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Did Age Discrimination Protections Help Older Workers Weather the Great Recession?

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Did Age Discrimination Protections Help Older Workers Weather the Great Recession?

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 workers in states with stronger and weaker laws, to their younger counterparts, both 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 this may be because 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, or because higher termination costs associated with stronger age discrimination protections do more to deter hiring when future product and labor demand is highly uncertain.

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I. 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 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 are seeking new employment, have had greater difficulty becoming re-employed.

The implication is that the effects of the Great Recession – which are likely to linger for many years – 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 and Duggan, 2003; Dorn and Sousa-Poza, 2010; Riphahn, 1997). Difficulties in getting hired seem likely to exacerbate these effects, making it harder to return to employment, and forestalling efforts of older individuals to find the partial-retirement jobs that they often use to bridge career employment to retirement (Cahill et al., 2005; Johnson et al., 2009).

The increase in unemployment durations for older workers has led to speculation that age discrimination plays a role.[1] One possible perspective is a static one. In particular, there may be no change in the tendency of employers to engage in age discrimination during and after the Great Recession. Nonetheless, the large numbers of layoff that occurred may have put more older workers at risk of experiencing age discrimination, leading to large increases in unemployment durations.

Alternatively, there may have been increases in discrimination. 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 argument goes back to Ashenfelter (1970) and Freeman (1973), and was recently considered by Biddle and Hamermesh

[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).

(2013). The data are consistent at least with rising perceived discrimination. As Figure 2 shows, based on Age Discrimination in Employment Act (ADEA) claims filed with the U.S. Equal Employment Opportunity Commission (EEOC), that age discrimination claims rose sharply at the beginning of the Great Recession, and have remained elevated. (Claims also rose during the earlier recession covered in this graph, but subsequently fell more quickly.)

This basic hypothesis that age discrimination may have increased or become more important during the Great Recession, and remained prominent, provides a simple motivation for our analysis. In particular, many states offer stronger protections against age discrimination than the federal ADEA. Recent work finds that these stronger state protections affect employment and Social Security benefit claiming of older individuals, leading to more delaying of claiming benefits until the Full Retirement Age (FRA) and increased employment prior to the FRA, in part because stronger age discrimination protections are associated with increased hiring of older individuals into new jobs (Neumark and Song, forthcoming).

Motivated by this hypothesis and this other evidence, we ask whether these stronger age discrimination protections at the state level also acted to protect older workers during and after the Great Recession. Of course we do not actually know whether age discrimination was or is occurring. But we can ask whether these state protections reduced the adverse effects of the Great Recession on older workers relative to younger workers.

The reality can, however, be more complicated. The labor market turbulence that severe recessions create may make it difficult to sort out the effects of age discrimination versus changing business conditions on employment adjustments, reducing the likelihood that workers, attorneys, or the EEOC or state commissions that enforce state anti-discrimination laws perceive age discrimination, or that claims of age discrimination can prevail. These conjectures suggest that stronger age discrimination protections may be particularly valuable during severe recessions, in which case older workers should have fared relatively better during and after the Great Recession in states with stronger protections. On the other hand, the diminished effectiveness of age discrimination protections during recessions could give rise to a situation in which the Great Recession led to worse outcomes for older worker in states with stronger age

discrimination protections. One possibility is that because stronger state age discrimination laws impose constraints on employers, there could be more "pent-up demand" for age discrimination in these states, which firms act on during a sharp downturn. This type of story has parallels to economic research arguing that firms may undertake more organizational restructuring or labor reallocation during economic downturns (e.g., Aghion and Saint-Paul, 1998; Davis and Haltiwanger, 1990; Koenders and Rogerson, 2005).

An alternative story with the same consequence stems from past work thinking about the effects of anti-discrimination laws on hiring and terminations. In particular, there has been some speculation that age discrimination laws may reduce hiring of older workers. Why? In hiring discrimination cases it is difficult to identify a class of affected workers, and for this reason and others economic damages can be much smaller than in termination cases (Adams, 2004; Posner, 1995), reducing the effectiveness of these laws in hiring cases. Moreover, because the ADEA makes it more difficult to terminate older workers (Neumark and Stock, 1999), it may actually *discourage* their hiring (Bloch, 1994; Lahey, 2008a). This might be a better argument for younger groups protected by anti-discrimination laws than for older workers, because older workers are unlikely to stay with employers for a long time (in contrast, say, to a decision about hiring a young black man protected by race discrimination laws). However, during and after the Great Recession, product and labor demand may have been sufficiently uncertain 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.

Because of these considerations, we present estimates that distinguish the effects of age discrimination protections before, during, and after the Great Recession. To summarize the results, we find some mixed evidence, but generally 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 mediated 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 damages than the federal ADEA were associated with relatively higher unemployment rates of older men (by about one 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 in states with stronger age discrimination protections in the form of applicability of state laws to smaller employers than those covered by the ADEA experienced larger relative declines in the employment-to-population ratio (by about 1.5 percentage points), and where larger damages are allowed there was also a larger relative decline in the hiring rate (by about 0.3 percentage point during the Great Recession, and 0.7 percentage point afterwards). 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.

In general, then, older workers bore more of the brunt of the Great Recession in states with stronger age discrimination protections. This contrasts with the pre-Great Recession period, when both types of stronger state age discrimination laws were associated with better relative outcomes for older workers – most consistently with regard to hiring rates. Thus, this evidence is consistent with the interpretation that in normal times age discrimination protections help older workers, as suggested by past work (Neumark and Stock, 1999; Adams, 2004). However, during an experience like the Great Recession age discrimination may worsen either because severe labor market disruptions make it difficult to discern discrimination, so that employer behavior in states with and without stronger age discrimination protections becomes more similar, or 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.

II. Related Research

There are four strands of related prior research. First, existing research provides ample evidence that age discrimination remains pervasive (Neumark, 2008). Moreover, some research as well as a good

deal of conjecture suggests that age discrimination is particularly pervasive with regard to hiring (Adams, 2004; Hirsch et al., 2000; Hutchens, 1988; Lahey, 2008b; Posner, 1995).

Second, research establishes the effects of state and federal age discrimination laws in increasing employment of protected older workers (Neumark and Stock, 1999; Adams, 2004), although not new hiring (Adams, 2004; Lahey, 2008a). More recent evidence establishes that state age discrimination protections that are stronger than the ADEA made it easier for workers affected by increases in the FRA to remain employed, and finds evidence that these stronger state age discrimination protections were associated with increased hiring of those affected by increases in the FRA (Neumark and Song, forthcoming).

Third, research has begun to look at some of the effects of the Great Recession on older workers. Gustman et al. (2011) find little impact on flows into retirement, although their data go only through 2010 and the labor market for older workers worsened subsequently. 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 the 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 et al. (2009) note that the increase in unemployment rates for older men relative to younger men was higher (for the December 2007-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 decline in manufacturing as a share of employment – a sector with considerable protections for more-senior workers – and a decline 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 bigger for older workers (aged 51-60), especially in relative terms since their counterfactual no-displacement earnings are higher. However, the relative difference between displacements occurring during recessions or not are more modest for older workers than for younger workers (although relative to counterfactual earnings, the losses are larger for older workers displaced in non-recessionary periods than for any of the younger groups during recessions).

