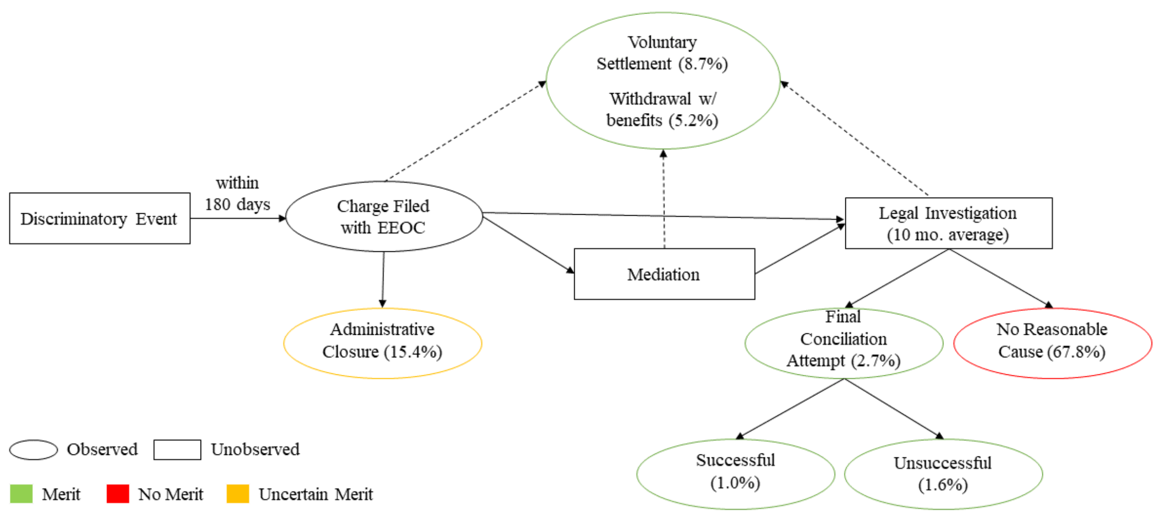



Figure 1: EEOC Charge Filing Process and Resolution

Flow chart describing the order of events, beginning with the discriminatory action and ending with the resolution of the EEOC discrimination charge. Percentages are shown for ADEA hiring and firing charges in our baseline sample. A small fraction of charges (0.3%) are resolved through EEOC-initiated litigation (not shown above).

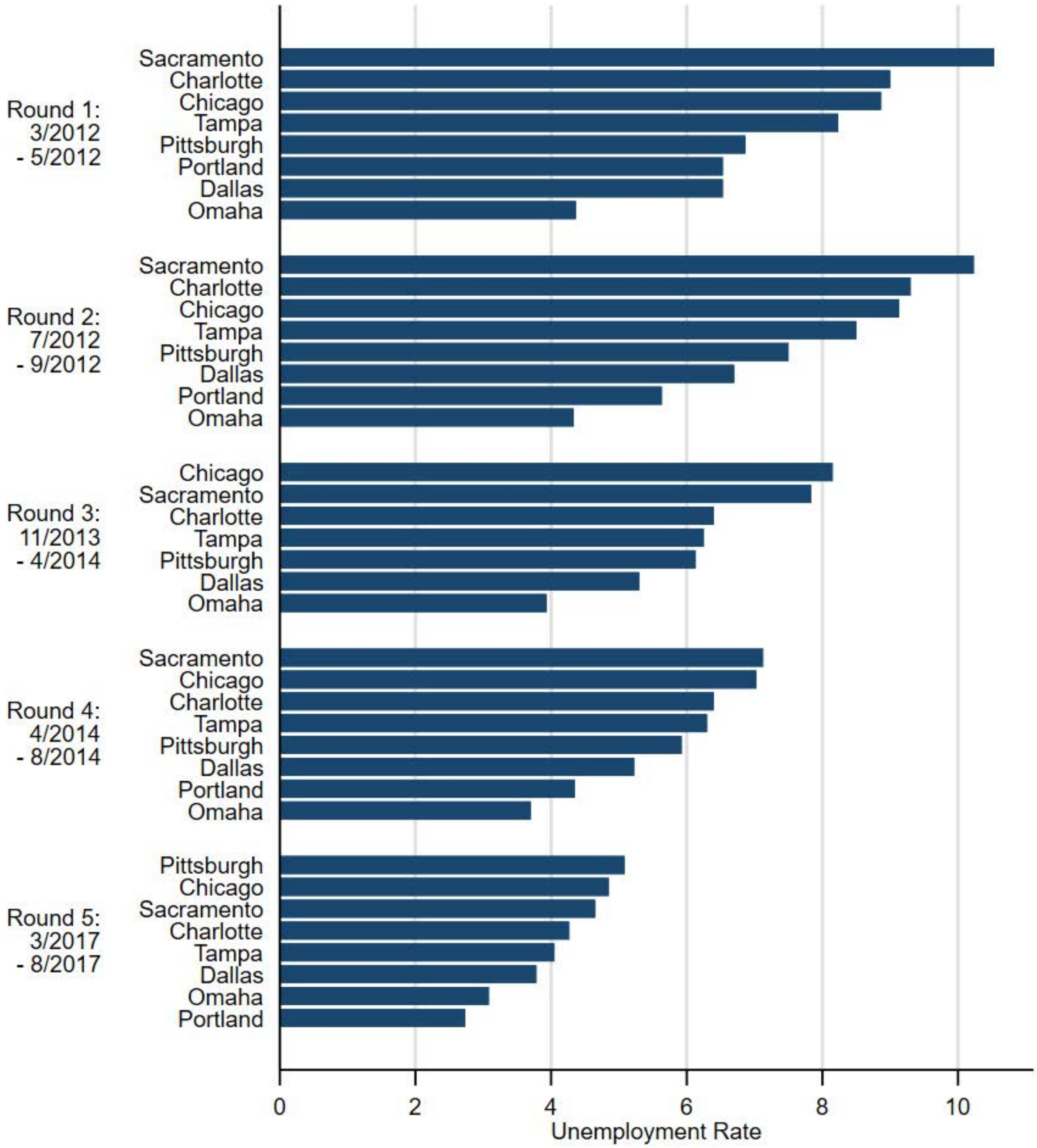



Figure 2: Local Unemployment Rates in Correspondence Studies

Unemployment rates calculated at the MSA level for a city, averaged over the relevant time period in a round, for the Farber et al. (2017) and Farber et al. (2019) data.

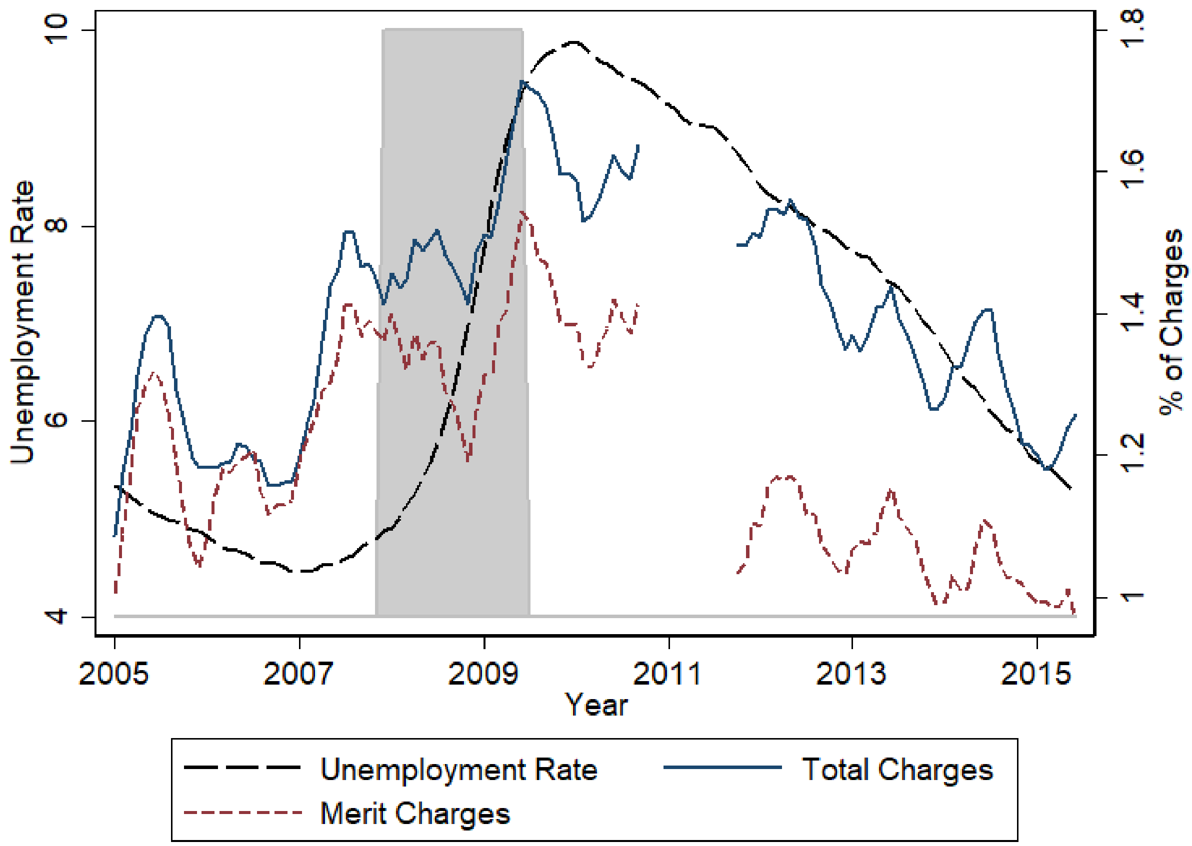
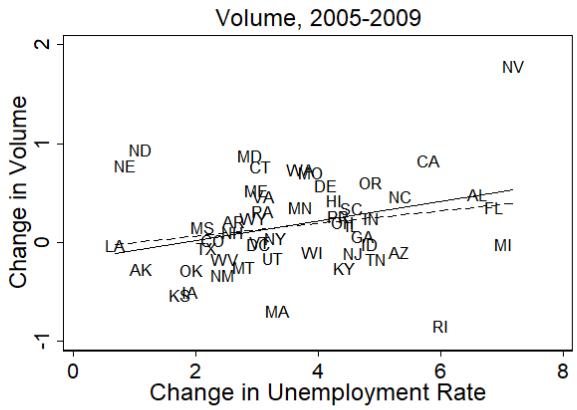
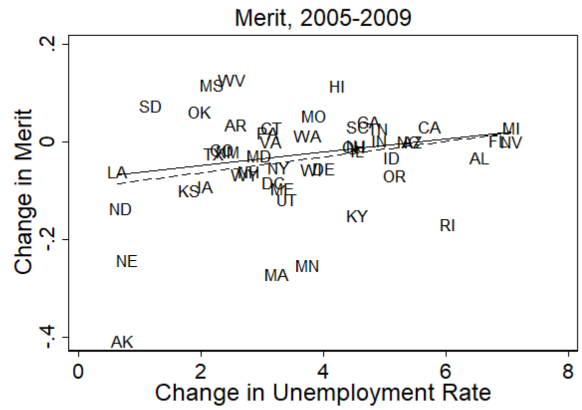



