DID AGE DISCRIMINATION PROTECTIONS HELP OLDER WORKERS WEATHER THE GREAT RECESSION?*

David Neumark
Department of Economics
UCI
3151 Social Science Plaza
Irvine, CA 92697,
NBER, and IZA
dneumark@uci.edu

Patrick Button
Department of Economics
UCI
3151 Social Science Plaza
Irvine, CA 92697
pbutton@uci.edu

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Abstract
We examine whether stronger age discrimination laws at the state level moderated the impact of the Great Recession on older workers. We use a difference-in-difference-in-differences strategy to compare older and younger workers, in states with stronger and weaker laws, before, during, and after the Great Recession. We find very little evidence that stronger age discrimination protections helped older workers weather the Great Recession, relative to younger workers. The evidence sometimes points in the opposite direction, with stronger state age discrimination protections associated with more adverse effects of the Great Recession on older workers. We suggest that during an experience like the Great Recession, severe labor market disruptions make it difficult to discern discrimination, weakening the effects of stronger state age discrimination protections. Alternatively, higher termination costs associated with stronger age discrimination protections may do more to deter hiring when future product and labor demand is highly uncertain.

INTRODUCTION

The Great Recession led to dramatic increases in unemployment rates and unemployment durations for workers of all ages. But unemployment durations of older individuals—both men and women—rose far more dramatically (Figure 1). The relative increase in unemployment durations for older workers indicates that older individuals who became unemployed as a result of the Great Recession, or who were seeking new employment, had greater difficulty becoming re-employed.

[Figure 1 here]

As a consequence, the effects of the Great Recession may pose challenges to longer term reforms intended to increase employment of older workers, such as increases in the Full Retirement Age (FRA) for Social Security. Unemployed workers may be more likely to claim Social Security benefits early (Hutchens, 1999), to forego returning to work, and to seek support from other public programs to bridge the period until age 62 (Autor & Duggan, 2003; Dorn & Sousa-Poza, 2010; Riphahn, 1997).

The increase in unemployment durations for older workers led to speculation that age discrimination played a role.[1] This basic hypothesis that age discrimination may have increased or become more important during and after the Great Recession provides a simple motivation for our analysis. In particular, many states offer stronger protections against age discrimination than the federal Age Discrimination in Employment Act (ADEA). Earlier research found that age discrimination laws boosted employment of older workers (Neumark & Stock, 1999; Adams, 2004). And recent work finds that current stronger state age discrimination protections affect employment of older individuals, in part via increased hiring of older individuals into new jobs (Neumark & Song, 2013).

Motivated by this hypothesis and evidence, we ask whether stronger age discrimination

protections at the state level helped protect older workers during and after the Great Recession. 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.

To answer this question, we present estimates of the effects of age discrimination protections before, during, and after the Great Recession. To summarize the results, we generally find little evidence that stronger age discrimination protections helped older workers weather the Great Recession, relative to younger workers. The negative conclusion is particularly clear for men. Indeed for the subset of cases where we find evidence that stronger state age discrimination protections influenced the effects of the Great Recession, they appear to have made things relatively worse for older men. In particular, state age discrimination laws allowing larger financial damages than the federal ADEA were associated with relatively higher unemployment rates of older men (by about 1 percentage point in the period after the Great Recession) and longer unemployment durations of older men (by about 5.5 weeks during and after the Great Recession).

Similarly, in the period after the Great Recession, older women experienced larger relative declines in the employment-to-population ratio (by about 1.5 percentage points) in states with age discrimination protections that covered smaller firms than the ADEA did. And where larger damages are allowed, there was also a larger relative decline in the hiring rate (by about 0.7 percentage point during the period after the Great Recession). On the other hand, there is some evidence that in states with larger damages older women experienced relatively smaller increases in unemployment durations during the Great Recession (by about 4.7 weeks)—our one finding in which age discrimination laws appear to have protected older workers during the Great Recession.

Where we find that stronger state age discrimination laws were associated with worse outcomes for older workers during and after the Great Recession, we typically find that these laws

were also associated with better relative outcomes for older workers before the Great Recession. Thus, this evidence indicates that in normal times, age discrimination protections can help older workers. However, during an experience like the Great Recession, stronger age discrimination laws can become less productive or even counter-productive for older workers. As discussed more fully in the next section, the change in the effects of these laws may arise because severe labor market disruptions make it more difficult to discern discrimination, or because higher termination costs associated with stronger age discrimination protections do more to deter hiring of older workers when future product and labor demand is uncertain.

THE GREAT RECESSION, AGE DISCRIMINATION, AND STATE AGE DISCRIMINATION PROTECTIONS

What might we expect about the effects of the Great Recession on discrimination against older workers, and how these effects varied in states with stronger state age discrimination protections? Considering first the effects of the Great Recession in isolation, one possibility is that in slack labor markets long queues of job applicants make it less costly for employers to discriminate, because they are not passing up qualified older workers in favor of less-qualified younger workers. This general argument about discrimination and the business cycle goes back to Ashenfelter (1970) and Freeman (1973), and was recently considered by Biddle and Hamermesh (2013). The data are consistent at least with rising perceived age discrimination during and after the Great Recession. As Figure 2 shows, ADEA claims filed with the U.S. Equal Employment Opportunity Commission (EEOC) rose sharply at the beginning of the Great Recession and have remained elevated.[2] Claims also rose during the earlier recession covered in this graph, but subsequently fell more quickly. If age discrimination in fact increased during and after the Great Recession, then we might have expected older workers to fare better in states with stronger age discrimination laws.

[Figure 2 here]

The reality, however, could be more complicated, with stronger age discrimination protections leading to more adverse effects of the Great Recession on older workers. One possibility is that the constraints imposed by stronger age discrimination protections lead to more *pent-up demand* for shifting to a younger workforce (i.e., age discrimination). The labor market turbulence created by severe recessions may make it difficult to distinguish the effects of age discrimination and changing business conditions on employment adjustments, leading employers to act on this pent-up demand during and after severe adverse labor market shocks—with the result that more of this *restructuring* occurs in states with stronger age discrimination protections.[3]

An alternative possibility is suggested by past work on the effects of anti-discrimination laws on hiring and terminations. In particular, some scholars argue that age discrimination laws may reduce hiring of older workers. First, these laws may be ineffective at reducing discrimination in hiring because in hiring discrimination cases it is difficult to identify a class of affected workers, and economic damages can be much smaller than in termination cases (Adams, 2004; Posner, 1995). Second, because the ADEA makes it more difficult to terminate older workers, it may actually discourage their hiring (Bloch, 1994; Lahey, 2008a). Product and labor demand uncertainty during and after the Great Recession may have been sufficiently elevated that employers—in contemplating hiring an older worker—perceived a stronger possibility of wanting to terminate that worker before the worker voluntarily chose to leave. That is, in such a period it is more conceivable that stronger age discrimination protections deterred hiring through the termination cost channel.

RELATED RESEARCH

There are four strands of related prior research. First, existing research provides evidence that age discrimination remains pervasive (Neumark, 2008). Moreover, some research, as well as a good deal of conjecture, suggests that age discrimination in hiring is particularly problematic (Adams,

2004; Hirsch, Macpherson, & Hardy, 2000; Hutchens, 1988; Lahey, 2008b; Posner, 1995).

Second, research establishes that the advent of state and federal age discrimination laws increased employment of protected older workers (Neumark & Stock, 1999; Adams, 2004). More recent evidence indicates that state age discrimination protections that are stronger than the ADEA were associated with increased employment and increased hiring of older workers affected by increases in the Social Security FRA in the last decade (Neumark & Song, 2013). One earlier study, in contrast, suggests that state age discrimination laws hurt older workers (Lahey, 2008a). For the period prior to 1978, before the Department of Labor gave administrative responsibility for ADEA enforcement to the EEOC, Lahey finds little evidence that state age discrimination laws affected older workers. In the subsequent period, however, her evidence suggests that state age discrimination laws reduced employment (weeks worked) and increased retirement of white men older than 50 years of age. She also finds lower hiring rates for older workers where there are state laws, leading her to conclude that age discrimination laws hurt older workers by deterring hiring.

There are some problems, however, with this evidence and its interpretation. Lahey studies the existence of state laws, rather than focusing on specific features of these laws. All that the existence of a state law, per se, implies is a longer statute of limitations for filing a claim. However, Neumark and Song (2013) found that variation in the length of statutes of limitations had no impact. Second, we ultimately find evidence that during recessions age discrimination laws may have more adverse effects on older workers. Lahey's post-1977 period extends to 1991, and hence includes two recessions in the early 1980s—during the second of which unemployment rates went higher than in the Great Recession—and another in the early 1990s. Thus, the results may not, in fact, be so contradictory, even putting aside the issue that the studies do not look at the same features of age discrimination laws.

Moreover, Lahey potentially ignores evidence that may point to a different conclusion (see

Neumark, 2008, for a full discussion). If we accept Lahey's characterization of the federal law as becoming effective (to a large extent) in 1978, then there is an important source of identifying information that she ignores—namely, the extension of the federal law to states without anti-discrimination laws in 1978. Her evidence shows that between the pre-1978 and the 1978 to 1991 periods, hours of workers over 50 years of age fell in states with their own age discrimination laws, relative to the states without their own laws; there was no such change for those aged 50 and under. This implicit difference-in-difference-in-differences estimator suggests that when the federal law became more effective, relative employment of those older than age 50 increased precisely in the states that did not previously have state age discrimination laws. This would seem to imply that age discrimination laws—at least the federal law—boosted employment of protected workers, contrary to Lahey's conclusions.

Thus, our view is that while there have been conjectures that age discrimination laws can be harmful to older workers, and in particular reduce hiring, there is little compelling evidence to support this conjecture, and some evidence to the contrary. We obtain additional evidence on this question, including potential differences in the effects of stronger age discrimination protections over the business cycle.

A third strand of related work studies effects of the Great Recession on older workers. Gustman, Steinmeier, & Tabatabai (2011) find little impact on flows into retirement, although their data go only through 2010 and the labor market for older workers worsened subsequently (see, for example, Figure 1). Rutledge and Coe (2012) estimate the effect of the national unemployment rate during the Great Recession on early Social Security benefit claiming, estimating sizable impacts. Bosworth (2012) studies the impact of the Great Recession on retirement decisions of older workers, contrasting the push into retirement from job loss with an increased incentive to work longer stemming from financial losses, and concludes that the job loss

effect in increasing retirement is stronger.

Focusing on age differences, Munnell, Muldoon, and Sass (2009) note that the increase in unemployment rates for older men relative to younger men was higher (for the December 2007 to December 2008 period they study) than in past recessions, when unemployment rates for younger men rose much more sharply than unemployment rates for older men. They ascribe this to a decline in labor market protections for older workers stemming from the reduced employment share in manufacturing—a sector with considerable protections for more-senior workers—and reductions in the tenure of older workers relative to younger workers—implying less of a specific human capital advantage for older workers that would make firms less likely to lay them off.

Finally, although not the focus of their paper, Davis and von Wachter (2011) report estimates of the earnings loss associated with displacement, disaggregated by age, as well as whether the displacement occurred during a recession. They show that the losses are far larger for older workers (aged 51 to 60), especially in relative terms since their counterfactual non-displacement earnings are higher. However, comparing displacements between recessions and other periods, the relative cost of displacement during recessions is more modest for older workers than for younger workers.

Thus, the existing research suggests that there is age discrimination, that age discrimination laws have some beneficial effects for older workers, and that the Great Recession adversely affected older workers more than other workers. However, no existing research ties these phenomena together to ask whether age discrimination laws mitigated (or otherwise influenced) the effects of the Great Recession on older workers.

DATA

We rely primarily on two data sources: the Current Population Survey (CPS) and the Quarterly Workforce Indicators (QWI). The CPS data provide estimates of the unemployment rate, the

employment-to-population ratio, and unemployment durations, while the QWI data measure hiring and separations.

Current Population Survey (CPS)

The CPS monthly micro-data were used to construct estimates by state, month, age group, and gender, of the unemployment rate, the employment-to-population ratio, and median unemployment duration.[4] Population weights were used to create statistics that are representative of the populations within each state, age group, gender, and month cell.

The age groups we use are younger individuals (ages 25 to 44) and older individuals (ages 55 and older). There are two issues here: the appropriate control group for older workers, and how to define older workers. The federal ADEA applies to those aged 40 and over, while some state laws extend to younger workers. In that sense our younger (25 to 44) age group is not the ideal control. However, we chose this age range to match what is available in the QWI data, which are reported aggregated by age. We also regard it as relatively unlikely that there is much age discrimination faced by those aged 40 to 44. In defining older workers as 55 and older, we focus on ages for which policy reforms are attempting to increase attachment to the labor force and lengthen work lives. However, we report results showing that the conclusions are not sensitive to using different age ranges to define both older and younger workers.

Table 1 presents summary statistics for the CPS from 2003 to 2011 by age group and gender, weighted by state population.[5] The weighted estimates lead to estimates that are representative of the population. In the analysis that follows, we focus on the weighted estimates for precisely this reason; they are more informative about what age discrimination laws imply for the effects of the Great Recession on the U.S. labor force.[6] We do, however, also discuss the sensitivity of our results to using unweighted estimates.

[Table 1 here]

Unemployment rates are higher for younger individuals than older individuals, for both men and women (by 1.6 percentage points for men, and 1.8 percentage points for women), and unemployment rates are also lower for women (for both age groups). To some extent, the former difference likely reflects the subjective nature of unemployment, as older individuals who cannot find work may be more likely to leave the labor force. The employment-to-population ratios similarly show that younger men and women are more likely to be employed. In contrast to unemployment rates, durations are much higher for older than younger workers; median duration is higher by 7.8 weeks for older men, and by 6.7 weeks for older women.

Quarterly Workforce Indicators (QWI)

The QWI-based estimates of hiring and separations are based on quarterly data by age, gender, and state.[7] The QWI provides data in age groups bins, so the younger group is generated by summing ages 25 to 34 and ages 35 to 44, and the older group is generated by summing ages 55 to 64 and ages 65 to 99, separately by gender. QWI data became available for different states at different times and are updated for each state at different times.[8] To create a balanced panel, we use data from 2004:Q2 to 2011:Q4, excluding DC for which data are not available until 2005:Q3.[9] We divided hires and separations by the average state employment level from the QWI in 2005 to normalize our measures as rates, rather than levels that would reflect state population; we use employment levels for each of the two age groups, and for men and women separately. We chose to use 2005 because it is the first full year for which the QWI data are available. We wanted to fix the base year so that the denominator of the hiring (and separation) rate would not be influenced by changes in the employment level, which could itself be influenced by the variables we study. There is a slight risk that the base becomes less accurate as time moves forward because of changes in the age composition of the population. But given the relatively short sample period this seems unlikely to matter much.