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 (perhaps more than other workers). However, there is no existing research that ties these phenomena together to ask whether age discrimination laws mediated the effects of the Great Recession on older workers.

III. 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.

Current Population Survey (CPS)

The CPS monthly micro-data were used to construct estimates by state, month, age group, and sex of the unemployment rate, the employment-to-population ratio, and median unemployment duration.[2] Population weights were used to create statistics that are representative of the populations within each state, age group, sex, and month cell.

The age groups we use are younger (prime-age) individuals (ages 25 to 44) and older individuals (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 (25-44) 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 40-44. In

[2] 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).

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 sensitivity analyses later 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 sex, both unweighted and then weighted by state population.[3] The weighted estimates weight more populous states more heavily, leading 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.[4] We do, however, also discuss the sensitivity of our results to using unweighted estimates.

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, for the weighted estimates), 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 women (weighted estimates).

Quarterly Workforce Indicators (QWI)

For the QWI-based estimates of hiring, quarterly data by age, sex, and state were downloaded from the Cornell University's Virtual Research Data Center.[5] The QWI provides data in age groups bins, so the

[3] Most likely due 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 zeros. As we discuss later, the results were insensitive to dropping these cells from the analysis.

[4] 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.

[5] 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.

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 sex as well. QWI data became available for different states at different times, and are updated for each state at different times.[6] Data from all states are available from 2004:Q2 to 2011:Q4 for hires, and 2005:Q3 to 2011:Q4 if DC is included. We use 2004:Q2 to 2011:Q4 and exclude Washington, DC, to create a balanced panel.[7] We divided hires by the average employment level from the QWI in 2005, to normalize hires 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 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 sex. 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.

The QWI also reports data on separations, but not the reason for the separation (see Abowd et al., 2009, p. 208). Because we cannot distinguish quits and layoffs, it is difficult to interpret results for separations. For example, if age discrimination laws are associated with fewer separations for older worker 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? The latter may be more likely, since more separations are voluntary, although the effects we estimate could still be driven by the involuntary separations.[8]

[6] See http://www.vrdc.cornell.edu/qwipu/starting_dates.html (viewed May 20, 2013).
[7] We confirmed that results using an unbalanced panel beginning in 2004:Q2 and the later data for DC were very similar.
[8] McLaughlin (1991) points out that the interpretation of quits as voluntary and layoffs as involuntary may not be correct.

State Age Discrimination Laws

Data on age discrimination laws at the state level were compiled for Neumark and Song (forthcoming) and are used here. In this paper 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 number of employees working at a firm for state age discrimination laws to apply. Whereas the ADEA applies for firms with 20 or more workers, many states have lower minimums, and some apply to firms that have only one employee. Age discrimination laws are stronger – covering more workers – the lower this minimum firm size.[9] Figure 3 shows the minimum firm size required for each state as of 2003. Following Neumark and Song (forthcoming), we categorize states as either having lower firm-size minimum (fewer than 10) or higher firm-size minimum (10 or more).[10]

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 damage (henceforth "larger damages"). These are shaded in Figure 3. There were no changes to this classification of states during our sample period. Three states (AR, MS, and SD) do not have state age discrimination laws, and these are put in the higher firm-size minimum group and classified as not having larger damages, because in these states the ADEA prevails.

[9] Neumark and Song (forthcoming) 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 Small Business Administration, 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 55-64 (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. Data from 1995 cover firms with fewer than 25 employees and the result is starker. The percentage of workers at these small firms around 25 percent for ages 25-64, and jumps to 44 percent for those aged 65 and older. (See U.S. Small Business Administration, 2012.) Nonetheless, lower firm-size minimums are still irrelevant for many employers.

[10] Since 2003, there have been few changes to these laws; the only change during our sample period is when Nebraska changed its minimum firm size from 25 to 20 in 2007 and when Oklahoma changed it from 15 to one in December 2011. Since we classify states by having a lower firm-size minimum (fewer than 10) or higher firm-size minimum (ten or more), only Oklahoma's change requires recoding, and given that this change occurs in the final month of our sample, we ignore it as it could only have a negligible effect.

IV. Methods

To infer how stronger state age discrimination laws mediated 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 in this period relative to the pre-recession period. 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. With regard to shocks, for example, the industries that were more affected by the Great Recession may have tended to employ a greater share of older workers in some states.

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.

At the same time, as discussed in the Introduction, we are also interested in how labor market outcomes differed between older and younger workers in the period prior to the Great Recession, and how these differences were associated with stronger state age discrimination protections. The coefficients needed to reveal 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 for the post-recession period 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 choose to consider the recession period itself and the ensuing period separately, in part because (as we will see) the labor market dynamics were quite different in these periods, and in part because labor market changes often lag the output changes that define recessions.[11] In addition, we might expect the data to be more reflective of the influence of age discrimination laws (and other factors) 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 earlier, 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 seasonal adjustment:

$$\begin{aligned} Y_{ast} = \beta_0 + \beta_1 OLD_a + \beta_2 LAW_s + \beta_3 OLD_a \times LAW_s + \beta_4 GR_t + \beta_5 AfterGR_t + \beta_6 OLD_a \times GR_t \\ + \beta_7 OLD_a \times AfterGR_t + \beta_8 LAW_s \times GR_t + \beta_9 LAW_s \times AfterGR_t \\ + \boldsymbol{\beta_{10} OLD_a \times LAW_s \times GR_t + \beta_{11} OLD_a \times LAW_s \times AfterGR_t} + SA_t\lambda_1 + SA_t \times OLD_a\lambda_2 + \\ SA_t \times LAW_s\lambda_3 + SA_t \times OLD_a \times LAW_s\lambda_4 + \varepsilon_{ast} \end{aligned} \quad [1]$$

where the subscript a indexes the age group – younger (25 to 44) or older (55+) – s indexes the state, and t indexes time. The CPS data are monthly and extend from January 2003 to December 2011; the QWI data are quarterly and cover 2004:Q2 to 2011:Q4. Y_{ast} is the outcome variable, *OLD* equals one for the older group, and zero for the younger group, *GR* is a dummy for the time period of the Great Recession, *AfterGR* is a dummy for the time period after the Great Recession, and *LAW* is a dummy variable, varying across analyses for the two indicators we use of stronger state age discrimination laws. Rather than seasonally adjusting the data used in the regressions, we simply include calendar-month (CPS) or calendar-quarter

[11] 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.)

(QWI) dummy variables – denoted *SA* in equation [1] – including their interactions with *OLD*, *LAW*, and *OLD* × *LAW*, to approximate the seasonal adjustment made in the figures.