Figure 3: ADEA Hiring and Firing Discrimination Charges over Time

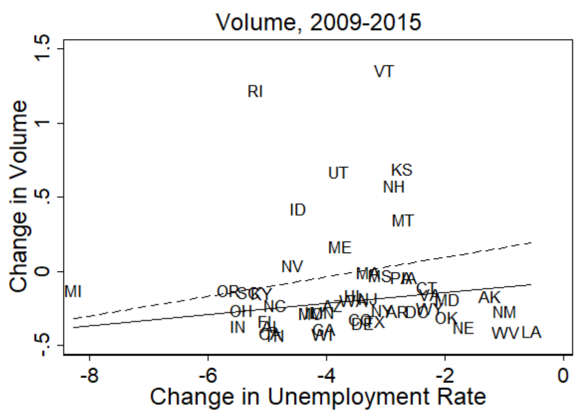
Seven-month smoothed monthly number of nationally aggregated hiring and firing ADEA discrimination charges filed with the EEOC, the smoothed number of those charges with merit, and the smoothed unemployment rate. Total and merit charges are measured as a % relative to the amount of charges in January of 2005. Data are missing from October 2010 through September 2011. The number of merit charges drops following the break in the data near the end of 2011. We include time fixed effects in all of our regressions, which should capture this level difference. Shading indicates the Great Recession, as defined by the NBER.



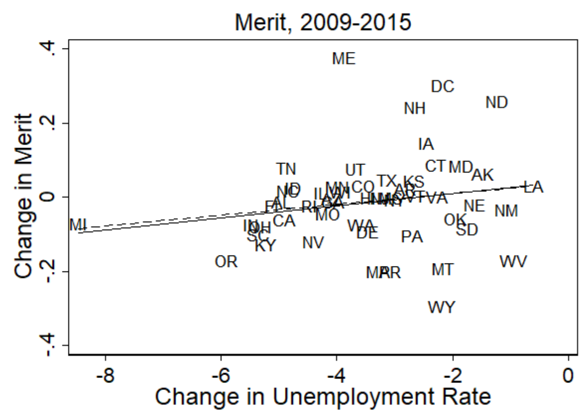

(a) Run-up to Great Recession




(b) Run-up to Great Recession



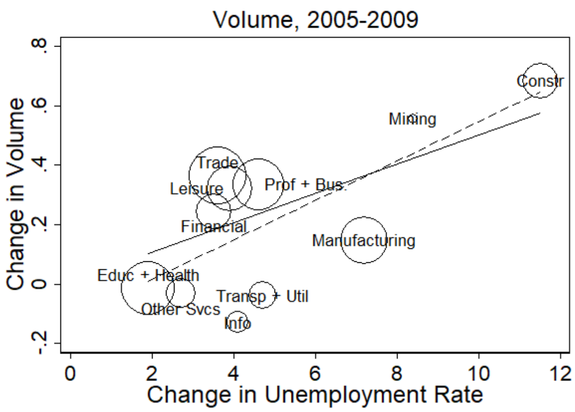

(c) Recovery from Great Recession



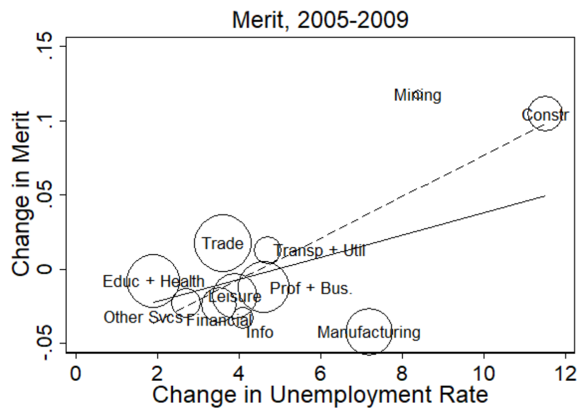

(d) Recovery from Great Recession

Figure 4: ADEA Firing + Hiring Charges Across the Great Recession (by State)

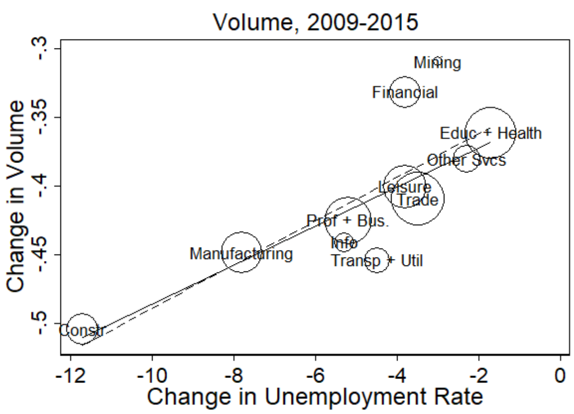
Change in volume is defined as the fractional change in charges relative to the size of each state's labor force. The solid line is the regression line weighted by the size of the state labor force, while the dashed line is unweighted. For visual clarity, the small state of ND is omitted from panel 4c; its changes in the unemployment rate and volume are -1.53% and 349%, respectively. Weighted regression line slopes (standard errors) for panels a-d, respectively, are 0.100 (0.047), 0.014 (0.005), 0.039 (0.028), and 0.016 (0.006).



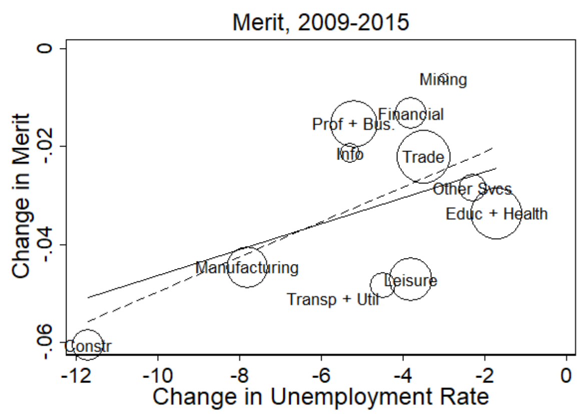

(a) Run-up to Great Recession




(b) Run-up to Great Recession




(c) Recovery from Great Recession




(d) Recovery from Great Recession

Figure 5: ADEA Firing + Hiring Charges Across The Great Recession (by Industry)

Change in volume is defined as the fractional change in charges relative to the size of each industry's labor force. The solid line is the regression line weighted by the size of the national industry labor force, while the dashed line is unweighted. Weighted regression line slopes (standard errors) for panels a-d, respectively, are 0.050 (0.020), 0.008 (0.004), 0.014 (0.003), and 0.003 (0.001).

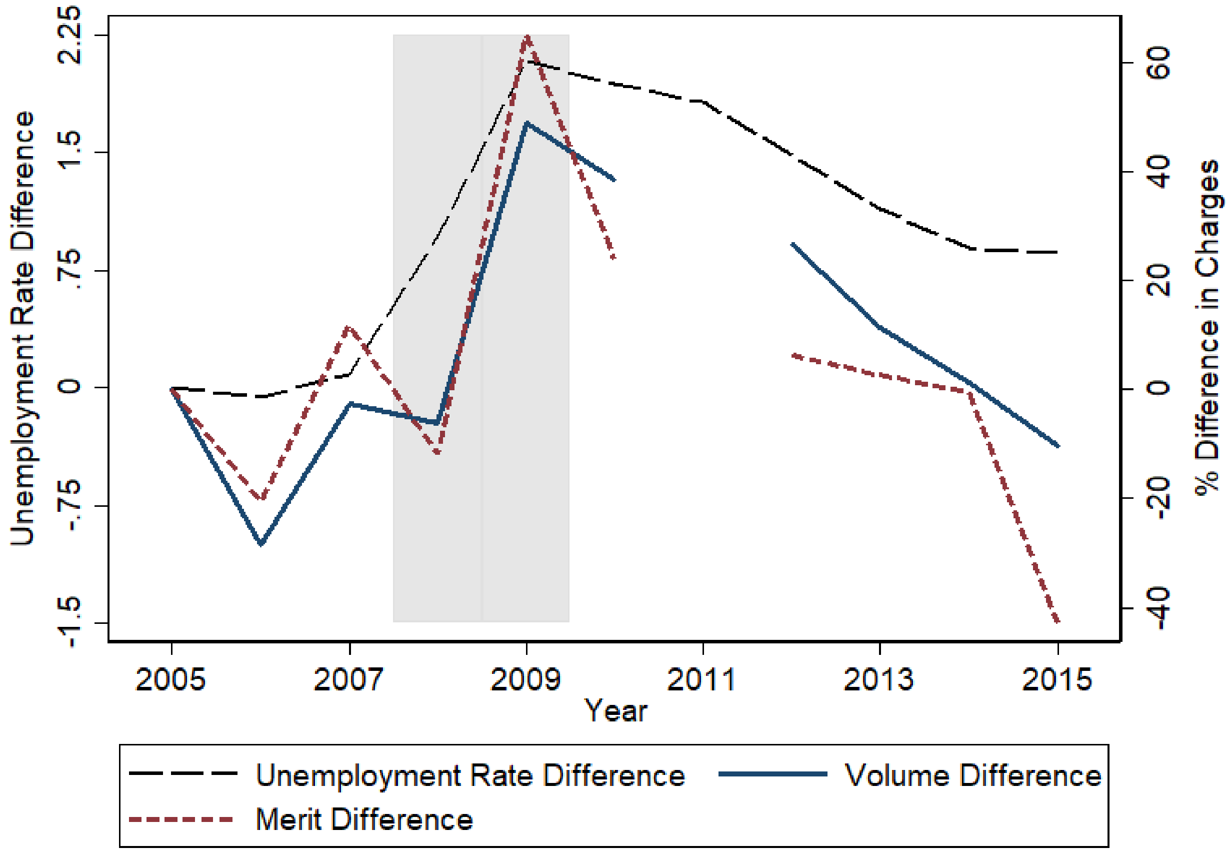
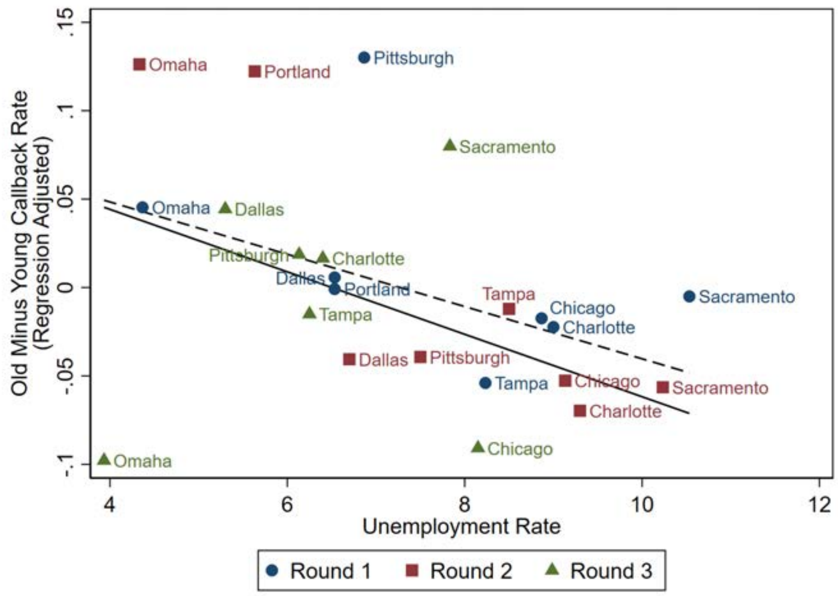
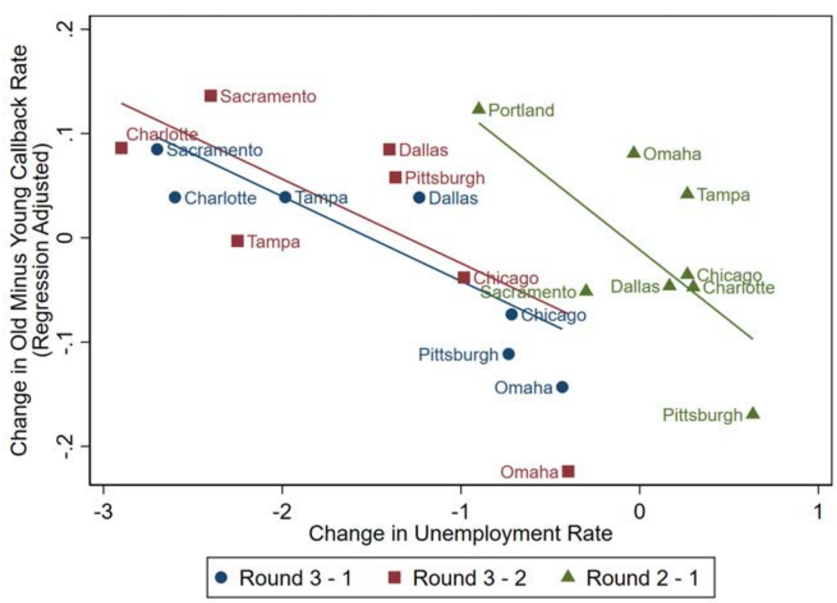