Table 2 presents summary statistics for the QWI by age group and gender. Not surprisingly, the hiring rate (as we define it) is higher for younger than for older workers, for both men and women. The hiring rate is slightly higher for men than for women in both age groups.

[Table 2 here]

The descriptive statistics for separation rates are very similar. Because we cannot distinguish voluntary and involuntary separations in the QWI data (see Abowd et al., 2009, p. 208), it is difficult to interpret results for separations.[10] For example, if age discrimination laws are associated with fewer separations for older workers during the Great Recession, is that because the laws lead to relatively fewer layoffs (i.e., more protection), or because the laws make it harder to get hired (less protection), so people do not leave jobs as readily? Our descriptive statistics and later results for separations largely mirror those for hiring, suggesting that the relationships we estimate between the Great Recession, age discrimination laws, and separations largely reflect effects on voluntary separations.

State Age Discrimination Laws

Data on age discrimination laws at the state level compiled for Neumark and Song (2013) are used here. We focus on two features of state age discrimination laws that were found in that research to be effective: lower firm-size minimums for the applicability of state age discrimination laws, and larger damages than under the federal ADEA.

The firm-size minimum specifies the minimum firm size that is bound by the state age discrimination law. The ADEA applies to firms with 20 or more workers, but many states have lower minimums. Age discrimination laws are stronger—covering more workers—the lower this minimum firm size.[11] Figure 3 shows the minimum firm size required for each state as of 2003. Following Neumark and Song, we categorize states as having a lower (fewer than 10) or higher (10 or more) firm-size minimum.[12]

[Figure 3 here]

Larger potential damages are likely to arise when the state age discrimination laws go beyond those of the federal law by providing compensatory or punitive damages, whether or not proof of intent or willful violation is required. In 2003, there were 29 states (plus DC) with larger potential damages (henceforth *larger damages*). These are shaded in Figure 3. There were no changes to this classification of states during our sample period.

METHODS

To infer how stronger state age discrimination laws affected the impact of the Great Recession on older versus younger workers, we need to isolate the effects of these laws from other influences that affect outcomes for these two age groups. These other influences can include differences that persist over time and across states. For example, we clearly want to control for average differences between, say, unemployment rates for older and younger workers. In addition, there may be some age-related differences that vary across states, perhaps because of differences in industrial composition, the actual demographic makeup of the broad age groups we use, and other policy differences. Finally, it is possible that the economic shocks caused by the Great Recession differed for older and younger workers nationally, as well as by state, or that policy changes adopted because of the recession had differential impacts.

To control for these confounding factors, we employ a difference-in-difference-in-differences (DDD) empirical strategy. In our case, we have four groups: (1) older individuals in states with stronger laws, (2) older individuals in states with weaker laws, (3) younger individuals in states with stronger laws, and (4) younger individuals in states with weaker laws. (We also have two classifications of stronger and weaker laws, as noted above, but we ignore that variation for this discussion.) Moreover, we compare differences between these four groups in periods during and after the Great Recession to before the Great Recession—which is our third level of

differencing—to ask how the impact of the Great Recession on older versus younger workers depended on state age discrimination laws.

We are also interested in differences, associated with stronger state age discrimination protections, in labor market outcomes for older and younger workers in the period prior to the Great Recession. The coefficients that identify these differences also emerge from the DDD estimates, unless we saturate the model so much so as to absorb pre-recession differences by age and state; we discuss this point later, and present results with and without this added level of saturation.

For the statistical analysis, we need to specify pre- and post-Great Recession periods. Based on NBER recession dates, we define the Great Recession as covering 2007:Q4 to 2009:Q2 for the quarterly QWI data, and December 2007 to June 2009 for the monthly CPS data. We chose to consider the recession period itself and the ensuing period separately, because labor market changes often lag the output changes that define recessions.[13] In addition, we might expect the data to be more reflective of the influence of age discrimination laws in the period following the large layoffs that occurred at the height of the Great Recession, when the very strong influence of product demand shocks probably dominated everything else. This implies that we have two DDD estimators—one pertaining to the Great Recession period relative to the earlier pre-recession period, and the other pertaining to the post-Great Recession period relative to the same pre-recession period.

We start with the basic DDD model that does not include other controls, except for seasonal differences:

$$Y_{ast} = \beta_0 + \beta_1 OLD_a + \beta_2 LAW_s + \beta_3 OLD_a \times LAW_s + \beta_4 GR_t + \beta_5 Aft_t + \beta_6 OLD_a \times GR_t + \beta_7 OLD_a \times AfterGR_t + \beta_8 LAW_s \times GR_t + \beta_9 LAW_s \times AfterGR_t \quad (1)$$

$$+\boldsymbol{\beta_{10}OLD_a \times LAW_s \times GR_t + \beta_{11}OLD_a \times LAW_s \times AfterGR_t}$$

$$+SA_t\lambda_1 + SA_t \times OLD_a\lambda_2 + SA_t \times LAW_s\lambda_3 + SA_t \times OLD_a \times LAW_s\lambda_4 + \varepsilon_{ast},$$

where the subscript a indexes the age group—younger (25 to 44) or older (55+)—s indexes the state, and t indexes time. Y_{ast} is the outcome variable, *OLD* equals 1 for the older group and 0 for the younger group, *GR* is a dummy for the period of the Great Recession, *AfterGR* is a dummy for the period after the Great Recession, and *LAW* is a dummy variable, varying across analyses for the two indicators of stronger state age discrimination laws. We do not seasonally adjust the data used in the regressions, but instead include calendar-month (CPS) or calendar-quarter (QWI) dummy variables—denoted *SA* in equation (1)—and their interactions with *OLD*, *LAW*, and *OLD* × *LAW*.[14]

The DDD parameters, which are of prime interest, are β_{10} and β_{11}; the corresponding terms in equation (1) are highlighted in boldface. β_{10} captures the effect of stronger age discrimination laws on older versus younger workers during the Great Recession compared to before, while β_{11} captures the same type of effect, but for the period after the Great Recession compared to the same pre-recession baseline. For example, suppose our dependent variable is the hiring rate. A positive coefficient on β_{10} (β_{11}) would indicate that age discrimination laws boosted the *relative* hiring of older workers during (after) the Great Recession, relative to the period prior to the recession (which means 2003 through November 2007 for the CPS data, and 2004:Q2 to 2007:Q3 for the QWI data).

As discussed earlier, we are also interested in β_3, which captures the differential effect of stronger age discrimination laws on older versus younger workers in the baseline period. At the same time, we might be less confident in a causal interpretation of this parameter because it is identified solely from variation in laws across states. For example, it is possible that stronger laws prevailing in the baseline pre-recession period were adopted in response to longer term labor

market differences between older and younger workers. In contrast, with age discrimination laws almost universally fixed over our sample period, the variation that identifies β_{10} and β_{11}, which is induced by the Great Recession, is quite clearly exogenous.

The identification argument regarding the DDD parameters is even more compelling in the specifications we estimate that more flexibly saturate the model. First, we add a full set of interactions between state dummy variables and dummy variables for each unique month (or quarter for the QWI data) in the sample. And second, we add a full set of interactions between the age categories and dummy variables for each month (quarter). Together, these interactions subsume the *OLD*, *LAW*, *GR*, *AfterGR*, *GR* × *OLD*, *AfterGR* × *OLD*, *GR* × *LAW*, and *AfterGR* × *LAW* variables in equation (1). These interactions allow an arbitrary (and hence much less constrained) pattern of changes over time by state in the dependent variables common to both older and younger workers, and allow for arbitrary changes by age over time, common to all states. They also let the baseline intercept vary by state, rather than allowing only a difference between states based on whether or not the state has a stronger age discrimination law.

With these detailed interactions added, the three variables from equation (1) that remain are the two triple interactions of most interest—*GR* × *OLD* × *LAW* and *AfterGR* × *OLD* × *LAW*. In addition, because we have not added interactions between the dummy variables for age and state, the *OLD* × *LAW* interaction also remains. We chose to focus on this saturated specification (without the age-by-state interactions) so that we could still identify the baseline pre-Great Recession difference between labor market outcomes for older versus younger workers.[15]

In addition to the dummy variable interactions, we add two control variables. The first captures extensions to the number of weeks of unemployment insurance (UI) available due to automatic increases from the Extended Benefits program and the Emergency Unemployment Compensation program created in June 2008. These UI increases are linked to decreases in the

likelihood of exiting unemployment, leading to higher unemployment rates (Rothstein, 2011) and longer unemployment durations (Farber and Valletta, 2013). We use data on the number of extra UI weeks available from Farber and Valletta (2013). We account for lagged labor market effects of these extensions by including lags of this variable through two years. The current and lagged values are entered interacted with *OLD*. Because we add them to the more-saturated model, the state-by-month (quarter) interactions subsume the effects of these controls on the reference younger group.

The second control accounts for the possibility that the economic shocks caused by the Great Recession had differential impacts on older and younger workers that vary by state. If we look at the correlations across two-digit NAICS industries between employment growth during or after the Great Recession, and the baseline (2003) ratio of older to younger workers, the correlations for the period of the Great Recession are −0.17 for men and −0.07 for women. For the period after the Great Recession, the corresponding correlations are 0.07 and 0.04.[16] Thus, to a limited extent, industries hit hardest during the Great Recession tended to employ a greater share of older workers, and the recovery was a bit stronger for these industries.

We want this control to be an exogenous measure of the age composition of employment demand shocks by state. We therefore construct it using information on national changes in employment coupled with the baseline age composition of industry employment in each state, as explained in Appendix A.[17] Again, this variable and all its lags are entered interacted with *OLD* in the more-saturated model.

With the interactions and controls added, our specification becomes (retaining the coefficient subscripts from equation [1])

$$
\begin{aligned}
Y_{ast} = \beta_0 + \ & \beta_3 OLD_a \times LAW_s \\
& \boldsymbol{+\beta_{10} OLD_a \times LAW_s \times GR_t + \beta_{11} OLD_a \times LAW_s \times AfterGR_t} \\
& + State_s \times Time_t \gamma + Age_a \times Time_t \delta \ + \textstyle\sum_{k=t}^{t-m} X_k \times OLD_a \pi_k \\
& + SA_t \times OLD_a \times LAW_s \lambda_4 + \varepsilon_{ast},
\end{aligned}
\tag{2}
$$

where *Time* is a vector of month dummy variables for the CPS data and quarter dummy variables for the QWI data, *State* is a vector of state dummy variables, and *Age* is vector of the age dummy variables.[18] *X* includes the UI and age composition controls, and is entered up to *m* lags (24 monthly lags with the CPS data, and eight quarterly lags with the QWI data).

RESULTS

Our analysis focuses on whether stronger state age discrimination protections led to relatively better or relatively worse outcomes for older workers during and after the Great Recession. We suggested reasons the effects could go in either direction. Our analysis also provides information on whether stronger state age discrimination protections were associated with differences in labor market outcomes for older versus younger workers in the baseline period prior to the Great Recession.

For each outcome, we first provide information on the DDD estimates in a non-parametric fashion (albeit with no controls) by graphing, for each dependent variable, the time-series of the difference-in-differences between older and younger workers, for states with stronger versus weaker laws. Comparing these series across time is informative about how the influence of age discrimination laws on older versus younger workers varied between any two periods; that is, the comparison between any two points of time is the DDD estimate between those periods. Of most interest are comparisons of the periods during and after the Great Recession with the period prior to its onset—which we also estimate with our regression model.

We then turn to the regression estimates of equations (1) and (2), which enable a sharper focus on the estimated differences between older and younger workers across states before, during, and after the Great Recession, and permit statistical inference on the differences of interest. In addition, of course, the regressions allow us to include the other control variables.

Unemployment Rates

In Figure 4, for unemployment rates and lower firm-size minimums, we show the full set of graphs that build up to the time-series graphs of the difference-in-differences, to illustrate the construction of the latter. Subsequently, we only show the difference-in-differences graphs. The first row of graphs presents seasonally adjusted time-series of unemployment rates for each of the four groups defined by age and whether or not there was a lower firm-size minimum.[19] The shaded region in each graph highlights the dates of the recession, based on NBER dates. The second row of graphs shows the difference in the time-series between older and younger workers, for states with stronger laws and weaker laws. And the third row of graphs shows the difference between these. As noted above, the latter graphs provide a difference-in-difference estimate at each point in time.

[Figure 4 here]

The top panels of Figure 4 show that, for both genders and both sets of states, unemployment rates—which were initially higher for younger than for older workers—rose substantially more for younger workers during the Great Recession (indicated by the shaded region), and remained elevated in relative terms for younger workers in the subsequent years shown. Thus, the Great Recession did not increase unemployment rates as much for older workers as for younger workers.

The second row in the figure displays the differences between unemployment rates of younger and older workers depending on whether there was a lower firm-size minimum, to make

it easier to see how the Great Recession affected older versus younger workers in each group of states. As the left-hand panel shows, the relative increase in the unemployment rate of younger men during the Great Recession was larger in states with a lower firm-size minimum. (The lines are in negative territory because unemployment rates increased by less for older than for younger workers.) However, the pattern reverses for some part of the period after the Great Recession (most notably beginning in 2011), with—in relative terms—larger increases in the unemployment rates of older men in the states with a lower firm-size minimum. For women the pattern is different, with the main indication being that a lower firm-size minimum was associated with relative increases in unemployment rates for older workers in the period after the Great Recession. In the middle graphs, there is little indication that unemployment rates for older workers relative to younger workers were much different in states with a lower firm-size minimum in the pre-recession period.

These differences are displayed yet more clearly in the bottom row of the figure, which shows the difference-in-differences estimates. In these panels a negative (positive) value indicates that a lower firm-size minimum is associated with smaller (larger) increases in unemployment among older workers relative to younger workers. For men, therefore, we see that during the Great Recession the line is almost always in negative territory, although often not by much. Subsequent to the Great Recession, however, the evidence is less clear. And for women the sharpest result appears to be for the period after the Great Recession, during which the stronger age discrimination protection is associated with higher relative unemployment of older workers.