The DDD parameters, which of 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 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:Q2-2007: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. As outlined in the Introduction, estimates of this parameter can be helpful in putting a theoretical interpretation on the results. At the same time, we might be less confident in a causal interpretation of this parameter because it is identified solely from cross-sectional variation, by age, 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 specifications that more flexibly saturate the model. In all of our main tables, we also report estimates of augmented versions of the DDD model that add a much greater level of saturation. 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, because this difference is potentially informative about the effects of state age discrimination laws in the pre-Great Recession period.[12]

In addition to the dummy variable interactions, we added 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 due to 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). To account for the lagged labor market effects of the extensions, we also include lags of this variable through two years. This variable (and all its lags) 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 ratio of older to younger workers, the correlations for the period of the Great Recession

[12] We also report the sensitivity of the estimated triple-difference parameters to adding the age by state interactions, and find very similar results.

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.[13] Thus, to a limited extent, industries hit hardest during the Great Recession tended to employ relatively more older workers, and correspondingly 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 the Appendix A. 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]):

$$Y_{ast} = \beta_0 + \beta_3 OLD_a \times LAW_s + \boldsymbol{\beta_{10} OLD_a \times LAW_s \times GR_t} + \boldsymbol{\beta_{11} OLD_a \times LAW_s \times AfterGR_t}$$
$$+ State_s \times Time_t \gamma + Age_a \times Time_t \delta + \sum_{k=t}^{t-m} X_k \times OLD_a \pi_k + SA_t \lambda_1 + SA_t \times OLD_a \lambda_2 + \quad [2]$$
$$SA_t \times LAW_s \lambda_3 + SA_t \times OLD_a \times LAW_s \lambda_4 + \varepsilon_{ast}$$

where *Time* is a vector of month dummy variables for the CPS data, and quarter dummy variables for the QWI data.

V. Results

Based on the arguments outlined above, our analysis focuses mainly 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 is also intended to provide information on whether stronger state age discrimination protections were associated with relatively better labor market outcomes for older workers in the baseline period prior to the Great Recession.

For each outcome, we first provide information on these questions by presenting a series of figures

[13] 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 GR according to the NBER.

that show the levels and differences over time.[14] For each outcome and for each type of law (lower firm-size minimum and larger damages), and for men and women, we present three figures. The first presents seasonally-adjusted time-series estimates for each of the four groups defined by age and the age discrimination laws. For the estimates derived from CPS data, the estimates are weighted by state population using the provided population weights, so the estimates are representative of the population in states with or without stronger laws. For the QWI data, the total number of hires are summed for states with and without stronger laws, and then divided by the sum of employment in these states in 2005. These estimates are implicitly weighted by state population, since larger states contribute more weight to the calculation. The second figure shows the difference in the time-series between older and younger workers, for states with stronger versus weaker laws. And the third figure shows the difference between these. The latter figure provides a difference-in-difference estimate at each point in time, and comparing this across time is then informative about how the influence of age discrimination laws on older versus younger workers varied with the onset of the Great Recession.

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 that could have differentially affected older and younger workers across states, in ways that differ during and after the Great Recession compared to earlier.

Unemployment rates

The top panels of Figure 4 show that, for both sexes, and irrespective of state age discrimination laws, 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

[14] In these figures, the data are seasonally adjusted using X-12-ARIMA.

older workers depending on whether there was a stronger state age discrimination law, 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 rose 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 the 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 for its age discrimination law 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. Looking at the bottom row, for men there is 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 is even 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.

Thus, for unemployment rates, there is relatively little indication that stronger state age discrimination protections protected older workers from increases in unemployment 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)-(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 – showing first the estimates of equation [1], and then the more-saturated model with controls (equation [2]) – and the next two columns are for women.

Turning to the estimates for firm-size minimums for men, first consider, as a preliminary, the evidence regarding some of the main effects. The estimated coefficient for *OLD* shows 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 then after the Great Recession. The differences, of course, are sharp – about two percentage points higher during the Great Recession, and five 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 one percentage point.

The main estimates of interest are 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 stronger age discrimination protection in the form of lower firm-size minimum. 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, only the coefficients of most interest – on *OLD* × *LAW*, *GR* × *OLD* × *LAW*, and *AfterGR* × *OLD* × *LAW* – are identified. As column (2) shows, the estimates are essentially unchanged.

Note that the UI extensions were not associated with differential effects on unemployment rates of older workers, as the estimated coefficient (0.01) is very small and insignificant.[15] The estimated coefficient of the age composition control interacted with OLD is negative but not significant; the negative sign is as expected since this control indicates that national trends in industry employment were favorable to older workers in the state, so their unemployment rate rose by less. The estimated sum of the coefficients is very large, but recall that these coefficients reflect a one percentage point differential

[15] 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 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 one percentage point to the unemployment rate. We do not necessarily attribute a causal interpretation to this because the extensions are triggered by unemployment rates.

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.[16]

Column (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 men.[17] Turning to the DDD estimates, the 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)-(4) of Table 3 do not provide evidence that a stronger age discrimination law in the form of a lower firm-size minimum for applicability of state 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 protection led to smaller increases in unemployment; and indeed for women the point estimates for the period after the Great Recession suggest if anything the opposite.

Columns (5)-(8) turn to the same specifications, but looking at stronger age discrimination protections in the form of larger damages, for which Figure 5 gave a stronger indication that this age discrimination protection *worsened* the effects of the Great Recession. Having gone through columns (1)-(4) in detail, we can summarize the results in columns (5)-(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

[16] This is generally true for all of the models we estimate below, so we do not revisit this point, nor discuss the estimated coefficients of these control variable more.

[17] 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.")

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 one – and statistically significant at the one-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 one percentage point.

In contrast, the estimated coefficient of *OLD* × *LAW* is negative for men, consistent with larger damages lowering unemployment of older workers *prior* to the Great Recession (by about 0.25 percentage point), 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 many of our 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. The formats of the figures for the graphical analysis and the tables for the regression analysis are the same, so we just highlight the main results.

As shown in the middle panels of Figure 6, 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. The bottom figure for men does not show 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 Figure 7, the results are similar for women. Older women show 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 a worsening in the relative employment-to-population ratios of older men in the period immediately subsequent to the Great Recession.

The regression estimates in Table 4 do 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 $GR \times OLD \times LAW$ and $AfterGR \times OLD \times LAW$). The one exception is in column (4), where the estimate implies that where firm-size minimums are smaller, women's employment-to-population ratio 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, suggesting employment-to-population ratio lower by 1.22 percentage points during the Great Recession, and 0.73 percentage points afterward. In contrast, the estimated coefficients of $OLD \times LAW$ 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.

Unemployment durations

Figure 8 does not provide 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. Looking at the bottom panels, 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 increase in median durations for women, but the period after the Great Recession the figure shows perhaps some increase in durations.

In Figure 9, the pre-Great Recession period exhibits significantly shorter durations for older men in states with larger damages; echoing earlier results, this is 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 afterward, 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 for median unemployment durations. In columns (1)-(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.