Figure 6: Differences-in-Differences for State-Industry Charges by Recession Exposure

Figure displays the mean differences in annual ADEA firing and hiring charge volume (solid line) and merit (short dashed line) for state-industries that receive an above-median versus below-median unemployment shock (median change in unemployment in the last versus first month of the Great Recession is 97%). The difference in the unemployment shock, which equals the difference in the state-industry unemployment rate across the two groups iin a given year, is represented by the long dashed line. 'Unemployment Rate Difference' and '% Difference in Charges' are expressed relative to the baseline difference in January of 2005. Data are missing from October 2010 through September 2011, and so we exclude 2011 from the graph. Shading indicates the Great Recession, as defined by the NBER.




(a) Age Callback Penalty versus Local Unemployment Rate




(b) Δ Age Callback Penalty versus Δ Local Unemployment Rate

Figure 7: Age Callback Penalty and the Local Unemployment Rates, Rounds 1-3

In panel (a), markers are the regression-adjusted differences in average callback rates between older and younger applicants, using the same control variables and weights as column 1 of Table 6. The solid line is the weighted regression line through the markers, while the dashed line is unweighted. In panel (b), the markers are the corresponding differences across rounds and the weighted regression line uses the average of the city-round weights.

Table 1: Resolution of ADEA Charges, 2005-2015

| | Firing | Hiring |
|---|---|---|
| **Resolutions by Type** | | |
| *Merit* | 0.172 | 0.141 |
| Settlement with benefits | 0.091 | 0.067 |
| Withdrawal with benefits | 0.055 | 0.035 |
| Reasonable cause | 0.025 | 0.038 |
| Successful conciliation | 0.010 | 0.013 |
| Unsuccessful conciliation | 0.015 | 0.025 |
| *No Merit* | | |
| No reasonable cause | 0.670 | 0.735 |
| *Uncertain Merit* | | |
| Administrative closures | 0.159 | 0.125 |
| **Compensation Awarded** | | |
| Average damages awarded | \$29,189 | \$21,929 |
| Total monetary benefits | \$270.8m | \$22.5m |
| Charges | 67,993 | 11,602 |

Average damages awarded is conditional on winning any compensation. Monetary benefits are in millions of dollars and exclude those obtained through litigation.

Table 2: Job Postings by City and Time Period in Correspondence Study

| | Round 1: 03-05/2012 | Round 2: 07-09/2012 | Round 3: 11/2013-04/2014 | Round 4: 04-08/2014 | Round 5: 03-08/2017 | Total |
|---|---|---|---|---|---|---|
| Charlotte, NC | 178 | 167 | 120 | 169 | 106 | 740 |
| Chicago, IL | 173 | 165 | 67 | 275 | 173 | 853 |
| Dallas, TX | 87 | 147 | 161 | 330 | 152 | 955 |
| Omaha, NE | 85 | 147 | 122 | 110 | 62 | 526 |
| Pittsburgh, PA | 145 | 156 | 157 | 149 | 93 | 700 |
| Portland, ME | 78 | 120 | 0 | 87 | 53 | 260 |
| Sacramento, CA | 110 | 156 | 93 | 170 | 99 | 628 |
| Tampa, FL | 171 | 157 | 114 | 228 | 113 | 783 |
| Total job postings | 1,027 | 1,215 | 834 | 1,518 | 851 | 5,445 |
| Applications/posting | 2 | 2 | 2 | 4 | 4 | |

Data collected by Farber et al. (2017). In rounds 1-3 either two younger or two older applications were sent to each job posting. In round 4, two younger and two older applications were sent to each job posting. In round 5, 4 applications were sent to each posting, 2 or 3 of which corresponded to an older applicant.

Table 3: Charge Volume and Unemployment

| | Firing + Hiring | | Firing | | Hiring | |
|---|---|---|---|---|---|---|
| | Base | PDL | Base | PDL | Base | PDL |
| Dep. var. = # of charges | (1) | (2) | (3) | (4) | (5) | |
| $\text{unemployment}_{jst}$ | $1.31^{***}$ | $1.35^{***}$ | $1.21^{***}$ | $1.23^{***}$ | $0.10^{***}$ | $0.12^{***}$ |
| | (0.25) | (0.27) | (0.23) | (0.25) | (0.03) | (0.04) |
| **Effect of 1 pp ↑ unemp** | **20.2** | **20.8** | **18.6** | **18.9** | **1.51** | **1.80** |
| Mean(# national charges) | 665.0 | 665.0 | 568.6 | 568.6 | 96.3 | 96.3 |
| % change | 3.0 | 3.1 | 3.3 | 3.3 | 1.6 | 1.9 |
| Elasticity | 0.21 | 0.21 | 0.22 | 0.23 | 0.11 | 0.13 |
| N (state-industry-months) | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 |
| Polynomial degree | | quadratic | | quadratic | | quadratic |
| AIC | 321,274 | 321,113 | 300,064 | 299,924 | 139,744 | 139,682 |
| $R^2$ | 0.469 | | 0.506 | | 0.088 | |

Industry-state-month level regressions for the volume of cases. The sample period spans 2005-2015. Regression coefficients show the change in charges filed per 100,000 increase in the number unemployed. Observations are weighted by the industry share of employment in each state's labor force. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in the national unemployment rate on the national monthly number of charges filed. The PDL model estimates the cumulative effect of previous and contemporaneous unemployment on current period charges using a polynomial distributed lag model; the coefficient reported equals the cumulative effect across all lags and the contemporaneous period. The AIC is used to choose the optimal number of lags, which equals 6 in all cases; while not shown, the BIC chooses the same lag structure. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level, and correspond to the implied cumulative effect in even-numbered columns.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 4: Charge Quality and Unemployment

| | Firing + Hiring | | Firing | | Hiring | |
|---|---|---|---|---|---|---|
| | Base | PDL | Base | PDL | Base | PDL |
| Dep. var. = $\mathbb{1}(merit)$ | (1) | (2) | (3) | (4) | (5) | (6) |
| $\text{unemployment}_{jst}$ | 17.6*** | 16.6*** | 13.0** | 11.9** | 19.5 | 18.2 |
| | (4.62) | (4.31) | (5.78) | (5.15) | (17.4) | (21.1) |
| age | 0.0015*** | 0.0015*** | 0.0015*** | 0.0015*** | 0.0016*** | 0.0016*** |
| | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0005) | (0.0005) |
| female | 0.0180*** | 0.0180*** | 0.0144*** | 0.0145*** | 0.0301*** | 0.0301*** |
| | (0.0024) | (0.0024) | (0.0025) | (0.0025) | (0.0061) | (0.0061) |
| private | 0.0410*** | 0.0410*** | 0.0413*** | 0.0412*** | 0.0411*** | 0.0408*** |
| | (0.0055) | (0.0055) | (0.0064) | (0.0064) | (0.0094) | (0.0093) |
| **Effect of 1 pp ↑ unemp** | **0.0012** | **0.0012** | **0.0009** | **0.0009** | **0.0013** | **0.0012** |
| Mean(merit) | .167 | .167 | .172 | .172 | .141 | .141 |
| % change | 0.7 | 0.7 | 0.5 | 0.5 | 0.9 | 0.8 |
| Elasticity | 0.04 | 0.04 | 0.03 | 0.03 | 0.05 | 0.05 |
| N (charges) | 78,021 | 78,021 | 67,988 | 67,988 | 11,600 | 11,600 |
| Polynomial degree | | quadratic | | quadratic | | linear |
| AIC | 67,660 | 67,654 | 60,533 | 60,528 | 8,431 | 8,430 |
| $R^2$ | 0.017 | | 0.018 | | 0.042 | |