In Figure 5, we turn to the same kind of evidence, but focusing on the other type of age discrimination protection—larger damages. We report only the time-series of the difference-in-differences estimates, corresponding to the bottom row of Figure 4. For men there is essentially no evidence that a state age discrimination law allowing for larger damages resulted in relatively

lower unemployment rates for older workers. Before the Great Recession there is little apparent difference. During the Great Recession the pattern is not consistent, although for most months the relative unemployment rate of older workers was higher in states with larger damages. In the period after the Great Recession there is rather clear evidence that relative unemployment rates for older workers were higher in states with larger damages under state law—especially the first 18 months or so after the Great Recession ended. For women this negative conclusion appears stronger; during most of the Great Recession period, and for the entire post-recession period, unemployment rates were higher for older relative to younger workers in the states with larger damages. However, the size of the gap is generally smaller than for men.

[Figure 5 here]

Thus, for unemployment rates, across both Figures 4 and 5 there is relatively little indication that stronger state age discrimination protections insulated older workers from increases in unemployment rates in the periods during and after the Great Recession. Indeed the most pronounced evidence appears to be in the opposite direction—for women with regard to lower firm-size minimums, and for both men and women for the stronger age discrimination protection in the form of larger damages.

The regression estimates, which are reported in Table 3, confirm these impressions. Columns 1 to 4 report the results for lower firm-size minimums, and columns 5 8 for larger damages. In each case, the first two columns are for men and the next two columns are for women; in each case we first report the estimates of equation (1), and then the more-saturated model with controls (equation (2)).

[Table 3 here]

In the estimates for firm-size minimums for men (column 1), first consider, as a preliminary, the evidence regarding some of the main effects. The estimated coefficient for *OLD*

implies that the baseline (pre-Great Recession) difference in unemployment rates is about a percentage point lower for older men, consistent with the usual finding that older workers have lower unemployment rates. The estimated coefficients on *GR* and *AfterGR* measure the differences in unemployment rates for the reference group of younger workers during and after the Great Recession. The differences, of course, are pronounced—about 2 percentage points higher during the Great Recession, and 5 percentage points higher in the subsequent period. The following two rows, for *GR* × *OLD* and *AfterGR* × *OLD*, show the differential effects of the Great Recession on unemployment rates of older workers. Consistent with what we saw in Figures 4 and 5, these estimates are negative, indicating that unemployment rates rose by less for older workers—by about 1 percentage point.

The main estimates of interest are highlighted in the top three rows. First, the estimated coefficient of *OLD* × *LAW* is the baseline difference in the relative unemployment rate of older versus younger workers in states with a stronger age discrimination protection. The estimated coefficient is negative, consistent with a lower firm-size minimum lowering unemployment of older workers in the pre-recession period, but the estimate (–0.14) is small and statistically insignificant.

Finally, the DDD parameters are the coefficients of *GR* × *OLD* × *LAW* and *AfterGR* × *OLD* × *LAW*. These estimates capture the differential effects of the Great Recession on unemployment rates of older versus younger workers, across states with and without a lower firm-size minimum. These estimates can be interpreted as estimating the change in the graphs in the bottom panels of Figure 4 from before the Great Recession to two subsequent periods—the Great Recession itself, and the period following the Great Recession. As column 1 shows, in this case both estimates are small and statistically insignificant, paralleling the ambiguous evidence for men in Figure 4.

In column 2 we enrich the specification by adding the state-by-month and age-by-month interactions, and the control variables for UI benefits and the age composition of demand. As explained earlier, with the rich interactions added, the coefficients of most interest—on *OLD* × *LAW*, *GR* × *OLD* × *LAW*, and *AfterGR* × *OLD* × *LAW*—remain identified. As column 2 shows, the estimates are essentially unchanged.

Note that the UI extensions are not associated with differential effects on unemployment rates of older workers, as the estimated coefficient (0.01) is very small and insignificant.[20] The estimated coefficient of the age composition control interacted with OLD is negative, but not significant; the negative sign is expected since this control indicates that if national trends in industry employment were favorable to older workers in the state, their unemployment rate would rise by less. The estimated sum of the coefficients is very large, but recall that these coefficients reflect a 1 percentage point differential between the predicted growth rate of employment for older versus younger workers that persists for two years. When we look at the individual regression coefficients, we find much smaller effects for any one period, and the effects dissipate within two years.[21]

Columns 3 and 4 turn to women. As shown in column 3, the baseline unemployment rate difference between older and younger women—the coefficient on *OLD*—is larger than for men (1.62 percentage points, versus 1.03 for men). The estimated coefficients for *GR* and *AfterGR* show that the Great Recession had a smaller impact on unemployment rates of younger women than of younger men.[22] Turning to the DDD estimates, the positive point estimates for the post-recession period (*AfterGR*) are larger for women than for men, consistent with Figure 4. But the estimate is insignificant.

Overall, the estimates in columns 1 to 4 of Table 3 do not provide evidence that a lower firm-size minimum for the applicability of state age discrimination laws had a statistically

significant impact on the influence of the Great Recession on the relative unemployment rates of older men or women. Certainly there is no statistical evidence that this variant of state age discrimination laws led to smaller increases in unemployment. Indeed for women the point estimates for the period after the Great Recession suggest if anything the opposite.

Columns 5 to 8 turn to the same specifications, but looking at stronger age discrimination protections in the form of larger damages. Recall that Figure 5 gave a stronger indication that this age discrimination protection worsened the effects of the Great Recession on older workers. Having gone through columns 1 to 4 in detail, we can summarize the results in columns 5 to 8, and the tables that follow, much more quickly.

First, as reflected in the estimated coefficients of *OLD* × *LAW*, for the period prior to the Great Recession there is no evidence of differential unemployment rates for older workers in states with larger damages. For the post-Great Recession period, which is the period when unemployment rates peaked, the DDD estimates of *AfterGR* × *OLD* × *LAW* for men are positive, very close in magnitude—around 1—and statistically significant at the 1-percent level. For women the estimates are about 0.5, but not statistically significant. For both men and women the estimated coefficients of *GR* × *OLD* × *LAW*, which capture the differential effects in states with stronger damages during the Great Recession, are positive (around 0.55 for men, and 0.37 for women), but not statistically significant. The positive estimates in all cases imply that where state age discrimination laws provided for larger damages, unemployment rates for older workers rose more in relative terms after the Great Recession, but the results are statistically significant only for men in the period after the Great Recession, for whom there was a relative increase in the unemployment rate of about 1 percentage point.

In contrast, the estimated coefficient of *OLD* × *LAW* is negative for men, consistent with larger damages lowering unemployment rates of older workers prior to the Great Recession (by

about 0.25 percentage points), although recall our earlier caveat that identification of this parameter is less compelling. However, the estimated differential for men prior to the Great Recession is not statistically significant, and the sign is reversed for women. Nonetheless, the results for men are indicative of a particular pattern of results that occurs in a number of the analyses reported below—with stronger age discrimination protections associated with better labor market outcomes for older workers prior to the Great Recession, but a relative worsening of outcomes during and after the Great Recession.

Employment-to-Population Ratios

We next turn to similar analyses for employment-to-population ratios. Figure 6 shows the time-series of the difference-in-differences estimates. Looking first at the graphs for a lower firm-size minimum, in the top row, in the period prior to the Great Recession, the relative employment-to-population ratios of older versus younger workers were higher in states with a lower firm-size minimum, consistent with a lower firm-size minimum improving labor market outcomes for older workers; for women, however, this is less pronounced. For men, across the entire time span there does not appear to be much of a change in relative employment-to-population ratios during or after the Great Recession, whereas for women the relative advantage of older workers eroded during these periods. Looking at larger damages, in the bottom row, the results are similar for older women, with persistently higher employment-to-population ratios in states with larger damages, although this does diminish during the Great Recession. For men, there is no clear difference before the recession, and if anything there is a worsening in the relative employment-to-population ratios of older men in the period immediately subsequent to the Great Recession.

[Figure 6 here]

The regression estimates are reported in Table 4. In this table (and the next) we conserve space by reporting only the estimates for the two DDD parameters and the interaction of *OLD* ×

LAW, which are the main effects of interest. The evidence in Table 4 does not provide much statistically significant evidence of differences in the effects of the Great Recession on employment-to-population ratios associated with stronger age discrimination protections (the estimated coefficients of *GR* × *OLD* × *LAW* and *AfterGR* × *OLD* × *LAW*). The one exception is in column 4, where the estimate implies that in states with lower firm-size minimums, the employment-to-population ratio for women fell by relatively more (a sizable 1.98 percentage points) in the period after the Great Recession. In addition, the point estimates for the corresponding specification for women for larger damages (column 8), for both during and after the Great Recession, are sizable and in the same negative direction, indicating employment-to-population ratios of older women that were lower in relative terms by 1.22 percentage points during the Great Recession, and 0.73 percentage points afterwards. In contrast, the estimated coefficients of *OLD* × *LAW* are almost always positive, although also not statistically significant. Nonetheless, like some of the preceding results for men for unemployment rates, the estimates are in the direction of age discrimination protections improving outcomes for older workers in the prior baseline period, but leading to worse labor market outcomes during or after the Great Recession.

[Table 4 here]

Unemployment Durations

In the top row of Figure 7, there is no clear evidence one way or the other that lower firm-size minimums for state age discrimination laws were associated with differential changes in median unemployment durations. For men there is no change apparent during the Great Recession, and in the period after the Great Recession the direction of the difference varies. For women, there is more of an indication that during the Great Recession a lower firm-size minimum was associated with smaller relative increases in median durations for women, but for the period after the Great

Recession the graph shows perhaps the opposite.

[Figure 7 here]

In the bottom row, the pre-Great Recession period exhibits significantly shorter unemployment durations for older men in states with larger damages, consistent with stronger laws helping these older workers in the pre-recession period. For women, however, there is no evidence of such an effect in this period. Turning to the period of the Great Recession and afterwards, for men the shorter durations of older workers evaporate, whereas for older women things seem to move in the opposite direction, with the data pointing to a decrease in durations during the Great Recession.

Table 5 presents the corresponding regression evidence. In columns 1 to 4, there is no statistically significant evidence that a lower firm-size minimum was associated with differential changes in unemployment durations of older men or older women; the estimated coefficients of *GR* × *OLD* × *LAW* and *AfterGR* × *OLD* × *LAW* are never statistically significant. As in the figures, the sign pattern is not consistent, with a lower firm-size minimum associated with longer spells for older men relative to younger men during the Great Recession, and shorter spells afterwards, whereas for women the signs are reversed.

[Table 5 here]

For larger damages, as reported in columns 5 to 8, the evidence is stronger. For men, the estimates suggest that larger damages resulted in longer durations of unemployment for older men relative to younger men by about five to 5.5 weeks both during and after the Great Recession. The estimates are statistically significant at the 1-, 5-, or 10-percent level depending on the specification and period. For women the signs are reversed, indicating that larger damages were associated with smaller increases in unemployment durations—by about three to four weeks—during and after the Great Recession; only the estimated coefficient for *GR* × *OLD* × *LAW* in the

less-saturated model is statistically significant. Note also that larger damages under state law were associated with shorter unemployment durations for men in the period prior to the Great Recession (*OLD* × *LAW*); the estimates are sizable (about 4.5 weeks) and statistically significant.

Hiring and Separations

Turning to hiring in the QWI data, the top row of Figure 8, for lower firm-size minimums, does not reveal much evidence of a change in the relative hiring rate of older workers during or after the Great Recession, although perhaps some decline for older men after the Great Recession. For larger damages, in the bottom row, there is a similar slight decline for older men, but a more-pronounced change for women, with the hiring rate for older women relative to younger women dropping during the Great Recession and afterwards, and remaining low.

[Figure 8 here]

We next turn to the regression results, in Table 6. For the hiring regressions (and the regressions for separations that follow), some of the other coefficient estimates we did not report in Tables 4 and 5 are of interest, so we report the full set of estimates. Note first the sharp drops in hiring during and after the Great Recession (*GR* and *AfterGR*), as well as the much lower hiring rates for older workers (*OLD*). Turning to the estimates of the DDD parameters, only for women and larger damages (column 8) is there statistically significant evidence that stronger state age discrimination protections are associated with differential changes in hiring rates, with the estimate indicating that the hiring rate declined by 1.07 percentage points in relative terms. Note also that in the results for both age discrimination protections, and for men as well as women, the *OLD* × *LAW* coefficient estimates are positive and (almost always) statistically significant, indicating that stronger age discrimination protections were associated with higher hiring rates of older relative to younger workers in the period prior to the Great Recession, by between 1.6 and 2.3 percentage points for men, and between 1.1 and 1.5 percentage points for women.

[Table 6 here]

As noted earlier, the QWI data on separations are harder to interpret, because they reflect both voluntary and involuntary separations. We report the results for separations in Figure 9 and Table 7. The graphs in Figure 9 look very similar to those for hiring in Figure 8, with perhaps modest declines in separation rates for older men after the Great Recession in states with stronger age discrimination protections, but the most-pronounced difference for older women in states with larger damages.

[Figure 9 here]

[Table 7 here]

In the regression estimates in Table 7, we find that the estimated coefficients of *OLD* × *LAW* are always positive and almost always significant, ranging from a 1.1 to 2.4 percentage points higher separation rate. If this reflects greater ease of getting hired for older workers, as suggested by the estimates in Table 6, then these positive estimates likely reflect a higher level of voluntary separations among older workers where age discrimination laws are stronger, presumably because it is easier for them to find new jobs. The differential effects of stronger age discrimination protections during and after the Great Recession are generally small and insignificant. The one exception is in column 8, where we find that the separation rate for older women in the period after the Great Recession rose by significantly less in states with larger damages (by 1.06 percentage points). This estimate is almost exactly the same as the corresponding estimate for hires, and so might be interpreted as also reflecting changes in voluntary quits—lower, in this case, because hiring is also lower.

The close parallels between the estimates (and graphs) for separations and hiring suggest that our estimates of the effects of age discrimination protections on separations—and how they vary across time and by age group—largely reflect voluntary separations. Thus, we are apparently

not getting much independent information from the separations data. In particular, we are not seeing effects on involuntary separations; the clearest evidence of this is the estimates in Table 7 indicating that separations fell during and after the Great Recession (*GR* and *AfterGR*), which we expect for voluntary but not involuntary separations. That does not mean that involuntary separations (terminations) did not change in the expected ways, but rather simply that the changes in voluntary separations dominate the data.

Statistical Power

We have found some evidence that state age discrimination laws were associated with statistically significant, differential effects of the Great Recession on older workers. This tells us that some of the changes that actually occurred were large enough relative to the precision of our estimates to be detected. But it is fair to ask, with respect to the many cases where we do not find statistically significant evidence, whether this is likely because the effects if they occurred were small, or because our empirical analysis does not detect as significant estimates that are substantively large.