For larger damages, as reported in columns (5)-(8), the evidence is stronger (and more consistent for each gender, although different between men and women). 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 the Great Recession and after the Great Recession. The estimates are generally statistically significant at the one-, five-, 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

Turning to hiring in the QWI data, the top panels of Figure 10 show the dramatic drop in hiring rates for all groups, in both sets of states, during the Great Recession, and how these hiring rates have remained low. The middle panels show that the hiring rates for older men and women have been consistently higher in states with a lower firm-size minimum, consistent with this type of age discrimination boosting hiring of older workers. However, the middle and bottom panels do not reveal much evidence of a change in the relative hiring rate of older workers during or after the Great Recession. For larger damages – shown in Figure 11 – there is 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.

Similarly, in the regression estimates in Table 6 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. Paralleling Figure 11, the evidence indicates that larger damages under state law were associated with relative declines in the hiring of older women in the period after the Great Recession, with the significant estimate in column (8) indicating that the hiring rate declined by 1.07 percentage points in relative terms. Note also that in both tables, and for men as well as women, the *OLD* × *LAW* coefficient estimates are positive and (almost always) 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.[18]

[18] As noted earlier, the QWI data on separations are harder to interpret, because they reflect both voluntary and involuntary separations. We report estimates of the same models for separations in Appendix Table B1. We find that the estimated coefficients of *OLD* × *LAW* are always positive and almost always significant, ranging from about a 1.1 to 2.4 percentage point 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.

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 the 1960s) on the effects of age discrimination laws on employment rates of older men, which point to increases of about four 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 four percentage points. There is no evidence on which to ask how the extent of discrimination might vary 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 four percentage points. And we have to recognize that this four 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 four percentage points. Suppose we cut it in half, to two percentage points. Then the question becomes whether we can detect as significant changes of two 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 that 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 0.4-0.8, meaning that we can detect as significant differences of about 0.8-1.6 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 10-25% range pre-Great Recession but drop by as much as 10 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 these for the five cases where we found significant differences in the relative effect of the Great Recession on older workers between states with stronger age discrimination protections and states without them. 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.[19]

The results of many of these analyses are reported in Table 7. In each case, the change indicated in the panel heading is relative to the baseline specification from the even-numbered columns of Tables 3-6, 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 for each outcome by state. In this case, we saturate the model as fully as we can with regard to all the two-way

[19] 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 these additional analyses discussed below lead to significant estimates.

interactions in equation [1]. The coefficient of *OLD* × *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.

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 population in the state and month. 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 more representative of what happened to workers during and after the Great Recession.

Finally, in panels (e)-(g) we use different age groups to define older and younger. In the first panel, we restrict the older group to 55-64 rather than 55+, and in the last two panels we reports results for the alternative definitions of the older group, but a more narrowly-restricted younger group (25-34 instead of 25-44). We view the results as qualitatively very similar. And more generally, across all of the robustness analyses we have explored this is true, although as Table 7 shows some analyses – especially not weighting – lead to far fewer significant results.

We did two other analyses for which results are available from the authors 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. 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 where we substituted for *GR* and *AfterGR* a continuous measure that captures the severity of the Great Recession within each state. This effectively means that we interact *GR* and *AfterGR* with a variable with only cross-state variation, allowing 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 sex, related to our age composition control – 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

Finally, Table 8 reports results for a falsification test. The principal concern 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). So it is possible that changes in age profiles of our outcomes over time that are correlated with differences in state age discrimination laws could spuriously generate our results.

We test for this alternative explanation of our results by choosing a recent, sustained period without a recession and asking if changes over this period generate similar findings. We consider the same five outcomes and state age discrimination protections as in Table 7, using the period 2002-2007, and treating 2005-2007 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 for the CPS data because the QWI data for many states do not go back far enough.

As the estimates in panel (b) 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 to the earlier baseline period. We do find a positive and significant effect coefficient on *Post-2004 × OLD × LAW* for the employment-to-population ratio for women, for lower firm-size minimums. But note that this is the opposite sign from the original results, and hence does not point to a trend toward lower employment of 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 8 rule out spurious trends driving the results.

Summary of results

Table 9 summarizes the results across both the main results in Tables 3-6, and the other robustness analyses we have reported. We report results for the two key triple-difference coefficients, of *GR × OLD × LAW* and *AfterGR × OLD × LAW*, which measure the change in labor market outcomes for older versus younger workers, during or after the Great Recession relative to the prior period, in states with stronger age discrimination protections versus other states. We also report on the estimates for *OLD × LAW*, which captures the difference in outcomes, associated with stronger protections, for older versus younger workers prior to the Great Recession. 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) and positive, (2) significant and negative, and (3) insignificant.

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 mediated 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, 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 men by on average about 5.5 weeks, and unemployment rates that were higher by about one percentage point (in the post-Great Recession period). These 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 the unemployment durations of older women during the Great Recession – by about 4.7 weeks; the one cell in the table reflecting this 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 for age discrimination laws older women had larger declines in their 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); these estimates indicating adverse effects of stronger age discrimination protections on older women are also 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* × *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 1.0 percentage points. (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 9. 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.[20] 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.

VI. Conclusions and Discussion

We generally do not find evidence that stronger state age discrimination protections helped older workers weather the Great Recession, and 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 – for these same outcomes, in particular 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

[20] There is an issue largely confined to the legal literature about the ADEA not helping older women so much because of the inability of women to file "intersectional" discrimination claims based on age and sex, since age and sex 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.

protections offered prior to the Great Recession were largely eroded during and after the recession. In general, this evidence emerges much more strongly for larger damages than for 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, whereas firm-size minimums apply to a subset of workers and firms.

This evidence is consistent with some of the theoretical conjectures we outlined in the introduction. In particular, the evidence that older workers fare better in normal times in states with stronger age discrimination protections is consistent with the idea that these protections help combat discrimination, and in general help older workers, and consistent with past work finding that the initial adoption of state and then federal age discrimination laws laws increased employment of older men (Neumark and Stock, 1999; Adams, 2004).

The evidence on unemployment durations and on hiring in particular is inconsistent with some claims in the literature that age discrimination laws generally worsen outcomes for older workers by deterring hiring, perhaps most plausibly because these laws increase termination costs. In particular, Lahey (2008a) studies the period pre- and post-1977. Looking first at 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 laws affected older workers. In the subsequent period, however, her evidence suggests that state age discrimination laws reduced employment of white men older than 50 years of age (measured as weeks worked), and made such individuals more likely to be retired. She also finds lower hiring rates for older workers where there are state laws. She suggests that because the ADEA makes it difficult to terminate the employment of older workers, it ends up deterring their hiring in the first place. This may be exacerbated by the difficulty of bringing suit over age discrimination in hiring, as discussed in the Introduction.

Lahey's post-1978 results are identified from cross-state variation in laws and outcomes for older versus younger workers. In that sense, these results are similar to our results for the pre-Great Recession

period, and therefore point in the opposite direction. However, there are some important differences and other explanations.