Individual level regressions for whether a case is determined to have merit. The sample period spans 2005-2015. Regression coefficients on 'unemployment' are multiplied by $10^{-8}$. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in a state-industry's monthly unemployment rate on the fraction of charges found to have had merit. The PDL model estimates the cumulative effect of previous and contemporaneous unemployment on current period charges using a polynomial distributed lag model; the coefficient reported equals the cumulative effect across all lags and the contemporaneous period. The AIC is used to choose the optimal number of lags, which equals 6 in all cases; while not shown, the BIC chooses the same lag structure. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level, and correspond to the implied cumulative effect in even-numbered columns.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: ADEA Charges and Alternative Measures of Labor Market Tightness

| | **Volume** | | **Merit** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| log tightness$_{jst}$ | -0.5263*** | | -0.0120** | |
| | (0.1584) | | (0.0056) | |
| recruiter-producer ratio$_{st}$ | | -133.3 | | -0.7148* |
| | | (262.7) | | (0.4083) |
| std. dev.(tightness or rpr) | 0.826 | 0.012 | 0.768 | 0.012 |
| **Effect of 1 std. dev. ↑** | **-0.435** | **-1.55** | **-0.009** | **-0.009** |
| Mean(dep. var.) | 1.12 | 14.1 | .167 | .167 |
| N | 73,176 | 5,858 | 77,665 | 78,375 |
| unit | jst | st | claimant | claimant |
| $R^2$ | 0.463 | 0.807 | 0.017 | 0.024 |

Regression specifications in columns (1)-(2) parallel those of Table 3, and columns (3)-(4) follow Table 4. Log tightness is defined as log job openings - log unemployment. The recruiter-producer ratio = 8.4× *rec*/l-8.4× *rec*, where *rec* equals the state-level employment in NAICS code 56131, which corresponds to "employment placement agencies and executive search services," and $l$ is state-level private employment. Volume regressions include state and time fixed effects while merit regressions additionally control for industry fixed effects, age, female, race, and whether the firm is private or public. Standard errors clustered at the state level.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 6: Callback Rates and Labor Market Conditions (Rounds 1-3)

| Dep. var. = $\mathbb{1}(callback)$ | (1) | (2) | (3) |
|---|---|---|---|
| $older_i$ x unemployment $rate_{ct}$ | -0.0243*** | -0.0232*** | -0.0231*** |
| | (0.0064) | (0.0063) | (0.0063) |
| $older_i$ | 0.0091 | 0.0024 | 0.0044 |
| | (0.0449) | (0.0445) | (0.0443) |
| unemployment $rate_{ct}$ | 0.0046 | -0.0002 | 0.0304* |
| | (0.0045) | (0.0122) | (0.0176) |
| $public_{ct}$ | -0.6234** | | |
| | (0.2383) | | |
| $older_i$ x $public_{ct}$ | 1.1906*** | 1.1682*** | 1.1465*** |
| | (0.2977) | (0.2950) | (0.2930) |
| $public_{ct}$ x unemployment $rate_{ct}$ | | | -0.2125** |
| | | | (0.0848) |
| Callback rate | .116 | .116 | .116 |
| City FE | | X | X |
| Time FE | | X | X |
| Job postings | 3,076 | 3,076 | 3,076 |
| Resumes | 6,152 | 6,152 | 6,152 |
| City-rounds | 23 | 23 | 23 |
| $R^2$ | 0.008 | 0.015 | 0.016 |

Correspondence study data originally collected by Farber et al. (2017) across 8 cities and 3 different time periods. In rounds 1-3 either two younger or two older applications were sent to each job posting. The variable '$older_i$' is a dummy for whether the applicant is over age 50, 'unemployment $rate_{ct}$' is the city-round unemployment rate, and '$public_{ct}$' is the fraction of the city's workforce employed in the public sector. Additional controls include dummies for the ficticious applicant's unemployment spell length and whether they held a low-level interim job. For each city-round, we follow Neumark et al. (2019b) and weight observations by the ratio of the share of employment in Office and Administrative support occupations at a national level to the share based on postings in the Farber et al. (2017) dataset. Standard errors clustered at the city-round level.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 7: Callback Rates and Labor Market Conditions (All 5 Rounds)

| Dep. var. = $\mathbb{1}(callback)$ | (1) | (2) | (3) |
|---|---|---|---|
| $\text{older}_i$ x unemployment $\text{rate}_{ct}$ | -0.0158*** | -0.0151*** | -0.0150*** |
| | (0.0045) | (0.0045) | (0.0045) |
| $\text{older}_i$ | 0.0285 | 0.0258 | 0.0267 |
| | (0.0402) | (0.0397) | (0.0393) |
| unemployment $\text{rate}_{ct}$ | 0.0045 | -0.0021 | 0.0021 |
| | (0.0045) | (0.0077) | (0.0123) |
| $\text{public}_{ct}$ | -0.4994*** | | |
| | (0.1706) | | |
| $\text{older}_i$ x $\text{public}_{ct}$ | 0.5617** | 0.5435* | 0.5299* |
| | (0.2757) | (0.2721) | (0.2680) |
| $\text{public}_{ct}$ x unemployment $\text{rate}_{ct}$ | | | -0.0279 |
| | | | (0.0474) |
| $\text{competing}_i$ | -0.0631 | | |
| | (0.0490) | | |
| $\text{older}_i$ x $\text{competing}_i$ | -0.0194 | -0.0194 | -0.0193 |
| | (0.0167) | (0.0164) | (0.0164) |
| $\text{competing}_i$ x unemployment $\text{rate}_{ct}$ | 0.0100 | 0.0118 | 0.0114 |
| | (0.0069) | (0.0084) | (0.0084) |
| Callback rate | .102 | .102 | .102 |
| City FE | | X | X |
| Time FE | | X | X |
| Job postings | 5,445 | 5,445 | 5,445 |
| Resumes | 15,628 | 15,628 | 15,628 |
| City-rounds | 39 | 39 | 39 |
| $R^2$ | 0.005 | 0.011 | 0.011 |

See notes to Table 6. In rounds 1-3 either two younger or two older applications were sent to each job posting. In rounds 4-5, 4 applications were sent to each job posting, at least two of which are older applicants and at least one of which is younger. The dummy variable '$\text{competing}_i$' is a dummy for being part of round 4 or 5 and captures the competition induced by the extra applications in these rounds. The interaction term '$\text{older}_i$ x $\text{competing}_i$' captures the effect of an older applicant competing with at least one younger applicant. Standard errors clustered at the city-round level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# 8 Online Appendix

“Age Discrimination across the Business Cycle”
By Gordon Dahl and Matthew Knepper

Appendix Figures and Tables

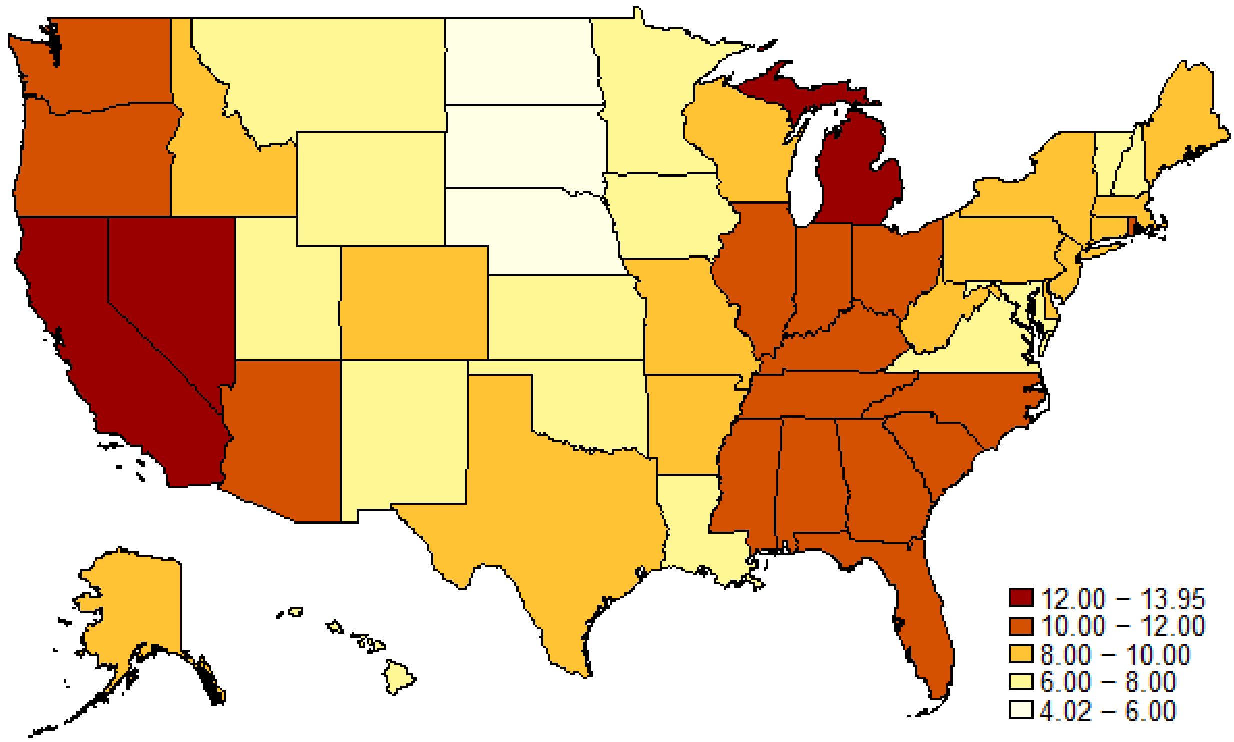



Figure A1: State Unemployment Rates at the Height of the Great Recession

Nonseasonally-adjusted monthly unemployment rates by state in December of 2009, split into quintiles.