We do not have any other evidence of expected effects with which to compare our estimated standard errors. We do have some much earlier evidence (from 1960s data) on the effects of age discrimination laws on employment rates of older men, which point to increases of about 4 percentage points for men aged 60 or 65 and older (Adams, 2004). So it might be reasonable to suppose that, as an upper bound, a stronger age discrimination law could boost employment by 4 percentage points. There is no evidence on how the extent of age discrimination varies over the business cycle. However, if we think of this question in the sense of a recession changing the effectiveness of age discrimination laws, then presumably this effect has to be smaller than 4 percentage points. And we have to recognize that this 4 percentage point figure does not come from the types of variation in age discrimination laws we study in the current period, but rather comes from the advent of these laws. Hence, we should probably start with a

maximum effect below 4 percentage points. Suppose we cut it in half, to 2 percentage points. Then the question becomes whether we can detect as significant changes of 2 percentage points (or less). For employment rates, the standard errors on our DDD estimates in the more-saturated models in Table 4 range from about 1.0 to 1.6 for men, and 0.7 to 1.0 for women. So on this score the answer is that we might be able to detect reasonably sized effects of the business cycle on age discrimination for older women, although perhaps less so for older men. Nonetheless, the point estimates for older men are small, so the issue is not large standard errors.

There is no existing research on which to draw for thinking about such calculations in the context of the other outcomes we study. Nonetheless, we can still think about the standard errors for these estimates for other outcomes and whether they would allow us to detect as statistically significant what seem to be meaningful differences. For unemployment rates, the DDD standard errors for the more-saturated models are around 0.4 to 0.8, meaning that we can detect as significant differences of about 0.8 to 1.6 or higher. For unemployment durations, the standard errors hover around 2.5. Our sense is that given the dramatic movements in unemployment rates and durations during and after the Great Recession, this implies that we could detect as significant relative changes across states that are meaningful, and conversely that the magnitudes of changes that would not be detected as significant (like changes of two weeks duration) would not be very substantive anyway. The same goes for hiring rates, which are in the 11 to 24 percent range pre-Great Recession, but drop by as much as 9 percentage points. The standard errors for hiring rates in the more-saturated models are about 0.4 to 0.8, suggesting that we would detect meaningful differences in changes in hiring rates.

Robustness Analyses

We next explore the robustness of the key results to variations in the specification, sample, or estimation. We report evidence for the five cases where we found significant differences in the

relative effect of the Great Recession on older workers between states with and without stronger age discrimination protections. These five cases are the effects of larger damages on unemployment rates of men; the effects of lower firm-size minimums on employment-to-population ratios of women; the effects of larger damages on unemployment durations of both men and women; and the effects of larger damages on hiring rates of women.[23]

The results of many of these analyses are reported in Table 8. In each case, the change indicated in the panel heading is relative to the baseline specification from the even-numbered columns of Tables 3 to 6, for which the estimates are repeated in panel a. First, in panel b we report estimates of the specification that adds state-by-age interactions, allowing more flexibly for differences in the age profile of each outcome by state. In this case, we saturate the model as fully as possible with regard to all the two-way interactions in equation (1), so the coefficient of $OLD \times LAW$ is no longer identified; but the two key triple interactions are. Comparing panels a and b, the point estimates are qualitatively similar, although two of the estimates become smaller and statistically insignificant—for the effect of lower firm-size minimums on the employment-to-population ratio of women in the period after the Great Recession, and for the effect of larger damages on the hiring rate of women in the same period. The estimated effects on unemployment rates and durations remain very similar, however.

[Table 8 here]

Second, we noted earlier that, in a small number of cases, there were no unemployed workers in the cell, so we set median duration to zero. However, this may not accurately reflect median durations in the corresponding states and months. Hence, in panel c we instead drop these cells from the analysis and re-estimate the models for unemployment duration. The estimates are scarcely affected, and indeed the estimate for men for the period after the Great Recession becomes more strongly significant.

In panel d we drop the weighting by state population. The signs of the estimates are unaffected, but there are some changes in the magnitudes and most of the estimated effects during and after the Great Recession become insignificant; in quite a few cases standard errors are larger. However, as argued earlier the weighted estimates are preferable, and more representative of what happened to workers during and after the Great Recession.

Finally, in panels e to g we use different age groups to define older and younger. In the first panel, we restrict the older group to 55 to 64 rather than 55+, and in the last two panels we report results for the alternative definitions of the older group, but a more narrowly restricted younger group (25 to 34 instead of 25 to 44). We view the results as qualitatively very similar. More generally, this is true across all of the robustness analyses we have explored, although as Table 8 shows, some analyses—especially not weighting—lead to fewer significant results.

We did two other analyses for which results are available upon request. First, the results are robust to using different lag lengths for the UI benefit and age composition controls (through one year or three years) as well as dropping these controls in the more-saturated models. Second, using a smaller firm-size minimum (fewer than six instead of fewer than 10 workers), we found qualitatively similar results, although the significant effect for the employment-to-population ratio for women becomes insignificant. This confirms the findings throughout the paper that the results for larger damages are stronger and more robust than the results for firm-size minimums.

Finally, in addition to the specifications discussed thus far, we estimated specifications using a continuous measure to capture the severity of the Great Recession in each state. This measure varies only across states. But we estimate a version of the saturated model in which the dummy variables *GR* and *AfterGR* are replaced with interactions of these variables with this continuous shock variable, to allow for differential impacts of the Great Recession across states in both the recessionary period itself and afterwards. The approach and results are described in an

appendix available from the authors. The results were very similar, in part because using an exogenous measure of the severity of the Great Recession across states—generated from national industry employment trends coupled with the workforce composition of each state by industry, age, and gender—does not generate much variation in the strength of the Great Recession across states. Using a measure like changes in unemployment rates would exhibit more cross-state variation, but would potentially be endogenous with respect to the effects of state age discrimination laws, if these laws affect the evolution of unemployment for a given economic shock.

Falsification Test

Table 9 reports results for a falsification test. The principal concern that this test addresses is that underlying trends could drive the results. Even though our estimated model is quite saturated, we of course cannot control for arbitrary changes in age profiles over time that vary by state (age-by-state-by-time interactions). Thus, changes in age profiles of our outcomes over time that are correlated with differences in state age discrimination laws could spuriously generate our results.

[Table 9 here]

We test for the presence of trends that could generate our evidence by choosing a recent sustained period without a recession and asking if changes over this period generate similar findings. We consider the same outcomes and state age discrimination protections as in Table 8, using the period 2003 to 2007, and treat 2005 to 2007 as the *recession*. This was actually a period of continuous expansion, so if we were to find similar estimates to what we actually find comparing the Great Recession period to the prior period, we would conclude that the results were instead driven by trends in labor market outcomes by age that happen to coincide with which states have stronger age discrimination protections. Note that, in this case, there is only one recession period, post-2004, and hence only one triple interaction, which we denote *Post-2004* ×

OLD × *LAW*. Also, we can only do this analysis for the CPS data because the QWI data for many states do not go back far enough.

As the estimates in panel b of Table 9 show, there is no evidence of changes over time in labor market outcomes of older relative to younger workers associated with stronger age discrimination protections that mimics what we find comparing the Great Recession period (or afterwards) to the earlier baseline period. We do find a positive and significant coefficient on *Post-2004* × *OLD* × *LAW* for the employment-to-population ratio for women, when we look at firm-size minimums. But this is the opposite sign from the original results, and hence does not point to a trend toward lower employment of older women in states with this stronger age discrimination protection. The one estimate that parallels the original results is the estimated coefficient of *OLD* × *LAW* in column 3. However, this captures the same shorter durations of unemployment for older men in non-recessionary periods that we found before, rather than changes over time. Thus, the estimates in Table 9 appear to rule out spurious trends driving the results.

Summary of Results

Table 10 summarizes the results across both our main specifications and our robustness analyses. We report results for the two key triple-difference coefficients—*GR* × *OLD* × *LAW* and *AfterGR* × *OLD* × *LAW*—and for the coefficients on *OLD* × *LAW*. To provide a summary of the evidence across all of the analyses we have discussed, the table reports the mean and range of estimates, as well as the number that are (1) significant (at the 10-percent level or less) and positive, (2) significant and negative, and (3) insignificant.

[Table 10 here]

Looking first at the triple-difference results, in the second and third rows, for men, we simply find no evidence that stronger age discrimination protections helped older workers weather the Great Recession, relative to younger workers. When there is evidence that stronger state age

discrimination protections influenced the effects of the Great Recession, they appear to have made things relatively worse for older workers. This is the case for unemployment rates and unemployment durations, and for larger damages under state law. These estimates suggest that state age discrimination laws allowing for larger damages were associated with higher unemployment durations of older men by on average about 5.5 weeks, and unemployment rates that were higher by about 1 percentage point (in the post-Great Recession period). The estimates indicating that age discrimination protections led to a worsening of unemployment-related outcomes for older workers, relative to younger workers, are shaded in the table.

For women, the evidence is more mixed. On the one hand, there is some evidence that stronger age discrimination protections in the form of larger damages were associated with relatively smaller increases in unemployment durations of older women during the Great Recession—by about 4.7 weeks; the one cell in the table reflecting this type of positive effect is indicated by a box. On the other hand, we also find that in the period after the Great Recession, in states with lower firm-size minimums, older women had larger declines in the employment-to-population ratio (by about 1.5 percentage points), and in states with larger damages they had bigger declines in their hiring rate (by about 0.3 percentage point during the Great Recession, and 0.7 percentage point afterwards). Like for men, the estimates indicating adverse effects of stronger age discrimination protections on older women are shaded.

Thus, between the results for men and women, there is very little evidence that stronger state age discrimination protections helped older workers weather the Great Recession. Moreover, there is quite a bit of evidence that the opposite occurred, with older workers bearing more of the brunt of the Great Recession in states with stronger age discrimination protections.

Finally, turning to the $OLD \times LAW$ estimates, there is some evidence that stronger age discrimination protections helped older men and women in the period prior to the Great Recession.

The evidence consistent with stronger protections helping older workers in the period prior to the Great Recession is statistically significant for unemployment durations and larger damages for men—indicating unemployment durations shorter by about 4.4 weeks—and for hiring rates for women—with hiring rates higher by about 1 percentage point. (Indeed Table 6 reports a higher hiring rate for older men and women in the pre-recession period in nearly all specifications.)

Putting the evidence together, in some cases we find that stronger age discrimination protections helped older workers in the pre-Great Recession period, but led to a relative worsening of outcomes for older workers during and after the Great Recession. The estimates where we find at least some statistically significant evidence of this pattern—i.e., significant beneficial effects pre-Great Recession, and adverse effects during or after the Great Recession—are boldfaced in Table 10. This evidence arises for unemployment durations for men, and for hiring rates for women—in both cases for larger damages. And the point estimates are rather sizable and consistent with this pattern for unemployment durations and larger damages for women as well.[24] Moreover, the pre- and post-Great Recession magnitudes are often similar but opposite signed, suggesting that the advantages these protections offered prior to the Great Recession may have been largely eroded during and after the recession.

CONCLUSIONS AND DISCUSSION

We generally do not find evidence that stronger state age discrimination protections helped older workers weather the Great Recession. Rather, for some outcomes older workers appear to have experienced relatively worse outcomes from the Great Recession in states with stronger age discrimination protections. The effects are often sizable; for example, state age discrimination laws allowing larger damages were associated with longer unemployment durations of older men by about 5.5 weeks during and after the Great Recession. In contrast, there is some evidence that—in particular for unemployment durations and hiring rates—stronger age discrimination

protections helped older men and women in the period prior to the Great Recession. The combined estimates often suggest that the advantages these protections offered prior to the Great Recession were largely eroded during and after the recession. Our evidence is stronger for state age discrimination laws that allow larger damages than for laws with smaller firm-size minimums. This is perhaps not surprising given that larger damages apply to all workers potentially affected by an age discrimination claim, and directly affect the financial incentives to pursue a claim.

This evidence is consistent with some of the past evidence and theoretical conjectures we have discussed. In particular, the evidence that older workers fare better in normal times in states with stronger age discrimination protections is consistent with past work finding that the adoption of state and federal age discrimination laws increased employment of older men (Neumark & Stock, 1999; Adams, 2004). But we also suggested that stronger age discrimination protections could become less effective or even increase discrimination against older workers during severe downturns. This could occur because in states with stronger laws there is more pent-up demand to shift to a younger workforce, and such shifts can be done during recessions when age discrimination is harder to detect. Alternatively, stronger protections against discrimination could increase termination costs, and these could weigh more heavily on hiring decisions during and after a severe recession when product and hence labor demand is uncertain.

It would be useful to try to distinguish among these alternative explanations. Our evidence pointing to an increase in age discrimination during and after the Great Recession in states with stronger age discrimination laws appears in the form of both lower hiring and lower separations of older workers (for women in particular, although the point estimates for men are in the same direction). The fact that we do not see evidence of higher separation rates for older workers indicates that the main effects arise through changes in voluntary rather than involuntary terminations, and hence reflect hiring effects. This may make it more likely that the evidence of

apparently greater discrimination against older workers stems from reduced hiring owing to higher termination costs. We cannot, however, rule out the possibility that during and after the Great Recession firms previously constrained by age discrimination laws did more restructuring towards a younger workforce through turnover and hiring rather than through involuntary terminations, perhaps because even in a period of considerable labor market turbulence, discrimination in terminations remains relatively easy to detect. In contrast, it likely would have been difficult to draw an inference of age discrimination from failure to get hired in the period during or after the Great Recession.

These conjectures about the decreased effectiveness of age discrimination protections in the aftermath of a severe recession have potentially different implications for longer run consequences. If indeed all that occurred is temporary increases in age discrimination, then we might anticipate that as the economy recovers the stronger state age discrimination protections—in the states that have them—would again become more effective at improving labor market outcomes for older workers.[25] On the other hand, there could be longer term adverse effects of even temporary changes in behavior. If it did indeed become easier to discriminate against older workers during the Great Recession and its aftermath, or employers were more likely to engage in such discrimination, then the extended periods of unemployment, especially among workers near retirement ages, might have hastened transitions out of the labor market and toward retirement, permanently lowering employment among older workers. In that sense, the longer run implications can be more severe than for temporary increases in discrimination after recessions against other, generally younger groups, for whom permanent labor force exit is much less likely. If, in fact, increases in age discrimination during and after sharp economic downturns do spur labor force exit, then given the imperative to extend work lives of older individuals, it may be useful to think about whether it is possible to modify age discrimination protections so that they

maintain their effectiveness in times of economic turbulence. It is not obvious what kinds of changes might meet this objective, since inferring discriminatory patterns in employer behavior will inevitably be difficult when labor markets are more volatile. But making it more difficult to discriminate in hiring, in general, could help.