First, Lahey studies the existence of state laws, rather than focusing on the features of these laws that we study. What the existence of a state law per se does is lengthen the statute of limitations for filing a claim. However, Neumark and Song (forthcoming) found, in a different type of study of the effects of state age discrimination laws on older workers, that variation in the length of statutes of limitations has no effect. Second, we have found, as explained above, 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 1978-1991 period, 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, 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.

In other words, Lahey is identifying the effects of age discrimination laws from cross-state variation only – the post-1978 differences between states with and without their own laws. But if the more important source of variation in the strength of age discrimination laws is the strengthening of the federal legislation post-1978, and the "catching up" of the strength of age discrimination laws in states that did not

previously have their own laws to those that did, then the evidence points in the opposite direction. 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. That said, this is still an unsettled question that deserves more research.

Moreover, our findings suggest that the effectiveness of age discrimination laws, and in particular their effects on hiring of older workers (which we presume can be reflecting in hiring rates or unemployment durations), vary over the business cycle. We discussed why the effectiveness of age discrimination laws could decline during severe downturns like that experienced during the Great Recession – and why stronger age discrimination protections could increase discrimination against older workers relatively more during such downturns. We suggested this could occur because in states with stronger laws, there is more pent-up demand to shed older workers, and this can be done during recessions when age discrimination protections are less effective. 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. Naturally it would be useful to try to distinguish among these alternative explanations; but that awaits further research.

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 happens is that there are 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.[21] On the other hand, the longer-term effects of even temporary changes in behavior could turn out to be adverse. 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

[21] 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.

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 terminations or other dimensions of 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.

Appendix A: Age Composition Control

Let subscripts *s* index states, *a* age group, *g* gender, and *k* industries. Denote by SE_{asgk03} total employment for age group *a*, in state *s*, for gender *g* and industry *k*, in the baseline year of 2003. Denote by AE_{kt} national (aggregate) 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 and state (and gender) based solely on national employment changes subsequent to the base year of 2003, by applying the national changes to the baseline composition, as in

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

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 and state (and gender).[22] 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.[23] 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:

$$CC_{gst} = \{(\log(PE_{old,g,s,t}) - \log(PE_{old,g,s,t-1}) - [\log(PE_{young,g,s,t}) - \log(PE_{young,g,s,t-1})]\} \times 100. \quad [A.2]$$

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 sex 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

[22] Since Massachusetts is missing from the QWI, we use CPS data to generate SE_{ask03} for the state.
[23] 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).

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 weighting during the base year of 2003.

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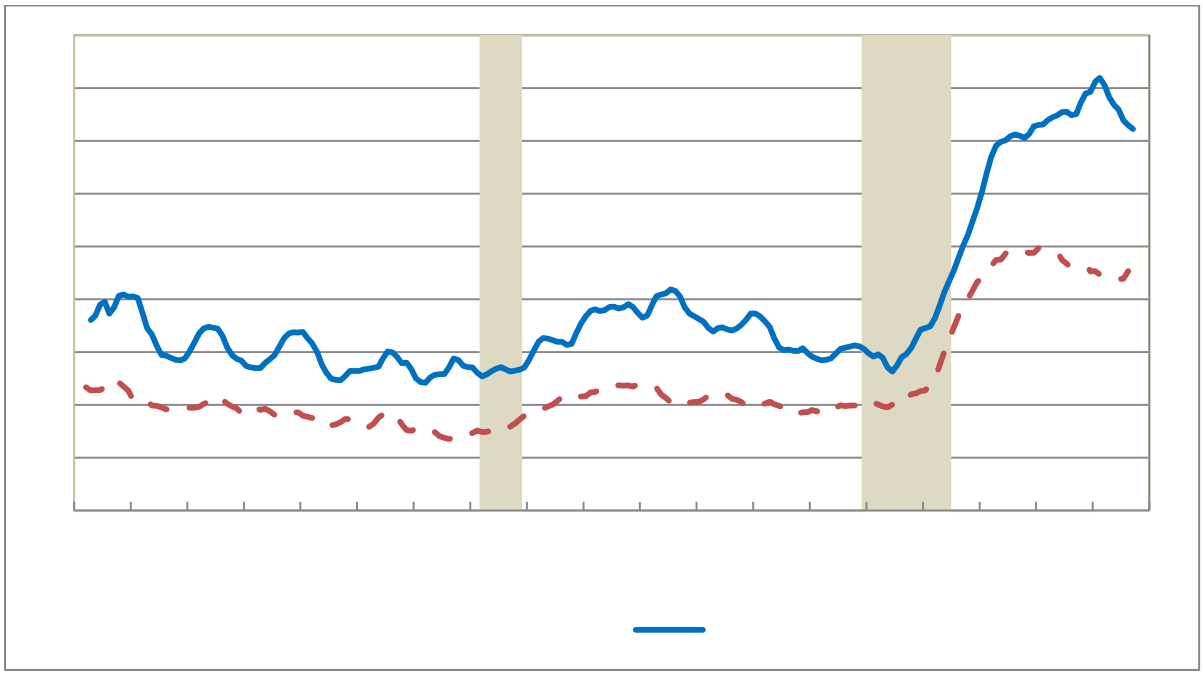
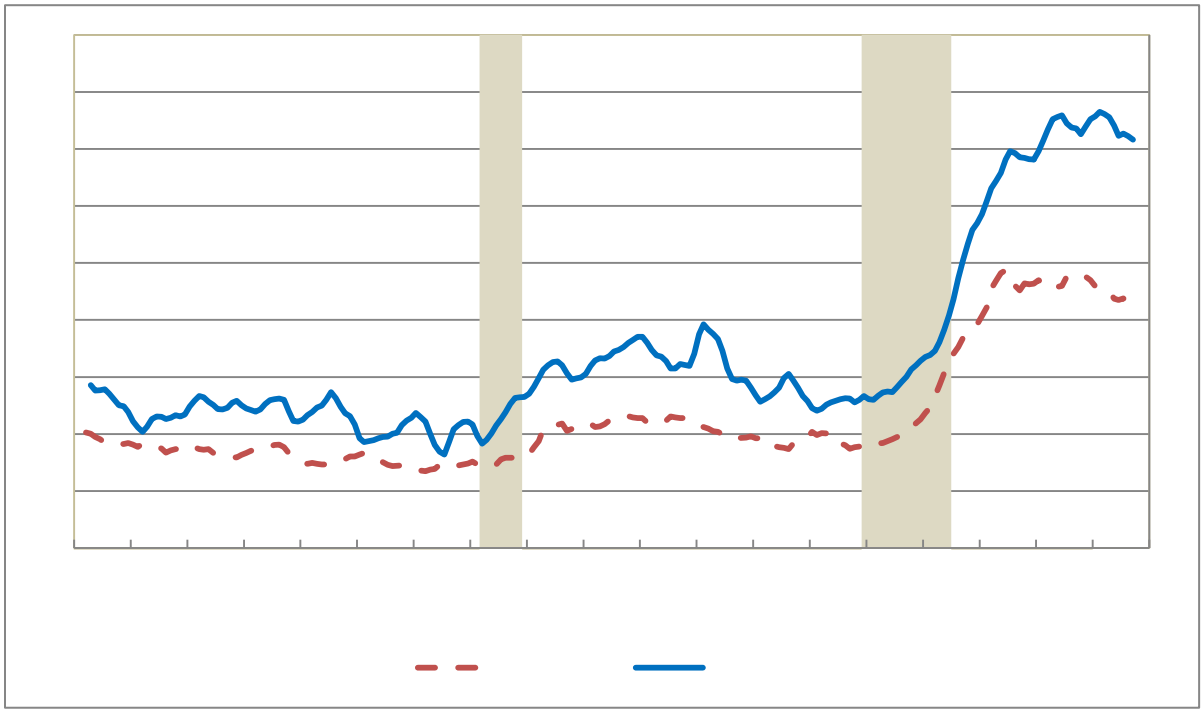
Figure 1: Median Unemployment Durations, in Weeks