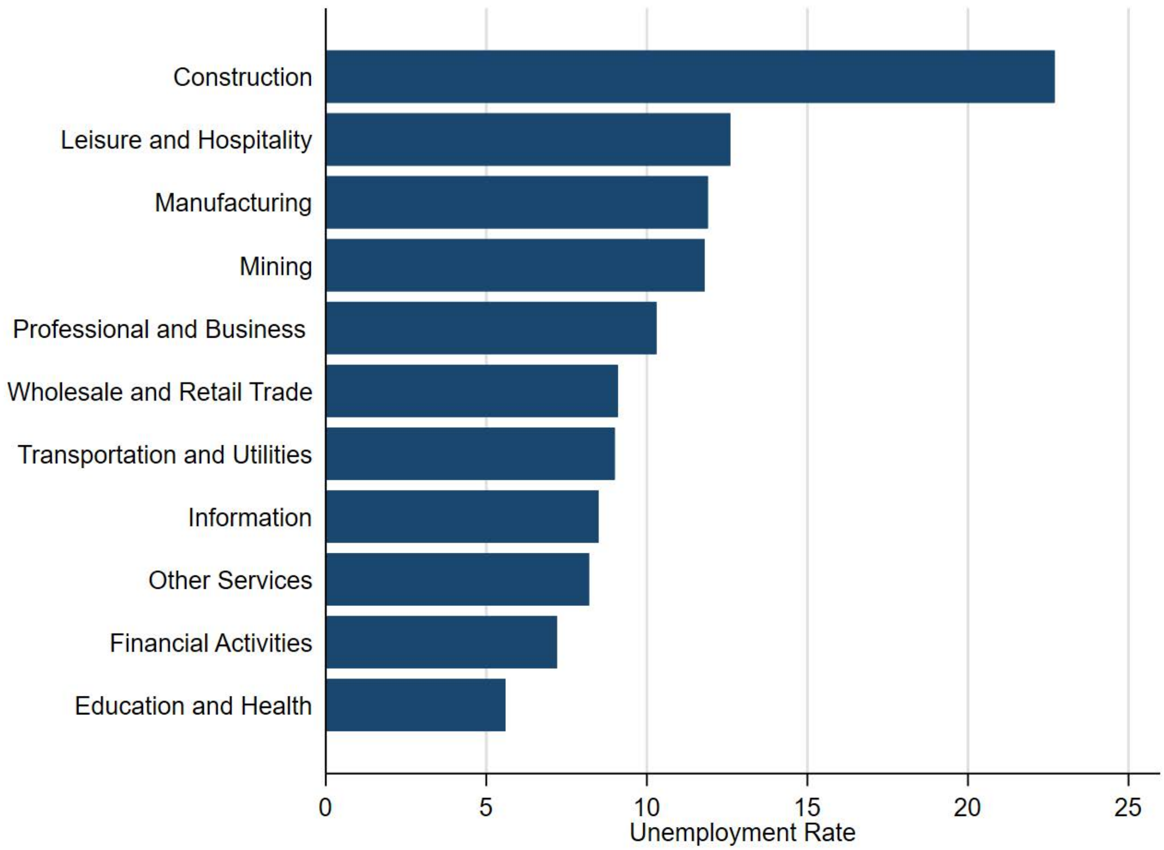
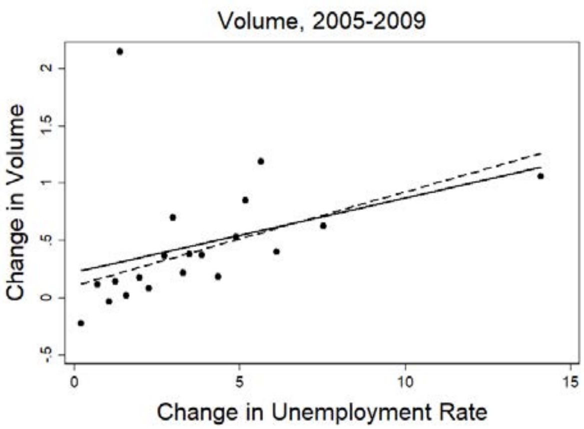
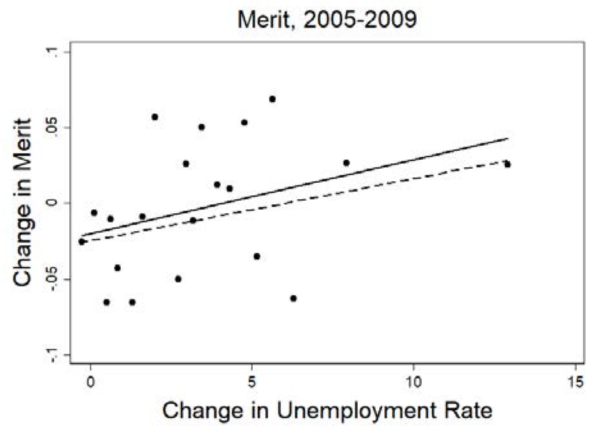



Figure A2: Industry Unemployment Rates at the Height of the Great Recession

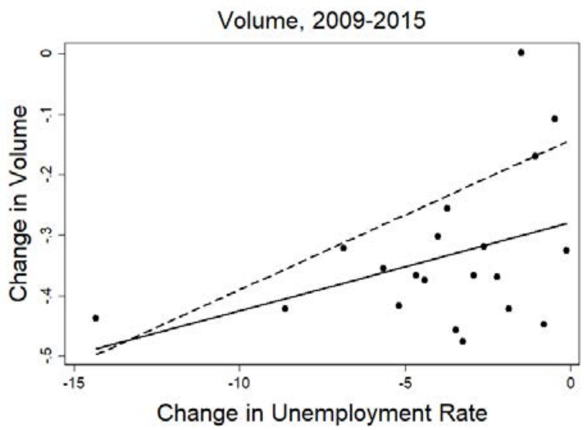
Nonseasonally-adjusted monthly unemployment rates by industry in December of 2009.



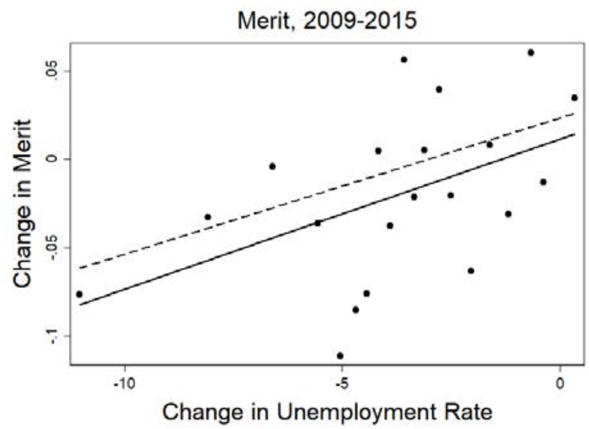

(a) Run-up to Great Recession




(b) Run-up to Great Recession



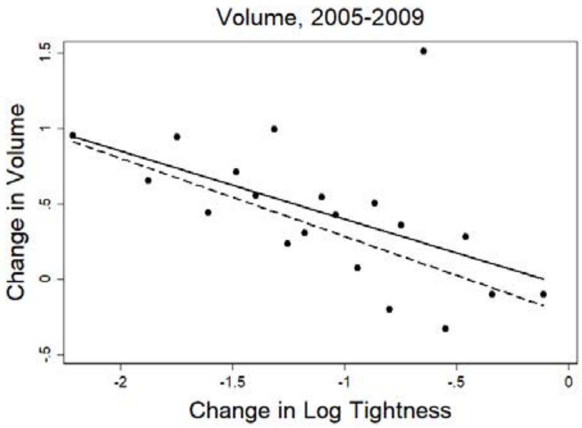

(c) Recovery from Great Recession



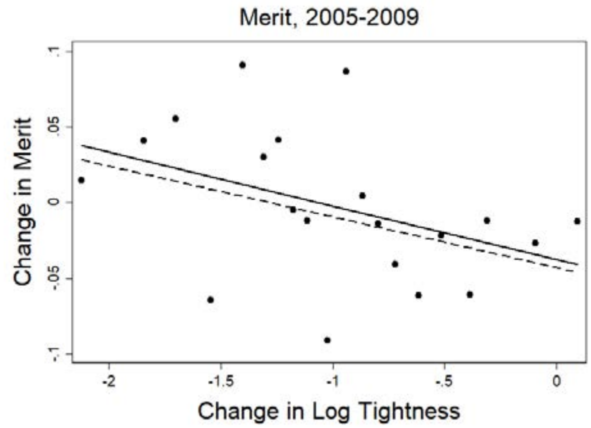

(d) Recovery from Great Recession

Figure A3: ADEA Charges Across the Great Recession (by State-Industry)

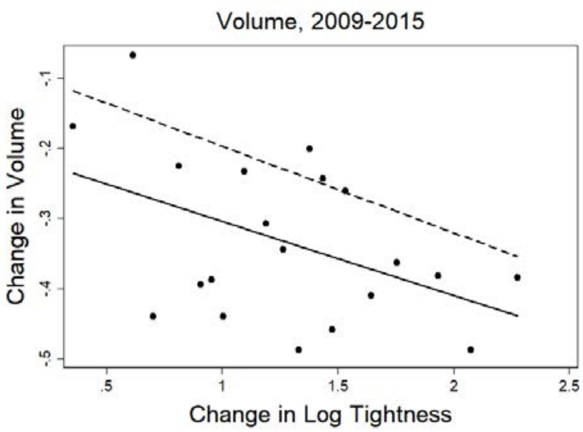
Binned scatter plots with weighted data and 20 equally sized bins are presented. Change in volume is defined as the fractional change in charges relative to the size of each state-industry's labor force. The solid line is the regression line weighted by the size of the state-industry labor force, while the dashed line is unweighted. For the merit graphs, only those state-industries with at least 2 ADEA charges in the pre and post periods are retained. This restriction removes 2.1% and 1.7% of total charges from panels b and d, respectively. Weighted regression line slopes (standard errors) for panels a-d, respectively, are 0.057 (0.018), 0.006 (0.003), 0.014 (0.008), and 0.008 (0.003).



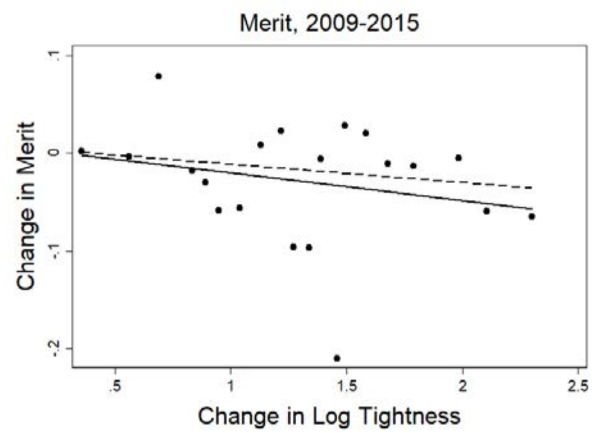

(a) Run-up to Great Recession




(b) Run-up to Great Recession




(c) Recovery from Great Recession




(d) Recovery from Great Recession

Figure A4: ADEA Charges and Labor Market Tightness (by State-Industry)

Binned scatter plots with weighted data and 20 equally sized bins are presented. Log tightness is defined as log job openings-log unemployment. Change in volume is defined as the fractional change in charges relative to the size of each state-industry's labor force. The solid line is the regression line weighted by the size of the state-industry labor force, while the dashed line is unweighted. For the merit graphs, only those state-industries with at least 2 ADEA charges in the pre and post periods are retained. This restriction removes 2.1% and 1.7% of total charges from panels b and d, respectively. Weighted regression line slopes (standard errors) for panels a-d, respectively, are -0.428 (0.093), -0.036 (0.017), -0.105 (0.053), and -0.027 (0.017).