David Neumark is Chancellor's Professor of Economics, University of California, Irvine; Research Associate at the NBER; and Research Fellow at IZA; Department of Economics, 3151 Social Science Plaza, University of California, Irvine, Irvine, CA 92697, email: dneumark@uci.edu. Patrick Button is a Ph.D. candidate in Economics, Department of Economics, 3151 Social Science Plaza, University of California, Irvine, Irvine, CA 92697, email: pbutton@uci.edu.

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APPENDIX A: AGE COMPOSITION CONTROL

Let subscripts s index state, a age group, g gender, and k industry. Denote by SE_{asgk03} total employment for a, s, g, and k, in the baseline year 2003. Denote by AE_{kt} national employment in each period t in industry k, and denote by AE_{k03} national employment in industry k in 2003. Then we can predict the variation in employment by age, state, and gender based solely on national employment changes by industry subsequent to 2003, by applying the national changes to the baseline composition, as in

$$PE_{agst} = \sum_{k} SE_{agsk03} \times \left(\frac{AE_{kt}}{AE_{k03}}\right). \tag{A.1}$$

We use non-seasonally adjusted monthly employment at the national level, by two-digit NAICS code, to measure AE_{kt} and AE_{k03}, both of which come from the Quarterly Census of Employment and Wages (QCEW). We use the QWI to measure SE_{agsk03}, since the QWI allows for employment estimates by age, state, and gender.[26] For each k, the ratio in equation (A.1) captures the growth in industry k over time. This is multiplied by the mean employment of age group a and gender g in state s and industry k in 2003.[27] This weights the national industry employment growth by the age and gender composition of employment in that industry in the baseline year. Our resulting age composition control is the difference in predicted employment growth rates between the two age groups, or

$$\begin{aligned} CC_{gst} = \{(&\log\left(PE_{old,g,s,t}\right) - \log\left(PE_{old,g,s,t-1}\right) \\ &- [\log(PE_{young,g,s,t}) - \log(PE_{young,g,s,t-1})]\} \times 100. \end{aligned} \tag{A.2}$$

CC_{gst} captures the difference in predicted growth rates, which can be interpreted as demand influences, between older and younger employment within the state (for each gender separately). If both groups are hit with the same predicted shock, then CC_{gst} equals zero. In contrast, for example, CC_{gst} will be positive if the shock that hit the state in period t was more favorable to employment of older workers. This variable should be exogenous to state economic developments

that could in turn be influenced by age discrimination laws, since it is based on national employment growth with fixed weights from the base year.

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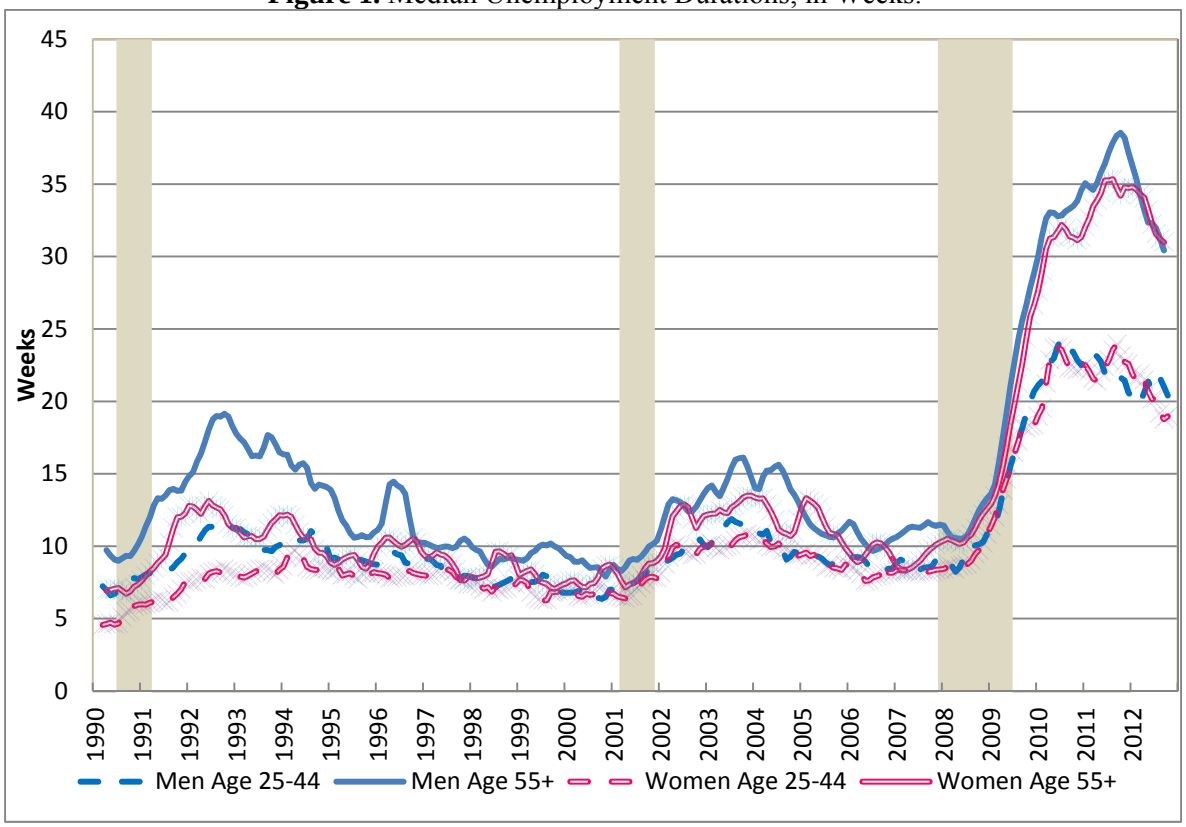
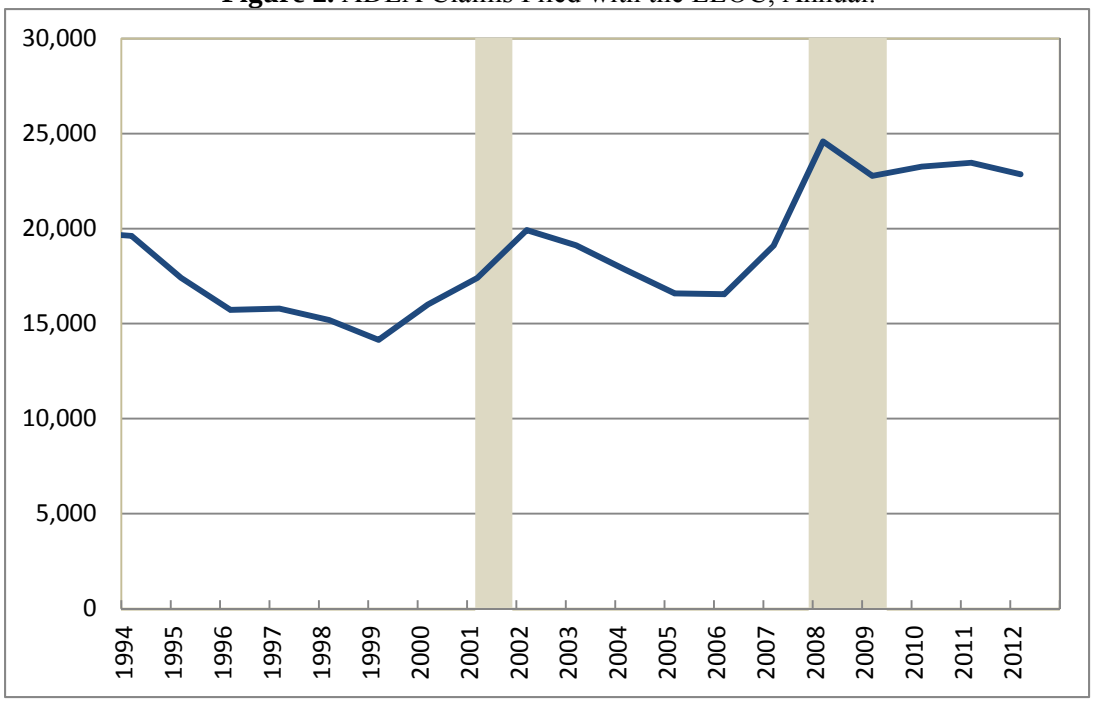
Figure 1. Median Unemployment Durations, in Weeks.



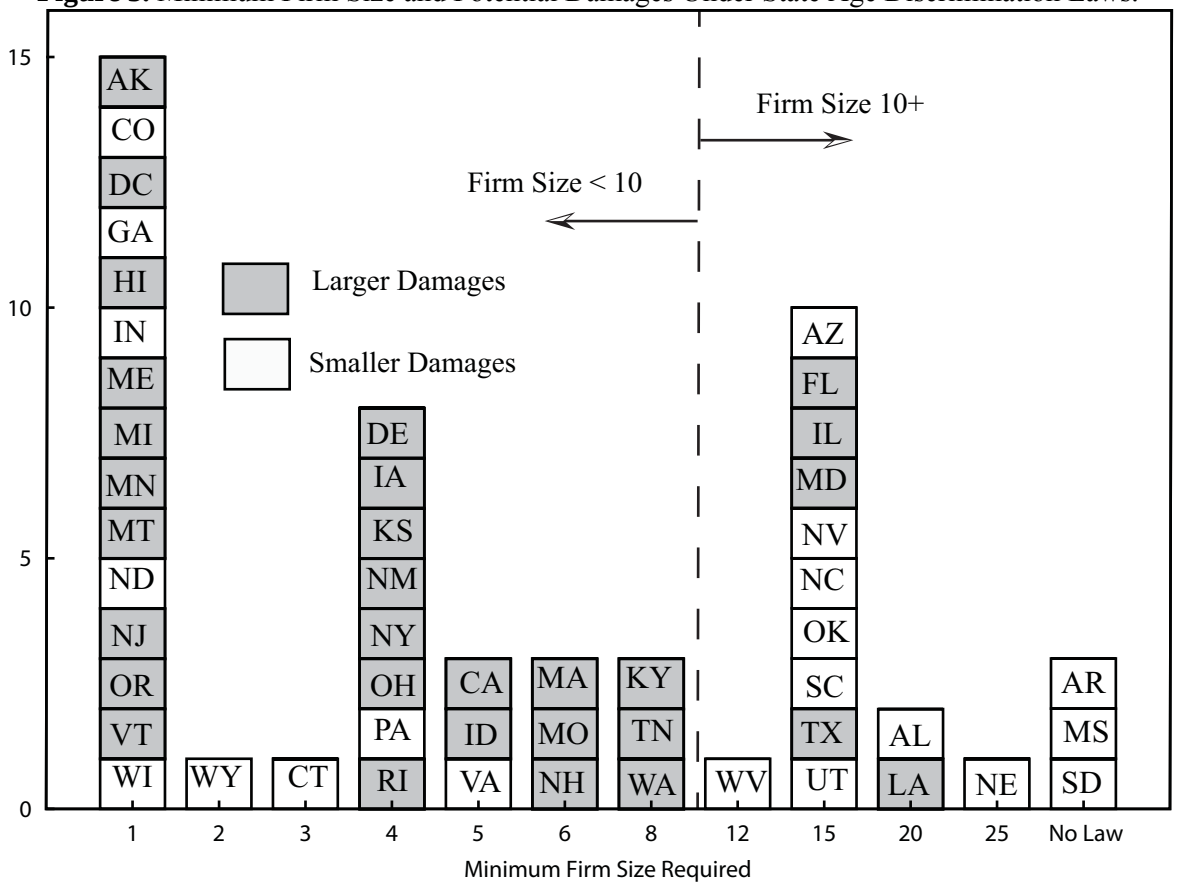
Notes: Shaded areas are recessions based on NBER dates. Each series was generated using the Current Population Survey. State estimates were calculated and weighted by state population to generate nationally representative estimates. Each series was seasonally adjusted and smoothed using X-12-ARIMA.

Figure 2. ADEA Claims Filed with the EEOC, Annual.



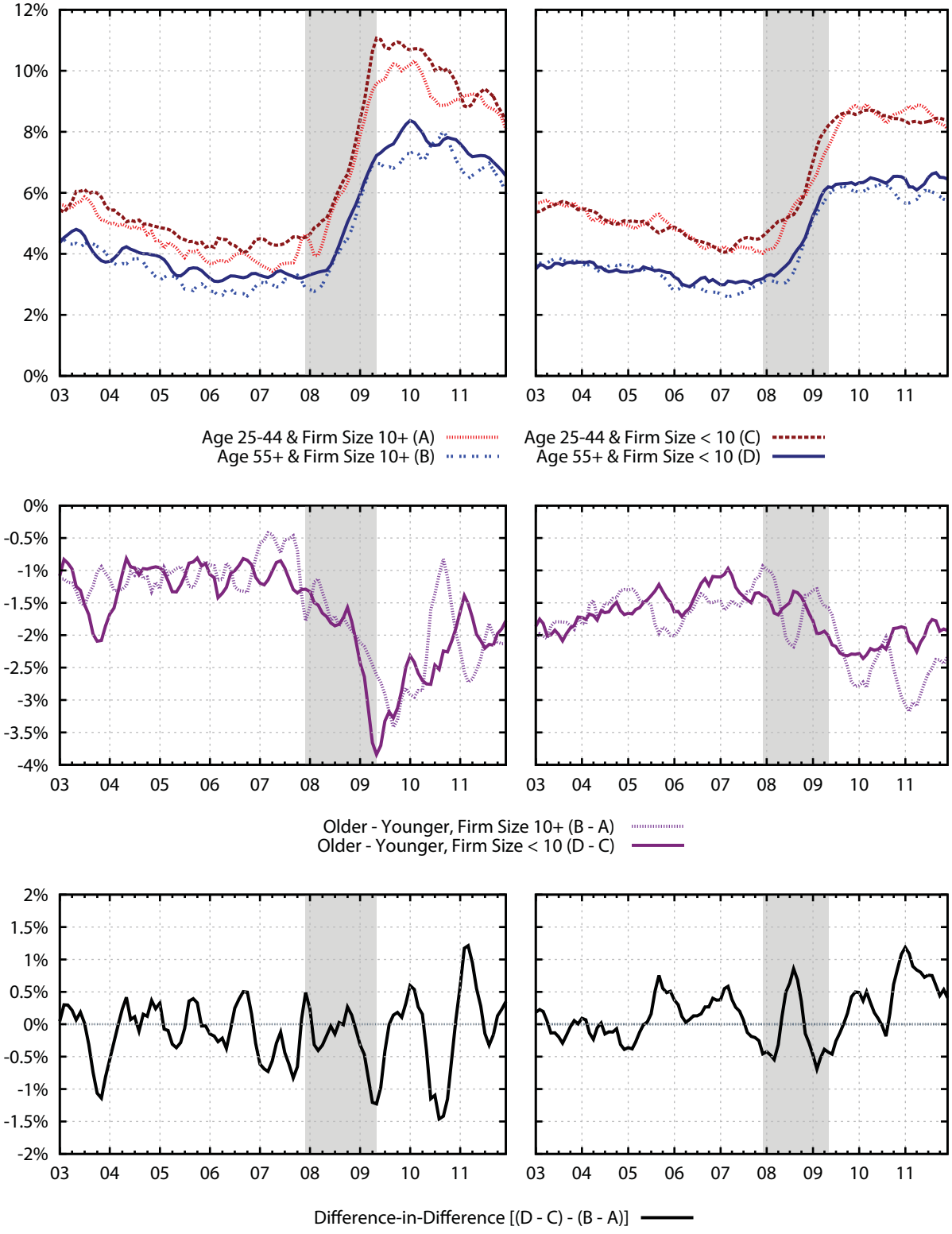
Notes: Shaded areas are recessions based on NBER dates. Data are annual and based on the government fiscal year. They are assigned to March of each year, which is midway through the fiscal year. *Source:* http://eeoc.gov/eeoc/statistics/enforcement/adea.cfm (viewed November 23, 2013)

Figure 3. Minimum Firm Size and Potential Damages Under State Age Discrimination Laws.