A. Men

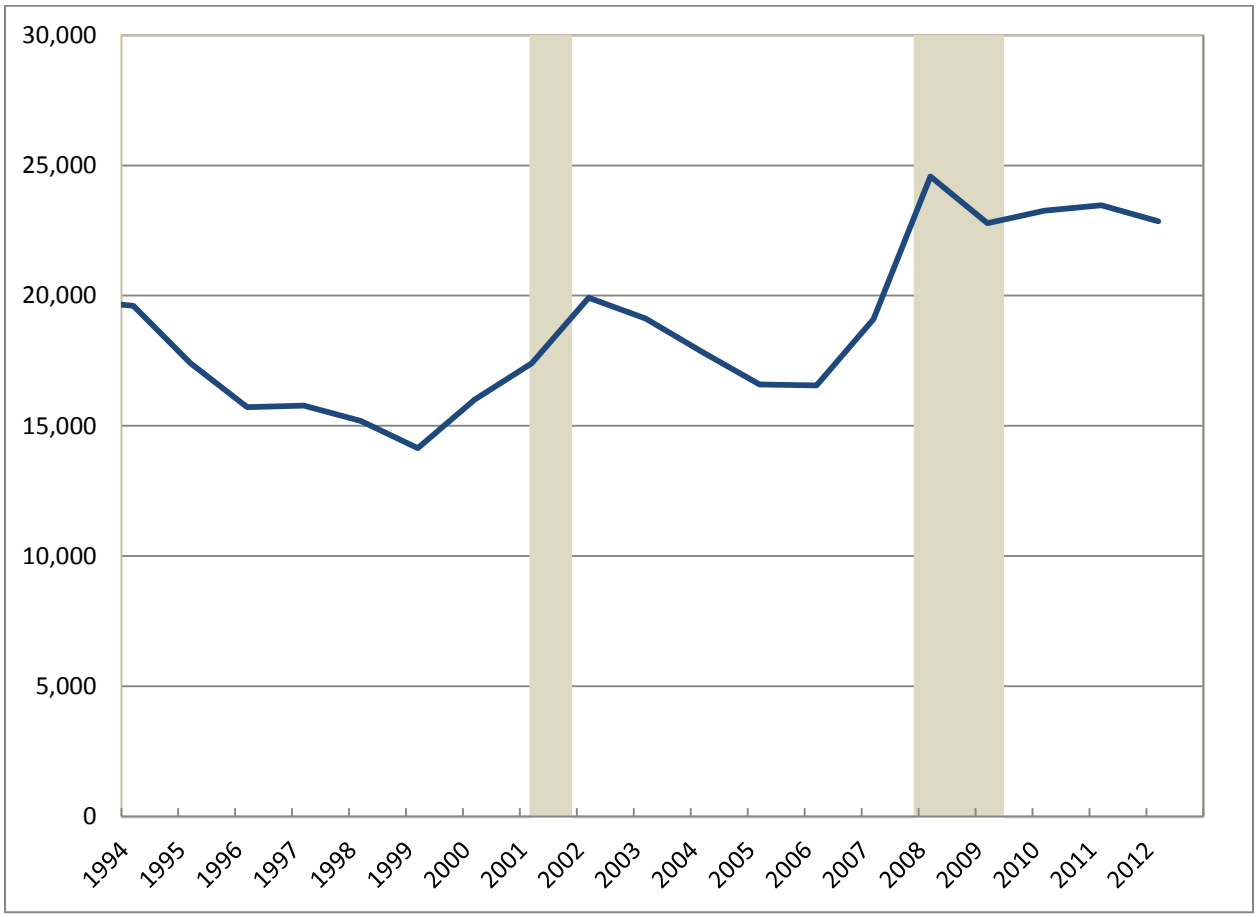


B. Women



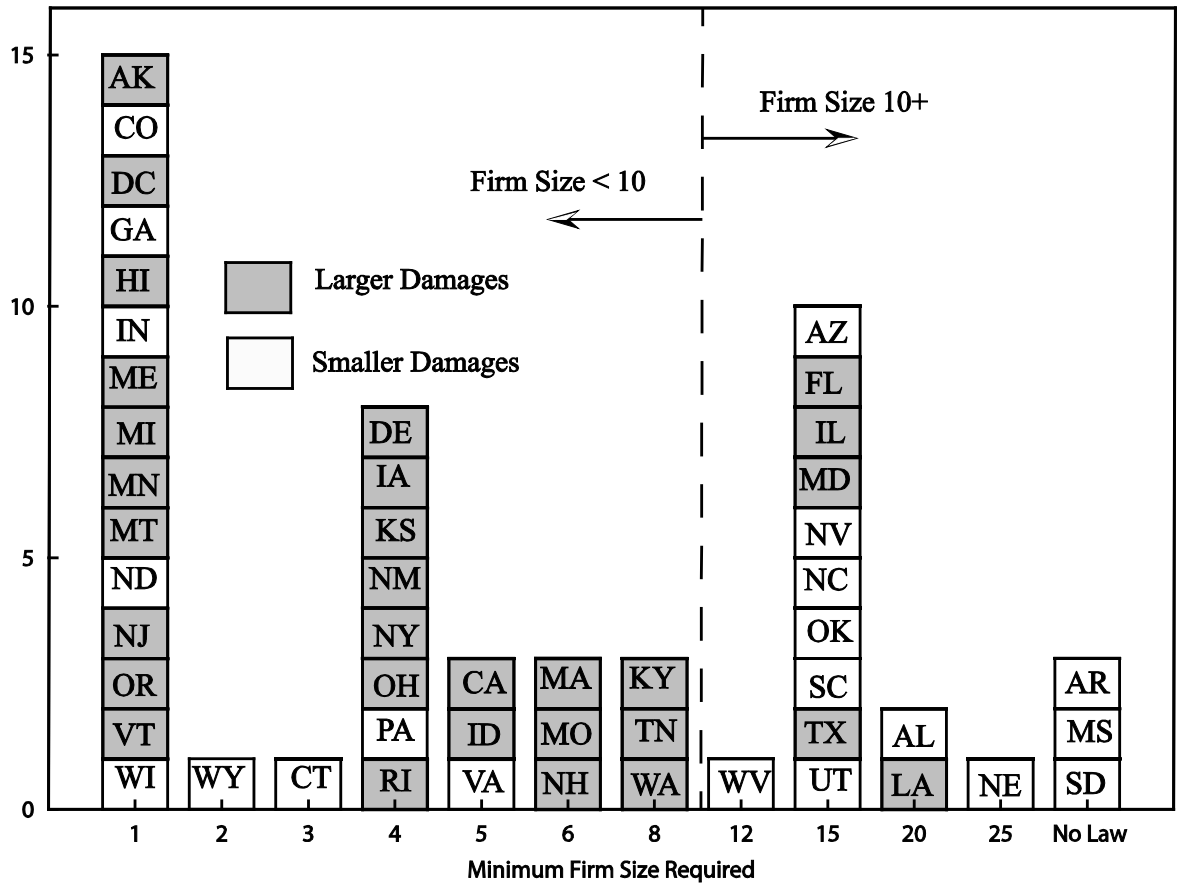
The shaded areas indicate official dates according to the NBER. Each series was generated using the Current Population Survey "Basic Monthly" micro-data. State estimates were calculated and weighted by state population to generate nationally representative estimates. Each series was seasonally adjusted and smoothed using X-12-ARIMA.