Figure A5: Charges Filed by Firm Size and Claim Quality

The EEOC reports number of employees in the bins used above.

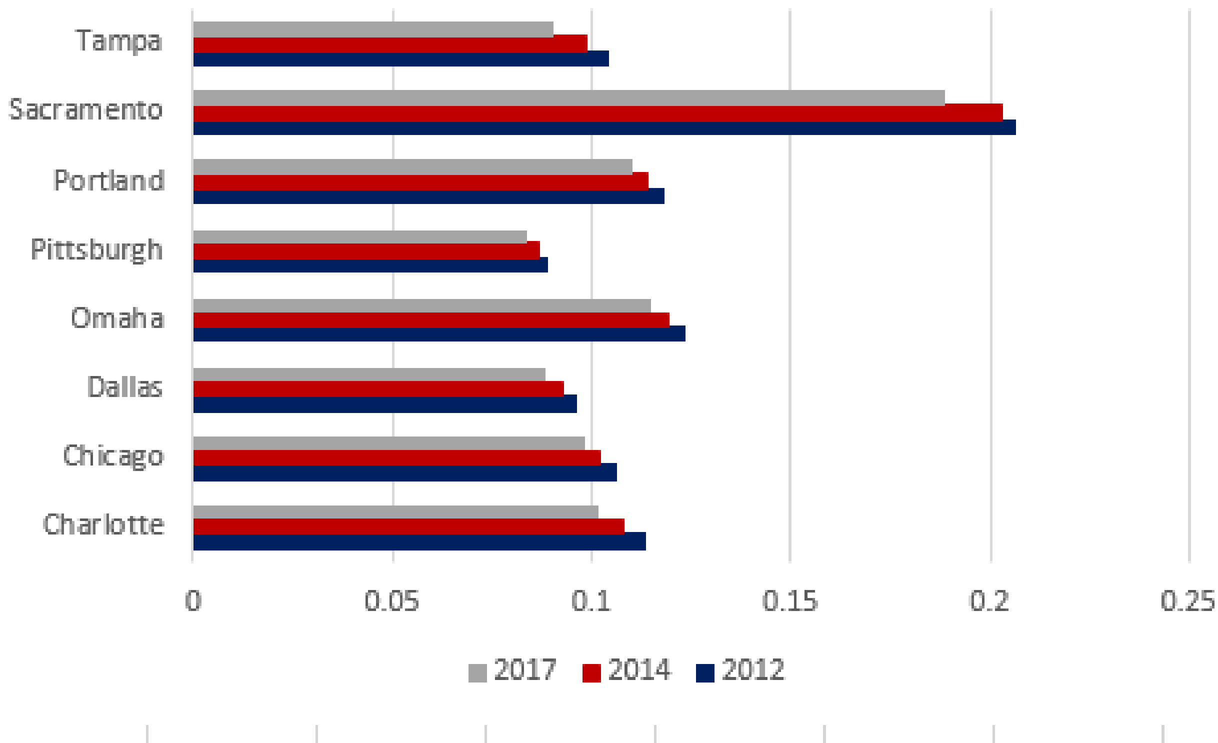



Figure A6: Size of Public Sector, by City and Year

The fraction of each city's workforce that is employed in the public sector based on BEA Regional Employment statistics.

Table A1: ADEA Charges by Type

| | Firing | Hiring |
|---|---|---|
| **Top Basis Categories** | | |
| Retaliation | 0.287 | 0.157 |
| Disability | 0.234 | 0.167 |
| Race-Black | 0.162 | 0.179 |
| Sex-Female | 0.147 | 0.097 |
| National Origin | 0.088 | 0.100 |
| Sex-Male | 0.056 | 0.085 |
| **Top Issue Categories** | | |
| Discharge | 1 | 0.135 |
| Hiring | 0.023 | 1 |
| Terms and Conditions | 0.198 | 0.072 |
| Harassment | 0.168 | 0.031 |
| Discipline | 0.115 | 0.013 |
| Reasonable Accom. | 0.059 | 0.016 |
| Wages | 0.040 | 0.015 |
| Suspension | 0.037 | 0.002 |
| Promotion | 0.036 | 0.037 |
| Demotion | 0.023 | 0.006 |
| Sexual Harassment | 0.020 | 0.004 |
| **Worker/Firm Characteristics** | | |
| Age | 56.0 | 56.0 |
| White | 0.569 | 0.559 |
| Black | 0.241 | 0.257 |
| Female | 0.510 | 0.370 |
| Legal representation | 0.172 | 0.073 |
| Private firm | 0.908 | 0.757 |
| Charges | 67,993 | 11,602 |
| Claims per charge | 4.19 | 3.24 |

ADEA firing and hiring charges filed with the EEOC between 2005 and 2015. Only the most prevalent basis and issue categories are shown. Because the number of claims per charge exceed 1, the fraction of all bases and of all issues need not sum to 1.

Table A2: Charge Volume and Unemployment, Full PDL Model

| | **Firing + Hiring** | | **Firing** | | **Hiring** | |
|---|---|---|---|---|---|---|
| | Base | PDL | Base | PDL | Base | PDL |
| Dep. var. = # of charges | (1) | (2) | (3) | (4) | (5) | |
| $\text{unemployment}_{jst}$ | 1.31*** | 1.22*** | 1.21*** | 1.09*** | 0.10*** | 0.13 |
| | (0.25) | (0.34) | (0.23) | (0.40) | (0.03) | (0.10) |
| $\text{unemployment}_{jst-1}$ | | 0.08 | | 0.60*** | | -0.51* |
| | | (0.41) | | (0.17) | | (0.31) |
| $\text{unemployment}_{jst-2}$ | | -0.62 | | -1.14*** | | 0.52*** |
| | | (0.44) | | (0.31) | | (0.16) |
| $\text{unemployment}_{jst-3}$ | | -0.04 | | 0.17 | | -0.21 |
| | | (0.47) | | (0.33) | | (0.17) |
| $\text{unemployment}_{jst-4}$ | | 0.53 | | 0.44 | | 0.09 |
| | | (0.56) | | (0.49) | | (0.14) |
| $\text{unemployment}_{jst-5}$ | | 0.24 | | 0.41 | | -0.18** |
| | | (0.30) | | (0.30) | | (0.08) |
| $\text{unemployment}_{jst-6}$ | | -0.05 | | -0.33 | | 0.29 |
| | | (0.34) | | (0.40) | | (0.20) |
| **Effect of 1 pp ↑ unemp** | **20.2** | **20.8** | **18.6** | **18.9** | **1.51** | **1.80** |
| Mean(# national charges) | 665.0 | 665.0 | 568.6 | 568.6 | 96.3 | 96.3 |
| % change | 3.0 | 3.1 | 3.3 | 3.3 | 1.6 | 1.9 |
| Elasticity | 0.21 | 0.21 | 0.22 | 0.23 | 0.11 | 0.13 |
| N (state-industry-months) | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 |
| Polynomial degree | | quadratic | | quadratic | | quadratic |
| AIC | 321,274 | 321,113 | 300,064 | 299,924 | 139,744 | 139,682 |
| $R^2$ | 0.469 | | 0.506 | | 0.088 | |

Industry-state-month level regressions for the volume of cases. The sample period spans 2005-2015. Regression coefficients show the change in charges filed per 100,000 increase in the number unemployed. Observations are weighted by the industry share of employment in each state's labor force. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in the national unemployment rate on the national monthly number of charges filed. The PDL model estimates the cumulative effect of previous and contemporaneous unemployment on current period charges using a polynomial distributed lag model; the total effect is the sum of coefficients across all lags. The AIC is used to choose the number of lags; while not shown, the BIC chooses the same lag structure. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Charge Quality and Unemployment, Full PDL Model

| | Firing + Hiring | | Firing | | Hiring | |
|---|---|---|---|---|---|---|
| | Base | PDL | Base | PDL | Base | PDL |
| Dep. var. = $\mathbb{1}(merit)$ | (1) | (2) | (3) | (4) | (5) | (6) |
| $\text{unemployment}_{jst}$ | 17.6*** | -2.6 | 13.0** | -15.0 | 19.5 | 15.6 |
| | (4.62) | (21.4) | (5.78) | (17.0) | (17.4) | (69.0) |
| $\text{unemployment}_{jst-1}$ | | 22.8 | | 24.9 | | 24.8 |
| | | (30.8) | | (30.7) | | (87.4) |
| $\text{unemployment}_{jst-2}$ | | -13.5 | | 15.2 | | -107* |
| | | (41.4) | | (55.9) | | (53.7) |
| $\text{unemployment}_{jst-3}$ | | 57.1** | | 6.48 | | 234** |
| | | (22.4) | | (21.6) | | (98.8) |
| $\text{unemployment}_{jst-4}$ | | -29.8 | | -10.2 | | -100 |
| | | (27.0) | | (27.6) | | (62.2) |
| $\text{unemployment}_{jst-5}$ | | 15.9 | | 50.5** | | -166 |
| | | (22.7) | | (22.0) | | (115) |
| $\text{unemployment}_{jst-6}$ | | -33.5 | | -60.2* | | 116 |
| | | (36.7) | | (32.5) | | (107) |
| age | 0.0015*** | 0.0015*** | 0.0015*** | 0.0015*** | 0.0016*** | 0.0016*** |
| | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0005) | (0.0005) |
| female | 0.0180*** | 0.0180*** | 0.0144*** | 0.0145*** | 0.0301*** | 0.0301*** |
| | (0.0024) | (0.0024) | (0.0025) | (0.0025) | (0.0061) | (0.0061) |
| private | 0.0410*** | 0.0410*** | 0.0413*** | 0.0412*** | 0.0411*** | 0.0408*** |
| | (0.0055) | (0.0055) | (0.0064) | (0.0064) | (0.0094) | (0.0093) |
| **Effect of 1 pp ↑ unemp** | **0.0012** | **0.0012** | **0.0009** | **0.0009** | **0.0013** | **0.0012** |
| Mean(merit) | .167 | .167 | .172 | .172 | .141 | .141 |
| % change | 0.7 | 0.7 | 0.5 | 0.5 | 0.9 | 0.8 |
| Elasticity | 0.04 | 0.04 | 0.03 | 0.03 | 0.05 | 0.05 |
| N (charges) | 78,021 | 78,021 | 67,988 | 67,988 | 11,600 | 11,600 |
| Polynomial degree | | quadratic | | quadratic | | linear |
| AIC | 67,660 | 67,654 | 60,533 | 60,528 | 8,431 | 8,430 |
| $R^2$ | 0.017 | | 0.018 | | 0.042 | |