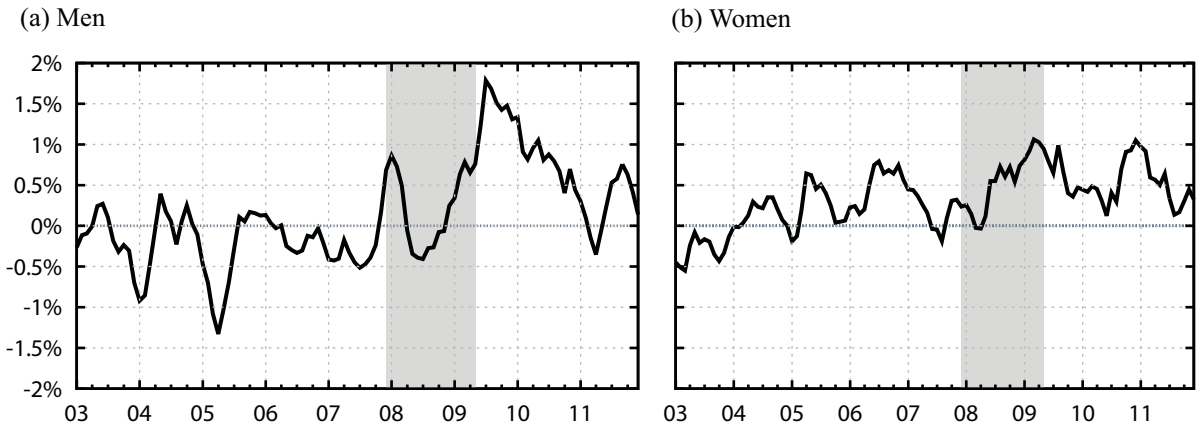
Notes: See Neumark and Song (2013) for additional details on age discrimination laws by state. Three states (Arkansas, Mississippi, and South Dakota) do not have state age discrimination laws and hence are put in the higher firm-size minimum group and classified as not having larger damages.

Figure 4. Unemployment Rates by Age and Firm-Size Minimum, Men (Left) and Women (Right).



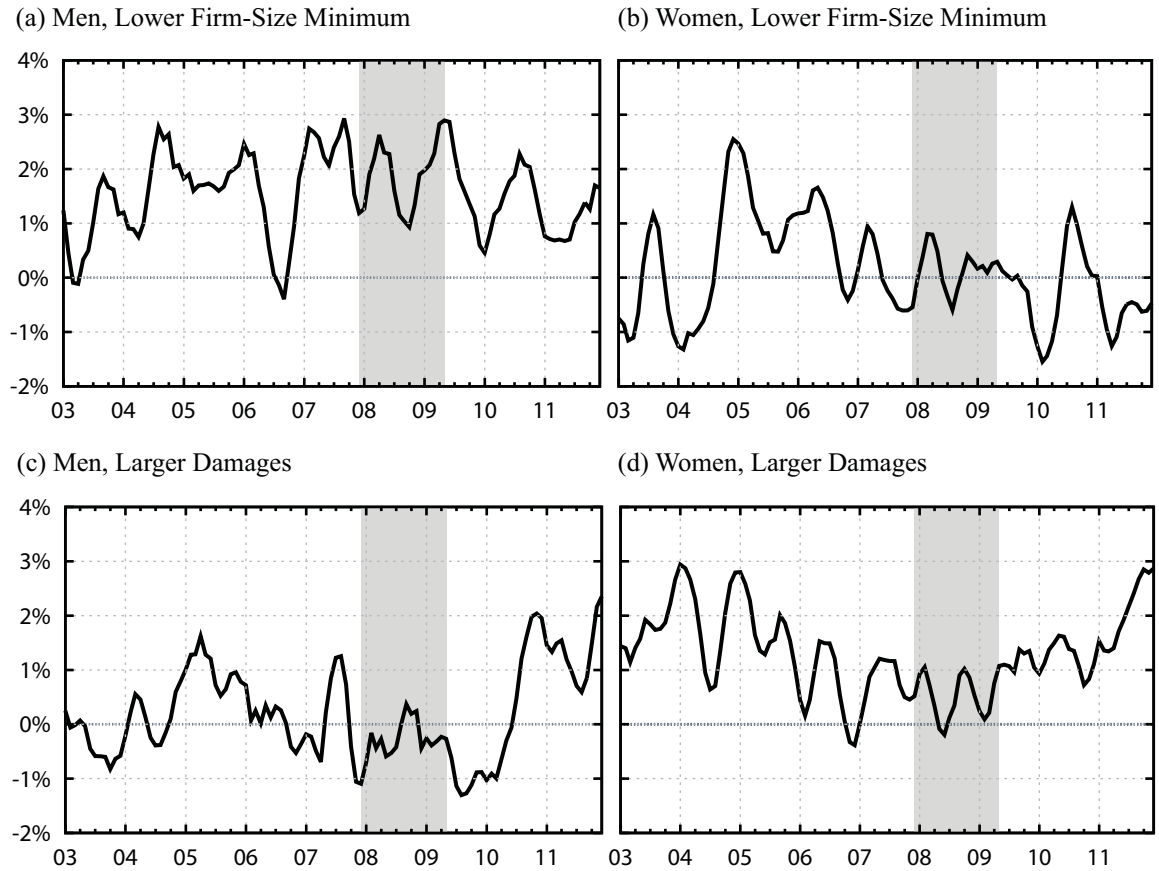
Notes: See the notes to Figure 1 above. States are divided into two groups based on the minimum firm size required for age discrimination laws to apply. See Figure 3 for additional details on age discrimination laws by state.

Figure 5. Differences-in-Differences in Unemployment Rates, by Larger Damages.



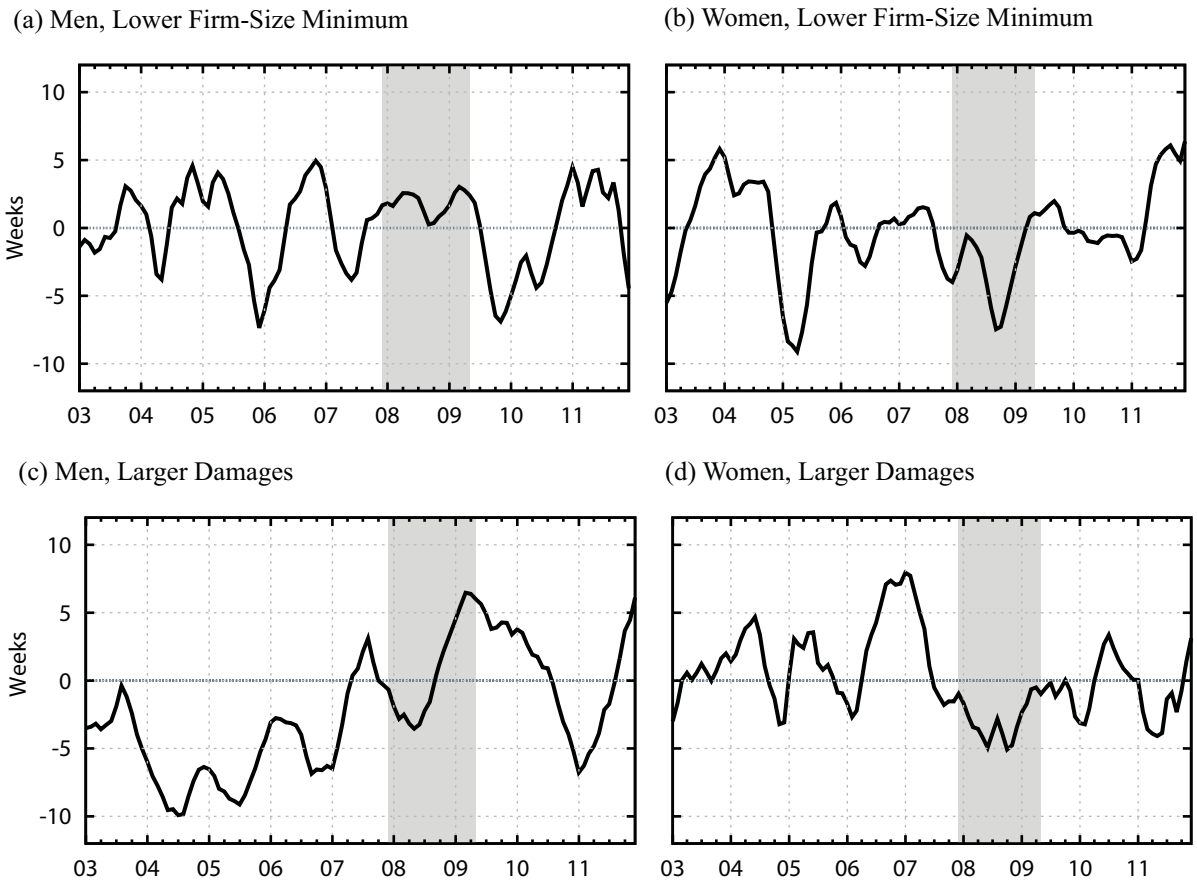
Notes: See the notes to Figure 4. The only difference compared to Figure 4 is that larger damages than available under federal law are used instead of firm-size minimums, and only the bottom graphs are presented. The differences-in-differences plotted are [older (55+) – younger (25 to 44) in the states with larger damages] – [older – younger in the states without larger damages].

Figure 6. Differences-in-Differences in Employment-to-Population.



Notes: See the notes to Figures 4 and 5. The differences-in-differences plotted are [older (55+) – younger (25 to 44) in the states with the stronger age discrimination protection indicated in the panel heading] – [older – younger in the states without that stronger age discrimination protection].

Figure 7. Difference-in-Differences in Median Durations.



Notes: See the notes to Figures 4 and 5. The differences-in-differences plotted are [older (55+) – younger (25 to 44) in the states with the stronger age discrimination protection indicated in the panel heading] – [older – younger in the states without that stronger age discrimination protection].

Figure 8. Differences-in-Differences in Hiring Rates.

Notes: See the notes to Figures 4 and 5. States are divided into four groups based on the age discrimination protection (lower firm-size minimum or larger damages) and age (25 to 44 and 55+). Each underlying series is generated by summing QWI estimates of the number of hires per quarter, by age group, in states of each law group, and dividing the entire series by the average employment across all those states in 2005. These estimates are implicitly weighted by state population, since larger states contribute more weight to the calculation. For most of the hiring series, X-12-ARIMA smoothes the data using the two nearest quarters, which eliminates 2004:Q2. The differences-in-differences plotted are [older (55+) – younger (25-44) in the states with the stronger age discrimination protection indicated in the panel heading] – [older – younger in the states without that stronger age discrimination protection].

Figure 9. Difference-in-Differences in Separation Rates.

Notes: See the notes to Figures 4, 5, and 11. Separation rates are calculated in the same way as hiring rates. The differences-in-differences plotted are [older (55+) – younger (25 to 44) in the states with the stronger age discrimination protection indicated in the panel heading] – [older – younger in the states without that stronger age discrimination protection].

Table 1. Summary statistics for CPS data, 2003 to 2011.

	Younger (25–44) Men		Younger (25–44) Women	
	mean	St. dev.	mean	St. dev.
Unemployment rate	6.5	3.1	6.1	2.5
Employment-to-population ratio	85.2	4.2	70.5	4.6
Median unemployment duration	14.2	9.2	13.8	8.8
	Older (55+) Men		Older (55+) Women	
	Mean	St. dev.	Mean	St. dev.
Unemployment rate	4.9	2.8	4.3	2.4
Employment-to-population ratio	43.3	4.6	31.8	3.8
Median unemployment duration	22.0	18.9	20.5	19.4
	Controls		Controls	
	Mean	St. dev.	Mean	St. dev.
Compositional control	0.007	0.348	-0.002	0.395
Extra UI weeks available	21.1	27.4	21.1	27.4

Notes: These statistics were generated for each state and month from 2003 to 2011 using the Current Population Survey (CPS) monthly micro-data using the CPS sample weights. There are 5,508 observations for each age group and gender. These estimates are weighted using state population estimates generated from the CPS. The population estimates are generated by summing the provided population weights for all observations for each state, yielding estimates that are based on census population estimates and projections (U.S. Bureau of Labor Statistics, 2006, Section 10-8). For median durations, for some small cells there are no unemployed workers; in these cases the missing observations are coded as zeroes. Data are not seasonally adjusted.

Table 2. Summary statistics for QWI data, hires and separations relative to 2005 employment (%), 2004:Q2 to 2011:Q4.

	Younger (25-44) Men		Younger (25-44) Women	
	Mean	St. Dev.	Mean	St. Dev.
Hires	18.5	4.2	17.3	3.9
Separations	18.1	4.0	16.9	3.6
	Older (55+) Men		Older (55+) Women	
	Mean	St. Dev.	Mean	St. Dev.
Hires	13.6	3.0	12.1	2.9
Separations	15.2	3.3	13.7	2.9

Notes: These statistics were generated for each state (excluding Washington, DC), quarter, and age group from 2004:Q2 to 2011:Q4 using the Quarterly Workforce Indicators (QWI). There are 1,519 observations for each age group and gender. These estimates are weighted by state population. See notes to Table 1. Data are not seasonally adjusted.