Figure 2: ADEA Claims Filed with the EEOC, Annual



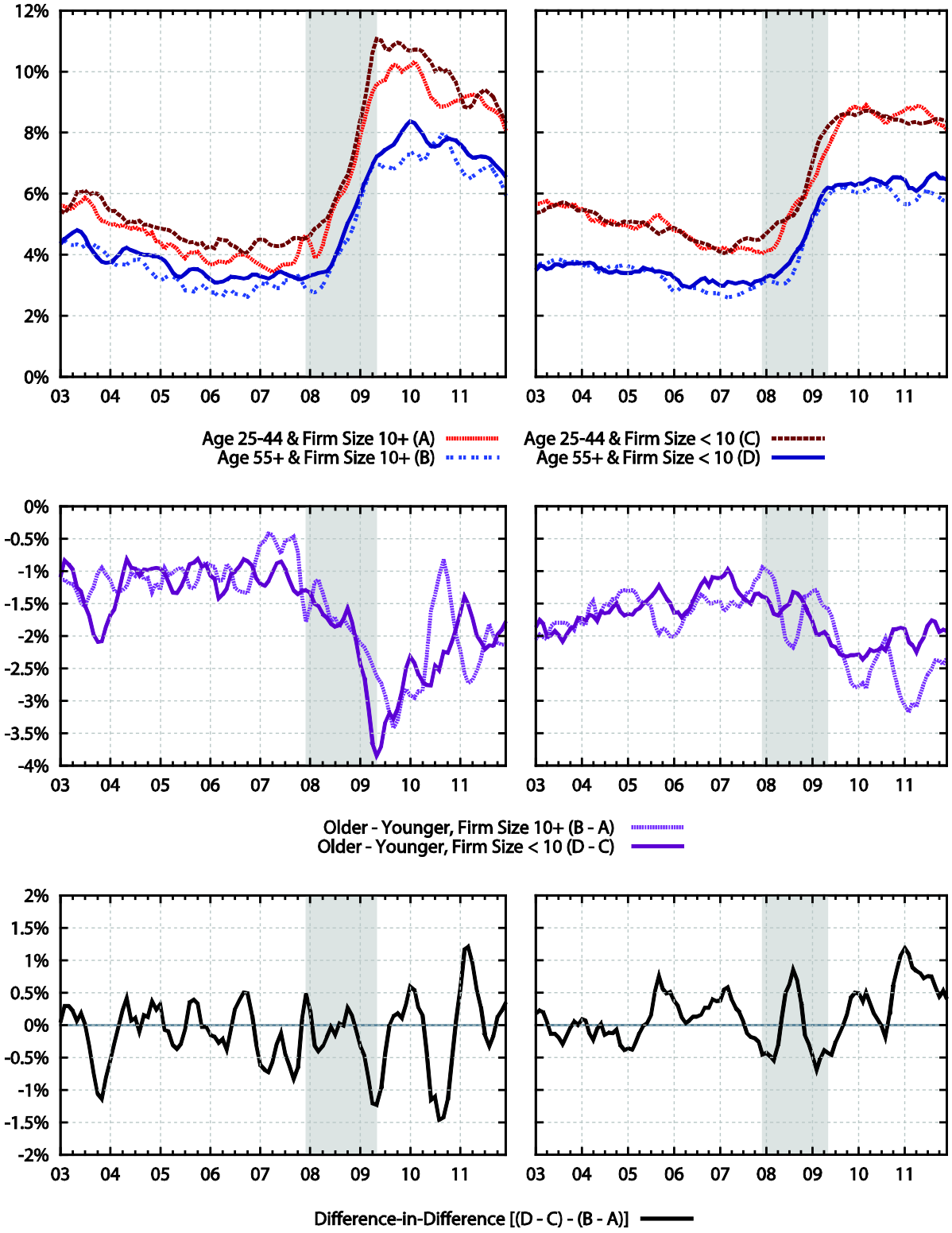
Shaded areas are recessions based NBER dates. Data are annual and based on the government fiscal year. They are assigned to March of each year, which is midway through the fiscal year. Source: http://eeoc.gov/eeoc/statistics/enforcement/adea.cfm (viewed November 23, 2013)

Figure 3: Minimum Firm Size Required and Potential Size of Damages under State Age Discrimination Laws