Individual level regressions for whether a case is determined to have merit. The sample period spans 2005-2015. Regression coefficients on 'unemployment' are multiplied by $10^{-8}$. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in a state-industry's monthly unemployment rate on the fraction of charges found to have had merit. The PDL model estimates the cumulative effect of previous and contemporaneous unemployment on current period charges using a polynomial distributed lag model; the total effect is the sum of coefficients across all lags. The AIC is used to choose the number of lags; while not shown, the BIC chooses the same lag structure. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A4: Charge Volume and Unemployment, Unweighted

| | **Firing + Hiring** | | **Firing** | | **Hiring** | |
|---|---|---|---|---|---|---|
| | Base | PDL | Base | PDL | Base | PDL |
| Dep. var. = # of charges | (1) | (2) | (3) | (4) | (5) | |
| $\text{unemployment}_{jst}$ | 1.96*** | 1.51*** | 1.75*** | 1.35*** | 0.21*** | 0.16 |
| | (0.43) | (0.33) | (0.41) | (0.37) | (0.05) | (0.11) |
| $\text{unemployment}_{jst-1}$ | | -0.05 | | 0.14 | | -0.20 |
| | | (0.30) | | (0.26) | | (0.17) |
| $\text{unemployment}_{jst-2}$ | | -0.01 | | -0.26 | | 0.25* |
| | | (0.26) | | (0.23) | | (0.14) |
| $\text{unemployment}_{jst-3}$ | | 0.40 | | 0.44* | | -0.04 |
| | | (0.32) | | (0.24) | | (0.14) |
| $\text{unemployment}_{jst-4}$ | | -0.25 | | -0.20 | | -0.05 |
| | | (0.43) | | (0.36) | | (0.13) |
| $\text{unemployment}_{jst-5}$ | | 0.28 | | 0.34 | | -0.05 |
| | | (0.22) | | (0.28) | | (0.09) |
| $\text{unemployment}_{jst-6}$ | | 0.11 | | -0.03 | | 0.14* |
| | | (0.37) | | (0.40) | | (0.08) |
| **Effect of 1 pp ↑ unemp** | **30.2** | **30.6** | **27.0** | **27.4** | **3.25** | **3.32** |
| Mean(# national charges) | 665.0 | 665.0 | 568.6 | 568.6 | 96.3 | 96.3 |
| % change | 4.5 | 4.6 | 4.7 | 4.8 | 3.4 | 3.4 |
| Elasticity | 0.31 | 0.32 | 0.32 | 0.33 | 0.23 | 0.23 |
| N (state-industry-months) | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 |
| Polynomial degree | | quadratic | | quadratic | | quadratic |
| AIC | 321,274 | 321,113 | 300,064 | 299,924 | 139,744 | 139,682 |
| $R^2$ | 0.413 | | 0.434 | | 0.070 | |

Regressions parallel Table 3, but are unweighted. Standard errors clustered at the state level.
$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A5: Robustness checks, All ADEA Firing + Hiring Charges

| Dep. var. = # of charges | (1) | (2) | (3) | Volume (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| unemployment$_{jst}$ | 0.72** | 1.40*** | 0.99*** | 0.70*** | | | |
| | (0.35) | (0.32) | (0.20) | (0.21) | | | |
| unemployment$_{st}$ | | | | | 3.09*** | | |
| | | | | | (0.54) | | |
| unemployment rate$_{st}$ | | | | | | 3.01* | |
| | | | | | | (1.56) | |
| emp:pop ratio$_{st}$ | | | | | | | -1.28** |
| | | | | | | | (0.64) |
| **Effect of 1 pp ↑** | **11.08** | **21.55** | **15.26** | **10.83** | **47.59** | **46.35** | **-30.50** |
| Mean(# national charges) | 651.0 | 694.3 | 512.5 | 644.3 | 665.0 | 665.0 | 665.0 |
| % change | 1.7 | 3.1 | 3.0 | 1.7 | 7.2 | 7.0 | -4.6 |
| Elasticity | 0.10 | 0.23 | 0.20 | 0.11 | 0.49 | 0.47 | -2.76 |
| N | 36,261 | 43,404 | 75,015 | 81,561 | 6,120 | 6,120 | 6,120 |
| $R^2$ | 0.400 | 0.548 | 0.492 | 0.333 | 0.905 | 0.704 | 0.693 |
| Dep. var. = $\mathbb{1}(merit)$ | | | | **Merit** | | | |
| unemployment$_{jst}$ | 36.6** | 10.3** | 19.3*** | 15.4*** | | | |
| | (14.0) | (4.97) | (5.20) | (4.01) | | | |
| unemployment$_{st}$ | | | | | 2.28** | | |
| | | | | | (1.11) | | |
| unemployment rate$_{st}$ | | | | | | 0.483** | |
| | | | | | | (0.179) | |
| emp:pop ratio$_{st}$ | | | | | | | -0.390*** |
| | | | | | | | (0.134) |
| **Effect of 1 pp ↑** | **0.0024** | **0.0007** | **0.0013** | **0.0011** | **0.0014** | **0.0048** | **-0.0039** |
| Mean(merit) | .181 | .155 | .169 | .165 | .167 | .167 | .167 |
| % change | 1.3 | 0.5 | 0.8 | 0.7 | 0.8 | 2.9 | -2.4 |
| Elasticity | 0.06 | 0.03 | 0.05 | 0.04 | 0.06 | 0.19 | -1.31 |
| N (charges) | 35,085 | 42,936 | 61,356 | 77,124 | 78,027 | 78,027 | 78,027 |
| $R^2$ | 0.021 | 0.016 | 0.017 | 0.018 | 0.017 | 0.026 | 0.026 |
| 2005-2009Q2 sample | X | | | | | | |
| 2009Q3-2015 sample | | X | | | | | |
| Age 50+ sample | | | X | | | | |
| Event date used | | | | X | | | |

See notes to Tables 3 and 4. Columns 1-4 test sensitivity to different time periods, a different age sample, and using the event date in place of the filing date. Column 5 uses the number unemployed at the state-month level instead of the industry-state-month level. Columns 6 and 7 are rate-on-rate regressions at the state level, where the dependent variable is the number of charges divided by the size of each state's labor force and population, respectively, and the regressions are weighted by each state's labor force and population, respectively. The top-panel coefficients show the change in charges filed per 100,000 increase in the number unemployed (employed). Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in the national unemployment rate on the national monthly number of charges filed.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Robustness check, Exclusion of Construction Industry

| Dep. var. = # of charges | Firing + Hiring (1) | **Volume** Firing (2) | Hiring (3) |
|---|---|---|---|
| $\text{unemployment}_{jst}$ | 1.42*** | 1.31*** | 0.11*** |
| | (0.26) | (0.24) | (0.04) |
| **Effect of 1 pp ↑ unemp** | **21.9** | **20.2** | **1.69** |
| Mean(# national charges) | 650.0 | 555.2 | 94.9 |
| % change | 3.4 | 3.6 | 1.8 |
| Elasticity | 0.24 | 0.24 | 0.12 |
| N (state-industry-months) | 72,885 | 72,885 | 72,885 |
| $R^2$ | 0.469 | 0.509 | 0.088 |
| Dep. var. = $\mathbb{1}(merit)$ | | **Merit** | |
| $\text{unemployment}_{jst}$ | 17.7*** | 13.7** | 16.5 |
| | (4.8) | (5.8) | (16.3) |
| **Effect of 1 pp ↑ unemp** | **0.0012** | **0.0009** | **0.0011** |
| Mean(merit) | .166 | .171 | .140 |
| % change | 0.7 | 0.5 | 0.8 |
| Elasticity | 0.04 | 0.03 | 0.04 |
| N (charges) | 76,263 | 66,381 | 11,430 |
| $R^2$ | 0.017 | 0.018 | 0.042 |

See notes to Tables 3 and 4. Columns (1), (2), and (3) show results for combined firing and hiring, firing, and hiring charges, respectively. The top-panel coefficients show the change in charges filed per 100,000 increase in the number unemployed, with regressions by each state-industry's monthly labor force. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Simple Lag Structure: Volume and Merit

| | | **Volume** | |
|---|---|---|---|
| | Firing + Hiring | Firing | Hiring |
| Dep. var. = # of charges | (1) | (2) | (3) |
| $\text{unemployment}_{jst}$ | 1.30*** | 1.11*** | 0.18 |
| | (0.29) | (0.39) | (0.14) |
| $\text{unemployment}_{jst-1}$ | 0.02 | 0.56*** | -0.54 |
| | (0.47) | (0.20) | (0.33) |
| $\text{unemployment}_{jst-2}$ | 0.01 | -0.47 | 0.47 |
| | (0.61) | (0.42) | (0.21) |
| **Effect of 1 pp ↑ unemp** | **20.5** | **18.6** | **1.66** |
| Mean(# national charges) | 665.0 | 568.6 | 96.3 |
| % change | 3.1 | 3.3 | 1.7 |
| Elasticity | 0.21 | 0.22 | 0.12 |
| N (state-industry-months) | 78,963 | 78,963 | 78,963 |
| $R^2$ | 0.469 | 0.506 | 0.089 |
| Dep. var. = $\mathbb{1}(merit)$ | | **Merit** | |
| $\text{unemployment}_{jst}$ | -12.5 | -21.4 | -1.8 |
| | (22.3) | (18.2) | (63.8) |
| $\text{unemployment}_{jst-1}$ | 30.1 | 31.6 | 52.0 |
| | (29.7) | (30.2) | (76.8) |
| $\text{unemployment}_{jst-2}$ | -2.2 | 5.2 | -31.4 |
| | (21.0) | (29.3) | (45.9) |
| **Effect of 1 pp ↑ unemp** | **0.0013** | **0.0010** | **0.0013** |
| Mean(merit) | .167 | .172 | .141 |
| % change | 0.8 | 0.6 | 0.9 |
| Elasticity | 0.04 | 0.03 | 0.05 |
| N (charges) | 78,020 | 67,988 | 11,600 |
| $R^2$ | 0.017 | 0.018 | 0.042 |