Table 3. Estimated impacts of lower firm-size minimums and larger damages on unemployment rates.

	Men		Women		Men		Women	
	Firm-size minimums				Larger damages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLD × LAW	**-0.14**	**-0.15**	**0.08**	**0.09**	**-0.26**	**-0.26**	**0.14**	**0.15**
	(0.23)	**(0.32)**	**(0.30)**	**(0.43)**	**(0.19)**	**(0.27)**	**(0.24)**	**(0.38)**
GR × OLD × LAW	**-0.14**	**-0.08**	**-0.18**	**-0.23**	**0.56**	**0.54**	**0.38**	**0.36**
	(0.41)	**(0.51)**	**(0.44)**	**(0.58)**	**(0.40)**	**(0.54)**	**(0.46)**	**(0.64)**
AfterGR × OLD × LAW	**0.14**	**0.18**	**0.35**	**0.47**	**0.96*****	**1.03*****	**0.42**	**0.51**
	(0.41)	**(0.55)**	**(0.48)**	**(0.71)**	**(0.26)**	**(0.38)**	**(0.54)**	**(0.79)**
OLD	-1.03***	…	-1.62***	…	-0.94***	…	-1.67***	…
	(0.20)		(0.28)		(0.14)		(0.19)	
LAW	0.45	…	0.01	…	0.62**	…	0.35	…
	(0.30)		(0.31)		(0.25)		(0.25)	
Great Recession (GR)	1.92***	…	0.64*	…	2.04***	…	0.92***	…
	(0.58)		(0.33)		(0.26)		(0.28)	
AfterGR	4.95***	…	3.71***	…	5.13***	…	3.82***	…
	(0.72)		(0.45)		(0.34)		(0.37)	
GR × OLD	-0.82**	…	0.12	…	-1.30***	…	-0.26	…
	(0.37)		(0.36)		(0.34)		(0.37)	
AfterGR × OLD	-1.28***	…	-0.94**	…	-1.85***	…	-1.00**	…
	(0.35)		(0.40)		(0.19)		(0.49)	
GR × LAW	0.10	…	0.39	…	-0.09	…	-0.04	…
	(0.63)		(0.40)		(0.45)		(0.38)	
AfterGR × LAW	0.08	…	-0.10	…	-0.19	…	-0.26	…
	(0.81)		(0.57)		(0.81)		(0.57)	
Cumulative effect, 2 years:								
UI benefit extensions	…	0.01	…	-0.02	…	0.00	…	-0.01
(weeks) × *OLD*		(0.03)		(0.03)		(0.03)		(0.03)
Age composition control	…	-2.00	…	5.12	…	-2.09	…	-0.79
× *OLD*		(12.43)		(21.17)		(11.97)		(23.39)
Includes full set of state × month and age × month, interactions	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors, clustered at the state level, are in parentheses. *, **, and *** mean statistically significant from zero at the 10-percent, 5-percent, and 1-percent levels, respectively. The sample period is 2003 to 2011. There are 11,016 observations. All estimates are weighted by state population. The unemployment rate and other proportion variables in following tables are on a scale of 0 to 100. For the unemployment insurance and compositional controls, both the contemporaneous variable and 24 months of lags are included in the even-numbered columns. In these columns, the addition of state-by-month fixed effects removes *GR*, *AfterGR*, *LAW*, *GR × LAW*, and *AfterGR × LAW*, and age-by-month fixed effects remove *OLD*, *GR × OLD*, and *AfterGR × OLD*. In the regressions, rather than using seasonally adjusted data, the models include calendar-month dummy variables. To allow for different seasonality by age group and type of state (defined by age discrimination law)—to better match the separate seasonal adjustment we use in the figures—these are also entered interacted with *LAW*, *OLD*, and *LAW × OLD*. (Prior to forming these interactions, the calendar-month dummy variables are demeaned, so that the estimated coefficients of *LAW*, *OLD*, and *LAW × OLD* reflect differentials evaluated at the sample means.)

Table 4. Estimated impacts of lower firm size minimums and larger damages on employment-to-population ratios.

	Men		Women		Men		Women	
	Firm-size minimums				Larger damages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLD × LAW	**1.55**	**1.66**	**0.34**	**-0.08**	**0.18**	**0.26**	**1.39**	**0.77**
	(1.32)	**(1.89)**	**(1.48)**	**(1.67)**	**(1.02)**	**(1.50)**	**(1.14)**	**(1.37)**
GR × OLD × LAW	**0.40**	**0.07**	**-0.16**	**-0.52**	**-0.41**	**-0.44**	**-0.87**	**-1.22**
	(1.09)	**(1.45)**	**(0.55)**	**(0.75)**	**(0.87)**	**(1.18)**	**(0.51)**	**(0.73)**
AfterGR × OLD × LAW	**-0.22**	**-0.25**	**-0.71**	**-1.98****	**0.27**	**0.12**	**0.17**	**-0.73**
	(1.00)	**(1.56)**	**(0.58)**	**(0.81)**	**(0.75)**	**(1.04)**	**(0.65)**	**(1.02)**
Main effects and two-way interactions from Table 3 included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cumulative effect, 2 years:								
UI benefit extensions	…	-0.06	…	0.05	…	0.02	…	0.01
(weeks) × *OLD*		(0.06)		(0.07)		(0.07)		(0.10)
Age composition control	…	17.94	…	208.47***	…	22.19	…	188.18**
× *OLD*		(27.64)		(72.66)		(31.16)		(76.22)
Includes full set of state × month and age × month, interactions	No	Yes	No	Yes	No	Yes	No	Yes

Note: See notes to Table 3.

Table 5. Estimated impacts of lower firm-size minimums and larger damages on median unemployment durations.

	Men		Women		Men		Women	
	Firm-size minimums				Larger damages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLD × LAW	**0.26**	**0.26**	**-0.28**	**-0.21**	**-4.66*****	**-4.46*****	**1.79**	**1.71**
	(1.23)	**(1.67)**	**(1.53)**	**(2.23)**	**(0.71)**	**(1.00)**	**(1.42)**	**(1.98)**
GR × OLD × LAW	**1.50**	**1.37**	**-2.51**	**-2.85**	**5.48*****	**5.57****	**-4.37****	**-4.35**
	(1.76)	**(2.51)**	**(1.79)**	**(2.47)**	**(1.68)**	**(2.29)**	**(2.02)**	**(2.98)**
AfterGR × OLD × LAW	**-0.89**	**-2.33**	**1.31**	**1.57**	**5.37*****	**5.04***	**-3.03**	**-3.13**
	(2.03)	**(2.71)**	**(1.69)**	**(2.39)**	**(1.82)**	**(2.52)**	**(1.98)**	**(2.89)**
Main effects and two-way interactions from Table 3 included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cumulative effect, 2 years:								
UI benefit extensions	…	0.27	…	-0.11	…	0.23	…	-0.08
(weeks) × *OLD*		(0.18)		(0.21)		(0.15)		(0.21)
Age composition control	…	89.60*	…	9.11	…	58.05	…	17.01
× *OLD*		(47.52)		(97.79)		(49.96)		(90.84)
Includes full set of state × month and age × month, interactions	No	Yes	No	Yes	No	Yes	No	Yes

Note: See notes to Table 3.

Table 6. Estimated impacts of lower firm-size minimums and larger damages on hires relative to 2005 employment (%).

	Men		Women		Men		Women	
	Firm-size minimums				Larger damages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLD × LAW	**2.25*****	**2.16****	**1.45*****	**1.30***	**1.63****	**1.62***	**1.37*****	**1.11**
	(0.67)	**(0.96)**	**(0.53)**	**(0.68)**	**(0.64)**	**(0.89)**	**(0.50)**	**(0.68)**
GR × OLD × LAW	**0.18**	**0.28**	**0.18**	**0.03**	**-0.11**	**-0.08**	**-0.21**	**-0.37**
	(0.45)	**(0.68)**	**(0.37)**	**(0.54)**	**(0.30)**	**(0.46)**	**(0.45)**	**(0.40)**
AfterGR × OLD × LAW	**-0.03**	**-0.40**	**0.03**	**-0.71**	**-0.12**	**-0.37**	**-0.49**	**-1.07****
	(0.48)	**(0.78)**	**(0.40)**	**(0.68)**	**(0.36)**	**(0.59)**	**(0.30)**	**(0.52)**
OLD	-8.08***	…	-7.83***	…	-7.76***	…	-7.84***	…
	(0.55)		(0.40)		(0.40)		(0.35)	
LAW	-3.31***	…	-2.88***	…	-1.16	…	-1.35	…
	(1.05)		(0.94)		(1.10)		(1.03)	
GR	-3.08***	…	-2.52***	…	-2.40	…	-2.01***	…
	(0.82)		(0.69)		(0.35)		(0.30)	
AfterGR	-6.13***	…	-5.96***	…	-5.06***	…	-5.31***	…
	(0.94)		(0.73)		(0.60)		(0.53)	
GR × OLD	2.10***	…	1.90***	…	2.29***	…	2.15***	…
	(0.42)		(0.35)		(0.14)		(0.14)	
AfterGR × OLD	4.12***	…	3.96***	…	4.17***	…	4.30***	…
	(0.46)		(0.37)		(0.24)		(0.22)	
GR × LAW	0.69	…	0.57	…	-0.34	…	-0.22	…
	(0.84)		(0.70)		(0.57)		(0.48)	
AfterGR × LAW	1.81*	…	1.65**	…	0.13	…	0.60	…
	(0.98)		(0.78)		(0.81)		(0.69)	
Cumulative effect, 2 years:								
UI benefit extensions	…	0.15***	…	0.10***	…	0.19***	…	0.12***
(weeks) × *OLD*		(0.05)		(0.03)		(0.06)		(0.03)
Age composition control	…	3.88	…	29.34***	…	2.85	…	30.67***
× *OLD*		(8.88)		(9.31)		(9.29)		(9.39)
Includes full set of state × quarter and age × quarter, interactions	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors, clustered at the state level, are in parentheses. *, **, and *** mean statistically significant from zero at the 10--percent, 5-percent, and 1-percent levels, respectively. The sample period is 2004:Q2–2011:Q4. There are 3,038 observations. All estimates are weighted by state population. The hiring variable is constructed by dividing the number of hires in the quarter by the average employment in 2005 for that state and multiplying by 100. For the unemployment insurance and compositional controls, both the contemporaneous variable and lags through eight quarters are included in the even-numbered columns. In these columns, the addition of state-by-quarter fixed effects removes *GR*, *AfterGR*, *LAW*, *GR × LAW*, and AfterGR × *LAW*, and age-by-quarter fixed effects remove *OLD*, *GR × OLD*, and *AfterGR × OLD*. In the regressions, rather than using seasonally adjusted data, all regression models include calendar-quarter dummy variables. To allow for different seasonality by age group and type of state (defined by age discrimination law)—to better match the separate seasonal adjustment we use in the figures—these are also entered interacted with *LAW*, *OLD*, and *LAW × OLD*. (Prior to forming these interactions, the calendar-quarter dummy variables are demeaned, so that the estimated coefficients of *LAW*, *OLD*, and *LAW × OLD* reflect differentials evaluated at the sample means.)

Table 7. Estimated impacts of lower firm-size minimums and larger damages on separations relative to 2005 employment (%).

	Men		Women		Men		Women	
	Firm-size minimums				Larger damages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLD × LAW	**2.35*****	**2.26****	**1.49*****	**1.36***	**1.62****	**1.60****	**1.33****	**1.09**
	(0.68)	**(0.97)**	**(0.53)**	**(0.68)**	**(0.66)**	**(0.91)**	**(0.50)**	**(0.68)**
GR × OLD × LAW	**0.06**	**0.19**	**0.11**	**0.00**	**-0.07**	**-0.04**	**-0.17**	**-0.28**
	(0.44)	**(0.67)**	**(0.36)**	**(0.50)**	**(0.31)**	**(0.47)**	**(0.24)**	**(0.37)**
AfterGR × OLD × LAW	**-0.18**	**-0.55**	**-0.01**	**-0.77**	**-0.19**	**-0.42**	**-0.46**	**-1.06****
	(0.45)	**(0.76)**	**(0.37)**	**(0.66)**	**(0.33)**	**(0.58)**	**(0.27)**	**(0.53)**
OLD	-6.33***	…	-5.92***	…	-5.94***	…	-5.88***	
	(0.56)		(0.41)		(0.40)		(0.35)	
LAW	-3.09***	…	-2.69***	…	-1.01	…	-1.17	…
	(1.01)		(0.90)		(1.07)		(1.00)	
GR	-2.35***	…	-2.11***	…	-1.75***	…	-1.63	…
	(0.67)		(0.57)		(0.33)		(0.30)	
AfterGR	-6.16***	…	-5.61***	…	-5.18***	…	-4.94***	…
	(0.85)		(0.67)		(0.60)		(0.55)	
GR × OLD	2.32***	…	2.02***	…	2.40***	…	2.20***	…
	(0.40)		(0.34)		(0.16)		(0.13)	
AfterGR × OLD	4.60***	…	4.28***	…	4.61***	…	4.58***	…
	(0.42)		(0.34)		(0.22)		(0.20)	
GR × LAW	0.56	…	0.54	…	-0.34	…	-0.19	…
	(0.69)		(0.59)		(0.50)		(0.44)	
AfterGR × LAW	1.57*	…	1.44*	…	0.03	…	0.38	…
	(0.89)		(0.72)		(0.77)		(0.67)	
Cumulative effect, 2 years:								
UI benefit extensions	…	0.15***	…	0.10***	…	0.19***	…	0.12***
(weeks) × *OLD*		(0.05)		(0.03)		(0.06)		(0.03)
Age composition control	…	-4.62	…	27.95***	…	1.87	…	29.28***
× *OLD*		(8.79)		(9.45)		(8.95)		(9.53)
Includes full set of state × quarter and age × quarter interactions	No	Yes	No	Yes	No	Yes	No	Yes

Note: See notes to Table 6. The separations variable is constructed similarly to the hiring variable.

Table 8. Robustness of key results.