See notes to Tables 3 and 4. Columns (1), (2), and (3) show results for combined firing and hiring, firing, and hiring charges, respectively; the total effect is the sum of coefficients across all lags. The top-panel coefficients show the change in charges filed per 100,000 increase in the number unemployed, with regressions by each state-industry's monthly labor force. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A8: Worker Composition and the Increase in Charge Quality

| | Log(benefit) | $\mathbb{1}(merit)$ | |
|---|---|---|---|
| | (1) | (2) | (3) |
| $\text{unemployment}_{jst}$ | -0.095 | 17.3*** | 42.3*** |
| | (49.3) | (4.50) | (13.3) |
| unemployment×dispersion | | | -100** |
| | | | (43.1) |
| dispersion | | | 0.211*** |
| | | | (0.054) |
| **Effect of 1 pp ↑ unemp** | **-0.0015** | **0.0012** | **0.0007** |
| Mean(dep. var.) | 9.28 | .167 | .172 |
| % change | -0.02 | 0.7 | 0.4 |
| Elasticity | -0.001 | 0.04 | 0.02 |
| Issue and Basis FEs | X | X | |
| Discharges only | X | | X |
| N (charges) | 9,615 | 78,021 | 67,989 |
| $R^2$ | 0.143 | 0.022 | 0.022 |

Regression specifications parallel those of Table 4. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in a state-industry's monthly unemployment rate on the fraction of charges found to have had merit. Column 1 uses the natural log of monetary benefits in discharge cases for which the claimant receives positive compensation. Column 2 adds in fixed effects for the issues and bases included in a case. In column 3, the variable 'dispersion' is the quartile coefficient of wage dispersion (mean = 0.315, sd = 0.063), and we evaluate the effect of a 1 pp increase in unemployment at the mean level of industry wage dispersion. All regressions include state, time, and industry fixed effects and controls for age, female, race, and private firm. Standard errors clustered at the state level.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A9: Charge Volume and Unemployment, Females only

| | Firing + Hiring | | Firing | | Hiring | |
|---|---|---|---|---|---|---|
| | Base | PDL | Base | PDL | Base | PDL |
| Dep. var. = # of charges | (1) | (2) | (3) | (4) | (5) | |
| $\text{unemployment}_{jst}$ | 0.620*** | 0.583*** | 0.584*** | 0.541*** | 0.035*** | 0.042*** |
| | (0.123) | (0.130) | (0.115) | (0.122) | (0.010) | (0.011) |
| **Effect of 1 pp ↑ unemp** | **9.55** | **8.98** | **8.99** | **8.33** | **0.54** | **0.65** |
| Mean(# national charges) | 312.2 | 312.2 | 282.7 | 282.7 | 29.5 | 29.5 |
| % change | 3.1 | 2.9 | 3.2 | 2.9 | 1.8 | 2.2 |
| Elasticity | 0.21 | 0.20 | 0.22 | 0.20 | 0.12 | 0.15 |
| N (state-industry-months) | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 | 78,963 |
| Polynomial degree | | quadratic | | quadratic | | quadratic |
| AIC | 254,190 | 254,135 | 233,357 | 233,304 | 86,635 | 86,629 |
| $R^2$ | 0.390 | | 0.442 | | 0.053 | |

Industry-state-month level regressions for the volume of cases. The sample period spans 2005-2015. Regression coefficients show the change in charges filed per 100,000 increase in the number unemployed. Observations are weighted by the industry share of employment in each state's labor force. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in the national unemployment rate on the national monthly number of charges filed. The PDL model estimates the cumulative effect of previous and contemporaneous unemployment on current period charges using a polynomial distributed lag model; the coefficient reported equals the cumulative effect across all lags and the contemporaneous period. The AIC is used to choose the optimal number of lags, which equals 6 in all cases; while not shown, the BIC chooses the same lag structure. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level, and correspond to the implied cumulative effect in even-numbered columns.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A10: Charge Quality and Unemployment, Females only

| | **Firing + Hiring** | | **Firing** | | **Hiring** | |
|---|---|---|---|---|---|---|
| | Base | PDL | Base | PDL | Base | PDL |
| Dep. var. = $\mathbb{1}(merit)$ | (1) | (2) | (3) | (4) | (5) | (6) |
| unemployment$_{jst}$ | 13.4$^{*}$ | 12.8$^{*}$ | 9.69 | 9.24 | 17.6 | 19.8 |
| | (7.16) | (7.11) | (8.90) | (8.87) | (19.9) | (26.4) |
| age | 0.0012$^{***}$ | 0.0012$^{***}$ | 0.0012$^{***}$ | 0.0012$^{***}$ | 0.0002 | 0.0001 |
| | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0010) | (0.0010) |
| private | 0.0469$^{***}$ | 0.0469$^{***}$ | 0.0511$^{***}$ | 0.0510$^{***}$ | 0.0527$^{***}$ | 0.0523$^{***}$ |
| | (0.0067) | (0.0068) | (0.0073) | (0.0074) | (0.0151) | (0.0151) |
| **Effect of 1 pp ↑ unemp** | **0.0009** | **0.0009** | **0.0007** | **0.0007** | **0.0012** | **0.0013** |
| Mean(merit) | .171 | .171 | .173 | .173 | .153 | .153 |
| % change | 0.5 | 0.5 | 0.4 | 0.4 | 0.8 | 0.9 |
| Elasticity | 0.03 | 0.03 | 0.02 | 0.02 | 0.05 | 0.05 |
| N (charges) | 38,193 | 38,193 | 34,649 | 34,649 | 4,289 | 4,289 |
| Polynomial degree | | quadratic | | quadratic | | linear |
| AIC | 33,904 | 33,903 | 31,168 | 31,166 | 3,426 | 3,426 |
| $R^2$ | 0.022 | | 0.023 | | 0.077 | |

Individual level regressions for whether a case is determined to have merit. The sample period spans 2005-2015. Regression coefficients on 'unemployment' are multiplied by $10^{-8}$. Bolded 'Effect of 1 pp ↑ unemp' is the implied effect of a one percentage point increase in a state-industry's monthly unemployment rate on the fraction of charges found to have had merit. The PDL model estimates the cumulative effect of previous and contemporaneous unemployment on current period charges using a polynomial distributed lag model; the coefficient reported equals the cumulative effect across all lags and the contemporaneous period. The AIC is used to choose the optimal number of lags, which equals 6 in all cases; while not shown, the BIC chooses the same lag structure. All regressions include state, time, and industry fixed effects. Standard errors clustered at the state level, and correspond to the implied cumulative effect in even-numbered columns.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A11: Charge Quality and Unemployment, by Gender and Industrial Gender Mix

| | **Firing** | | **Hiring** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| unemployment$_{jst}$ $\times$ $\mathbb{1}$(female) $\times$ % female$_j$ | | -13.8 | | 124$^{**}$ |
| | | (28.7) | | (53.0) |
| unemployment$_{jst}$ $\times$ $\mathbb{1}$(female) | -3.6 | 3.3 | -0.8 | -65.5$^{**}$ |
| | (5.7) | (15.9) | (13.0) | (28.3) |
| Mean(dep. var.) | .171 | .171 | .134 | .134 |
| N (charges) | 66,421 | 66,421 | 10,032 | 10,032 |
| $R^2$ | 0.018 | 0.018 | 0.046 | 0.047 |

Regression specifications parallel those of Table 4, with the additional controls of % female$_j$, unemployment$_{jst}$, $\mathbb{1}$(female), unemployment$_{jst}$ $\times$ % female$_j$, and $\mathbb{1}$(female) $\times$ % female$_j$. unemployment$_{jst}$ indicates the number unemployed in a state-industry-month cell, $\mathbb{1}$(female) is a dummy variable for whether the charging party is female, and % female$_j$ denotes the fraction of jobs occupied by women for a given NAICS2 code. All regressions include state, time, and industry fixed effects and controls for age, female, race, and private firm. Standard errors clustered at the state level.

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A12: Callback Rates and Labor Market Conditions (Rounds 1-3), Unweighted

| Dep. var. = $\mathbb{1}(callback)$ | (1) | (2) | (3) |
|---|---|---|---|
| $older_i$ x unemployment $rate_{ct}$ | -0.0216** | -0.0204** | -0.0204** |
| | (0.0090) | (0.0090) | (0.0090) |
| $older_i$ | 0.0058 | -0.0006 | 0.0009 |
| | (0.0635) | (0.0635) | (0.0632) |
| unemployment $rate_{ct}$ | -0.0052 | -0.0073 | 0.0236 |
| | (0.0048) | (0.0087) | (0.0154) |
| $public_{ct}$ | -0.3782 | | |
| | (0.2552) | | |
| $older_i$ x $public_{ct}$ | 1.0236*** | 1.0097*** | 0.9897** |
| | (0.3580) | (0.3591) | (0.3573) |
| $public_{ct}$ x unemployment $rate_{ct}$ | | | -0.2021*** |
| | | | (0.0734) |
| Mean(callback rate) | .116 | .116 | .116 |
| City FE | | X | X |
| Time FE | | X | X |
| Job postings | 3,076 | 3,076 | 3,076 |
| Resumes | 6,152 | 6,152 | 6,152 |
| City-rounds | 23 | 23 | 23 |
| $R^2$ | 0.010 | 0.024 | 0.024 |

Regressions parallel Table 6, but are unweighted. Standard errors clustered at the city-round level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Callback Rates and Labor Market Conditions (All 5 Rounds), Unweighted

| Dep. var. = $\mathbb{1}(callback)$ | (1) | (2) | (3) |
|---|---|---|---|
| older$_i$ x unemployment rate$_{ct}$ | -0.0144** | -0.0135* | -0.0134* |
| | (0.0069) | (0.0069) | (0.0070) |
| older$_i$ | 0.0403 | 0.0338 | 0.0348 |
| | (0.0534) | (0.0522) | (0.0516) |
| unemployment rate$_{ct}$ | -0.0037 | -0.0005 | 0.0036 |
| | (0.0046) | (0.0085) | (0.0140) |
| public$_{ct}$ | -0.3350* | | |
| | (0.1811) | | |
| older$_i$ x public$_{ct}$ | 0.4622* | 0.4682* | 0.4547* |
| | (0.2578) | (0.2505) | (0.2469) |
| public$_{ct}$ x unemployment rate$_{ct}$ | | | -0.0276 |
| | | | (0.0534) |
| competing$_i$ | -0.0910 | | |
| | (0.0560) | | |
| older$_i$ x competing$_i$ | -0.0348 | -0.0352 | -0.0352 |
| | (0.0231) | (0.0227) | (0.0227) |
| competing$_i$ x unemployment rate$_{ct}$ | 0.0121 | 0.0217** | 0.0212** |
| | (0.0084) | (0.0085) | (0.0086) |
| Mean(callback rate) | .102 | .102 | .102 |
| City FE | | X | X |
| Time FE | | X | X |
| Job postings | 5,445 | 5,445 | 5,445 |
| Resumes | 15,628 | 15,628 | 15,628 |
| City-rounds | 39 | 39 | 39 |
| $R^2$ | 0.006 | 0.017 | 0.017 |

Regressions parallel Table 7, but are unweighted. Standard errors clustered at the city-round level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$