	Men, larger damages, unemp. rate (1)	Women, lower firm-size minimum, empl.-to-pop. (2)	Men, larger damages, unemp. durations (3)	Women, larger damages, unempdurations (4)	Women, larger damages, hires (5)
(a) Original results (even-numbered columns in Tables 3 to 6)					
OLD × LAW	-0.26	-0.08	-4.46***	1.71	1.11
	(0.27)	(1.67)	(1.00)	(1.98)	(0.68)
GR × OLD × LAW	0.54	-0.52	5.57**	-4.35	-0.37
	(0.54)	(0.75)	(2.29)	(2.98)	(0.40)
AfterGR × OLD × LAW	1.03***	-1.98**	5.04*	-3.13	-1.07**
	(0.38)	(0.81)	(2.52)	(2.89)	(0.52)
(b) Fully saturated (adding state × old interactions)					
GR × OLD × LAW	0.56	-0.16	5.60**	-4.14	-0.15
	(0.55)	(0.81)	(2.34)	(2.97)	(0.39)
AfterGR × OLD × LAW	1.03**	-0.98	5.10**	-2.93	-0.41
	(0.40)	(0.83)	(2.51)	(2.86)	(0.50)
(c) Dropping missing median duration estimates					
OLD × LAW	…	…	-4.88***	0.90	…
			(1.04)	(2.14)	
GR × OLD × LAW	…	…	5.74**	-4.52	…
			(2.46)	(3.06)	
AfterGR × OLD × LAW	…	…	5.39**	-2.57	…
			(2.51)	(3.09)	
N			10,811	10,672	
(d) Not weighted by state population					
OLD × LAW	-0.16	-0.16	-3.11**	2.13	1.13*
	(0.33)	(1.12)	(1.35)	(1.59)	(0.66)
GR × OLD × LAW	0.43	-0.93	4.77**	-4.54	0.19
	(0.45)	(1.15)	(2.21)	(3.11)	(0.31)
AfterGR × OLD × LAW	0.55	-1.30	2.10	-3.81	-0.40
	(0.52)	(1.10)	(2.67)	(2.67)	(0.46)
(e) 55–64 versus 25–44					
OLD × LAW	-0.20	1.26	-4.43**	0.75	1.05
	(0.28)	(1.31)	(1.88)	(1.97)	(0.84)
GR × OLD × LAW	0.63	0.20	7.86**	-4.51	-0.20
	(0.50)	(1.00)	(3.32)	(4.79)	(0.46)
AfterGR × OLD × LAW	0.87*	-1.47	6.14*	-1.43	-0.89
	(0.46)	(1.07)	(3.10)	(3.25)	(0.62)
(f) 55+ versus 25–34					
OLD × LAW	-0.28	-0.54	-4.03***	2.78	0.58
	(0.33)	(1.93)	(1.33)	(1.92)	(0.66)
GR × OLD × LAW	0.59	-0.84	5.25**	-5.93*	-0.53*
	(0.70)	(0.73)	(2.32)	(3.47)	(0.30)
AfterGR × OLD × LAW	1.28***	-2.37**	5.75*	-4.85	-0.85**
	(0.43)	(0.99)	(3.08)	(3.07)	(0.42)
(g) 55–64 versus 25–34					
OLD × LAW	-0.19	0.73	-4.13*	1.78	0.52
	(0.33)	(1.40)	(2.41)	(1.87)	(0.49)
GR × OLD × LAW	0.59	-0.10	7.40**	-5.97	-0.36
	(0.70)	(0.91)	(3.59)	(5.02)	(0.27)
AfterGR × OLD × LAW	1.10**	-1.71	6.70*	-2.85	-0.67*
	(0.50)	(1.16)	(3.75)	(3.28)	(0.35)

Notes: Estimates are based on the specification used in the even-numbered columns in the earlier tables. We include only the outcome variables and the law characteristics combinations that led to statistically significant results in Tables 3 to 6. No *GR × OLD × LAW* and *AfterGR × OLD × LAW* coefficients for other outcomes coupled with state age discrimination protections that are not shown in this table here were statistically significant. In panel (b) the state-by-age interactions subsume *OLD × LAW*.

Table 9. Falsification test.

	Men, larger damages, unemp. rate (1)	Women, lower firm-size minimum, empl.-to-pop. (2)	Men, larger damages, unemp. durations (3)	Women larger damages, unemp. daurations (4)
(a) Original results (even-numbered columns in Tables 3-6)				
OLD × LAW	-0.26	-0.08	-4.46***	1.71
	(0.27)	(1.67)	(1.00)	(1.98)
GR × OLD × LAW	0.54	-0.52	5.57**	-4.35
	(0.54)	(0.75)	(2.29)	(2.98)
AfterGR ×	1.03***	-1.98**	5.04*	-3.13
OLD × LAW	(0.38)	(0.81)	(2.52)	(2.89)
(b) Falsification: 2003-2007				
OLD × LAW	-0.17	-1.31	-5.00**	0.66
	(0.37)	(1.83)	(2.20)	(3.03)
Post-2004 ×	-0.16	2.04*	0.84	1.38
OLD × LAW	(0.34)	(1.19)	(3.31)	(3.37)

Notes: See notes to Table 8. Estimates are based on the specification used in the even-numbered columns in Table 3 to 6. In panel (b), we define 2003 to 2004 as the pre-recession period and 2005 to 2007 as the recession period. Since many states are missing from the QWI before 2004, we cannot conduct this falsification test for hires.

Table 10. Summary of key results.

		Men, larger damages, unemp. rate (1)	Women, lower firm-size minimum, empl.-to-pop. (2)	**Men, larger damages, unemp. durations (3)**	Women, larger damages, unemp. durations (4)	**Women, larger damages, hires (5)**
OLD × LAW	Mean	-0.24	0.09	**-4.39**	1.71	**1.03**
	Range	[-0.28, -0.16]	[-1.00, 1.26]	**[-4.88, -3.11]**	[0.75, 2.78]	**[0.52, 1.37]**
	# +Sig / −Sig / Not Sig	0/0/10	0/0/11	**0/11/0**	0/0/11	**4/0/6**
GR × OLD × LAW	Mean	0.55	-0.39	**5.82**	-4.65	**-0.29**
	Range	[0.43, 0.63]	[-0.93, 0.20]	**[4.77, 7.86]**	[-5.97, -4.14]	**[-0.53, -0.15]**
	# +Sig / −Sig / Not Sig	0/0/11	0/0/12	**12/0/0**	0/2/10	**0/1/10**
AfterGR × OLD × LAW	Mean	0.99	-1.45	**5.23**	-3.09	**-0.74**
	Range	[0.55, 1.28]	[-2.37, -0.72]	**[2.10, 6.70]**	[-4.85, -1.43]	**[-1.07, -0.40]**
	# +Sig / −Sig / Not Sig	10/0/1	0/5/7	**11/0/1**	0/0/12	**0/6/5**

Notes: This table summarizes the results for the regressions in Tables 3 to 6, the robustness analyses in Table 8, and the additional robustness analyses discussed in the text running the even-numbered regressions in Tables 3 to 6 without controls, with lags of the controls through one or three years, with the smaller firm-size minimum (< 6), and using a continuous measure of the business cycle shock. # + Sig / −Sig / Not Sig counts the number of regressions where the coefficient is positive and statistically significant (at the 10-percent level), negative and significant, or neither. Estimates surrounded by a box are those for which the evidence indicates that stronger age discrimination helped older workers relative to younger workers during or after the Great Recession. Shaded estimates are those that indicate the opposite. Boldfaced sets of estimates are those where the main effect indicates that prior to the Great Recession stronger age discrimination protections helped older workers, but led to relatively worse outcomes for them during or after the Great Recession.

ENDNOTES

[1] See, for example, http://www.nytimes.com/2013/02/03/business/americans-closest-to-retirement-were-hardest-hit-by-recession.html?pagewanted=all&_r=0, http://www.nytimes.com/2009/04/13/us/13age.html?pagewanted=all and http://www.cbsnews.com/2100-501445_162-4944750.html (all viewed April 16, 2013).
[2] It is also possible that there was no change in actual age discrimination during and after the Great Recession, but the large numbers of layoffs that occurred, coupled with reduced hiring, led to changing perceptions of age discrimination among older workers.
[3] This type of story parallels models in which, more generally, firms undertake more organizational restructuring or labor reallocation during economic downturns (e.g., Aghion & Saint-Paul, 1998; Davis & Haltiwanger, 1990; Koenders & Rogerson, 2005).
[4] We do not use mean duration due to bias from top coding and changes to the top coding in January, 2011, from two years to five (http://www.bls.gov/cps/duration.htm, viewed April 13, 2013).
[5] Owing to small sample sizes in some cells, in particular for older individuals in small states, there are occasionally cells with no unemployed individuals in the sample, in which case unemployment durations cannot be estimated. For our sample period there are two cells of younger men, four cells of younger women, 200 cells of older men, and 331 cells of older women with no unemployed observations, out of a total of 5,400 observations for each age group. For these cases, we replace the missing unemployment duration variables with zeroes. As we discuss later, the results are insensitive to dropping these cells from the analysis.
[6] Moreover, for the CPS data (although not the QWI data discussed below), we are using a sample of the population to estimate the data for each state and month cell, which provides an econometric rationale for weighting to account for the greater accuracy of the estimates from large cells.
[7] These were were downloaded from the Cornell University's Virtual Research Data Center. The QWI provides data for all states and the District of Columbia, with the exclusion of Massachusetts. We use the R2013Q1 release, as of May 7, 2013. By downloading data from the Cornell RDC website, we acknowledge support by NSF Grant #SES-0922005 that made these data possible.
[8] See http://www.vrdc.cornell.edu/qwipu/starting_dates.html (viewed May 20, 2013).
[9] We confirmed that results using an unbalanced panel beginning in 2004:Q2 and the later data for DC were very similar.
[10] McLaughlin (1991) points out that thinking of quits as voluntary separations and layoffs as involuntary separations may not be correct.
[11] Neumark and Song (2013) find that older workers tend to work at smaller firms, which could reinforce the effects of these lower firm-size minimums. This is also echoed in 2011 data from the U.S. Small Business Administration (2012), which show that the percentage of workers aged 65 and over who work at firms with fewer than 50 employees jumps markedly (by 10 percentage points), relative to those aged 55 to 64 (and also drops with age over other age ranges), and correspondingly the percentage at firms with 500 or more employees drops by 9 percentage points. Nonetheless, lower firm-size minimums are irrelevant for many employers.
[12] The only changes during our sample period were when Nebraska changed its minimum firm size from 25 to 20 in 2007 and when Oklahoma changed from 15 to 1 in December 2011. Given our classification of states, only Oklahoma's change would require recoding, but given that this change occurred in the final month of our sample, we ignore it as it could only have a negligible effect.
[13] For example, following the Great Recession, aggregate U.S. economic growth became positive in the third quarter of 2009 (http://www.bea.gov/national/index.htm#gdp, viewed August 27, 2012), whereas job growth (as measured by the payroll survey) did not become positive until the fall of 2010 (http://www.bls.gov/webapps/legacy/cesbtab1.htm, viewed August 27, 2012). (It actually ticked up seven months earlier, but then declined again slightly.)
[14] This approximates the seasonal adjustment used in Figure 1, and in the other figures described below.
[15] We also report the sensitivity of the estimated triple-difference parameters to adding the age-by-state interactions, and find very similar results.
[16] For these calculations we wanted to measure growth between the same calendar months to avoid seasonality. We therefore use December 2007 to December 2008 for the Great Recession and June 2009 to June 2011 for after. The start dates we use (December 2007 and June 2009) match the start and end dates of the Great Recession according to the NBER.

[17] All appendices are available at the end of this article as it appears in the JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley/com/cgi-bin/jhome/34787.
[18] Note that in the saturated model *SA*, *SA* × *OLD*, and *SA* × *LAW* from equation (1) drop out.
[19] The data are seasonally adjusted using X-12-ARIMA.
[20] If we simply add the controls to the specification in column 1, we can also identify the effect of the UI benefit extensions on the reference younger group, and overall. In this case the sum of the main effects, which reflects the effect on older workers of an extra week of benefits that lasts for two years, was 0.11 and statistically significant. To put the estimate in perspective, it implies that a 9.1 week extension that lasted for two years would add 1 percentage point to the unemployment rate. We do not necessarily attribute a causal interpretation to this because the extensions are triggered by unemployment rates.
[21] This is generally true for all of the models we estimate below, so we do not revisit this point, nor discuss further the estimated coefficients of these control variables.
[22] The difference in the early period, reflected in the estimated coefficient of *GR*, has been noted in the popular press, which at the height of the Great Recession coined the label "mancession" (http://economix.blogs.nytimes.com/2009/08/10/the-mancession/?_r=0, viewed October 18, 2013). This was attributed to the overrepresentation of men in cyclically sensitive industries like construction and manufacturing that were hit hardest initially. However, when government employment fell later on as states faced budget crunches, women experienced larger job losses (http://www.sfgate.com/news/article/Women-hit-harder-by-government-job-cuts-4322420.php, viewed October 18, 2013). (These overall trends are more likely to be reflected in the unemployment rates for younger men and women, because their employment rates are so much higher.) Moreover, as documented by Hoynes et al. (2012), the recovery has been stronger for men (which they term a "he-covery").
[23] We also estimated all of these robustness analyses for the other specifications for which we did not find any significant estimates of the DDD interactions *GR* × *OLD* × *LAW* and *AfterGR* × *OLD* × *LAW*, and verified that in no case did the additional analyses discussed below lead to significant estimates.

To conserve space, we do not report the results for separation rates of women, which, as we have noted, very closely parallel the results for hiring. We did check all the results discussed below and verified that the conclusions with regarding to the hiring specifications carry over to separations.
[24] There is an issue largely confined to the legal literature about the ADEA not helping older women significantly because of the inability of women to file *intersectional* discrimination claims based on age and gender, since age and gender are covered in separate statutes (the ADEA and Title VII). Song (2013) discusses this work and presents some evidence that the initial enactment of the ADEA and earlier state age discrimination laws did not do as much to help older women. However, there is not related information on these kinds of differential effects in the contemporaneous period. And unless age discrimination is worse for women than for men, the inability to file intersecting claims does not disadvantage women. As the results indicate, we do not find much evidence of weaker effects of age discrimination laws for women.
[25] A reviewer pointed out one possible exception. Specifically, the pent-up demand for discrimination could be driven by moral hazard among older workers protected by stronger age discrimination laws, leading them to become less productive. If such behavior led to, for example, lower human capital investment or deterioration of work habits that would deter their hiring even when age discrimination protections are strong, then older workers terminated during or after the Great Recession could face persistently worse employment prospects going forward.
[26] Since Massachusetts is missing from the QWI, we use CPS data to generate SE_{ask03} for the state.
[27] Since Arizona has missing data in 2003, we use 2004 as the baseline for that state. See http://www.vrdc.cornell.edu/qwipu/starting_dates.html (viewed May 20, 2013).