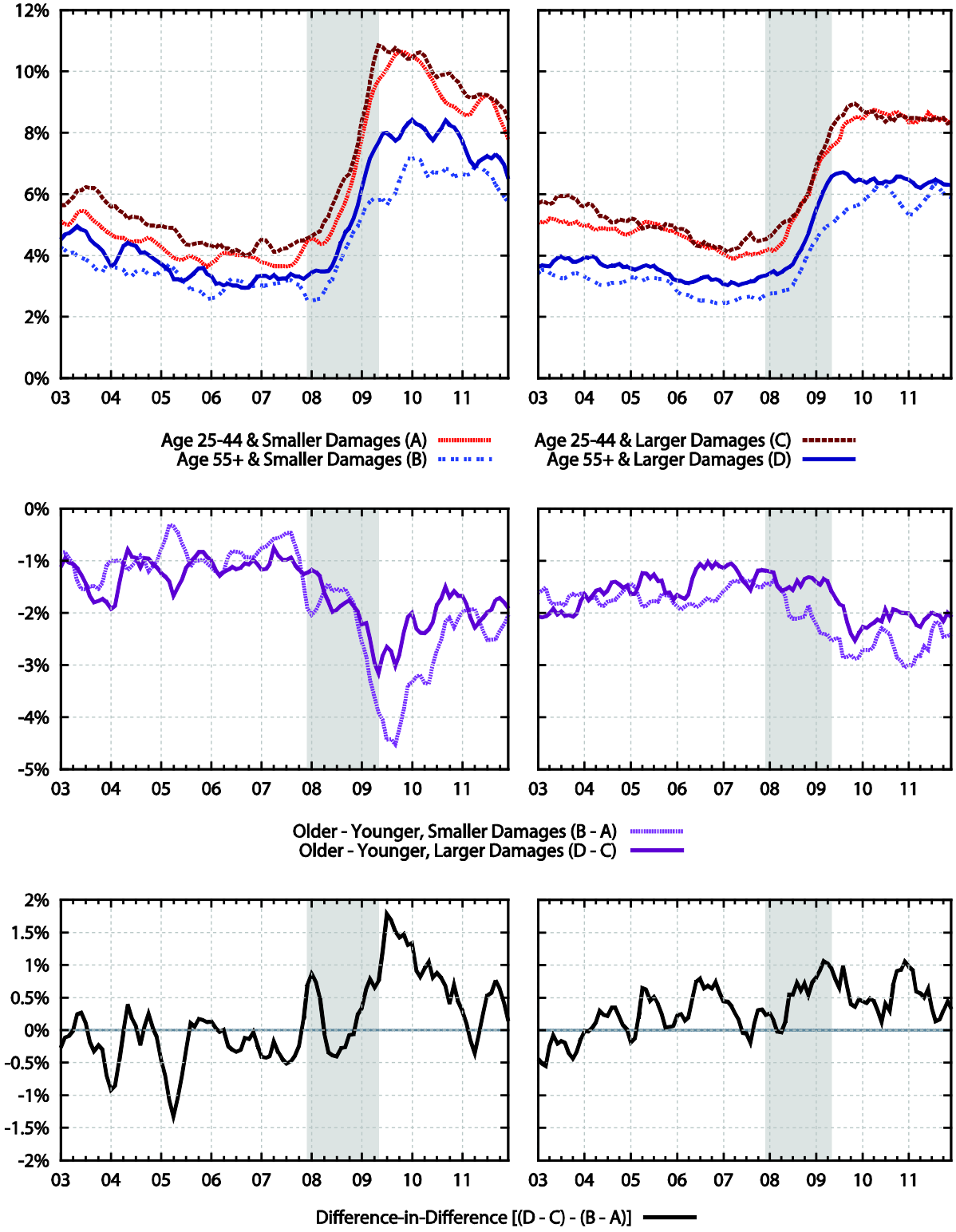
See Neumark and Song (forthcoming) for additional details on age discrimination laws by state.

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



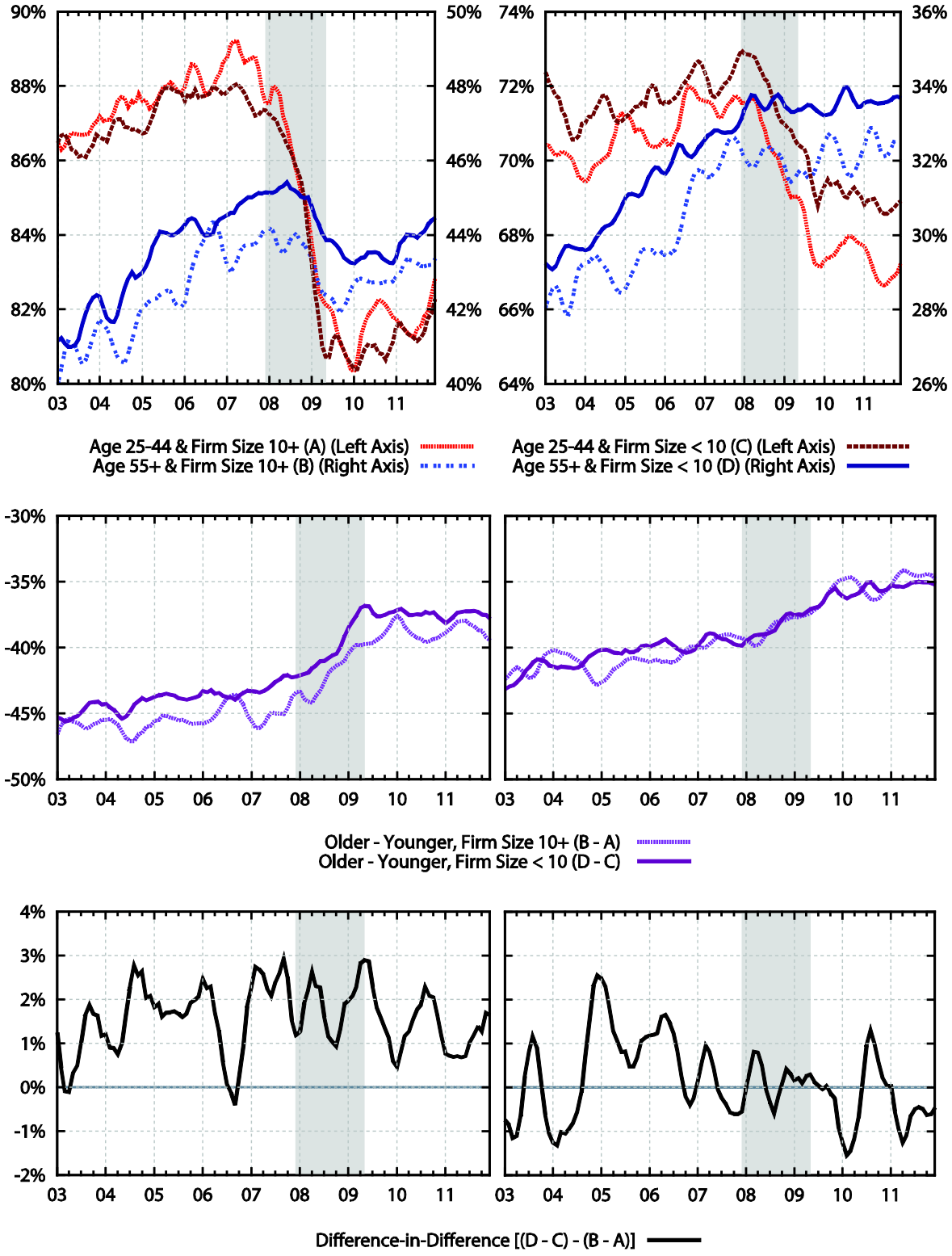
See the notes to Figure 1 above. States are divided into two groups based on the minimum firm size required for age discrimination laws to apply. See Figure 3 for additional details on age discrimination laws by state. Each series in the top figure is generated using the Current Population Survey "Basic Monthly" micro-data, using the provided population weights, and seasonally adjusted and smoothed using X-12-ARIMA.

Figure 5: Unemployment Rates by Age and Size of Damages, Men (Left) and Women (Right)



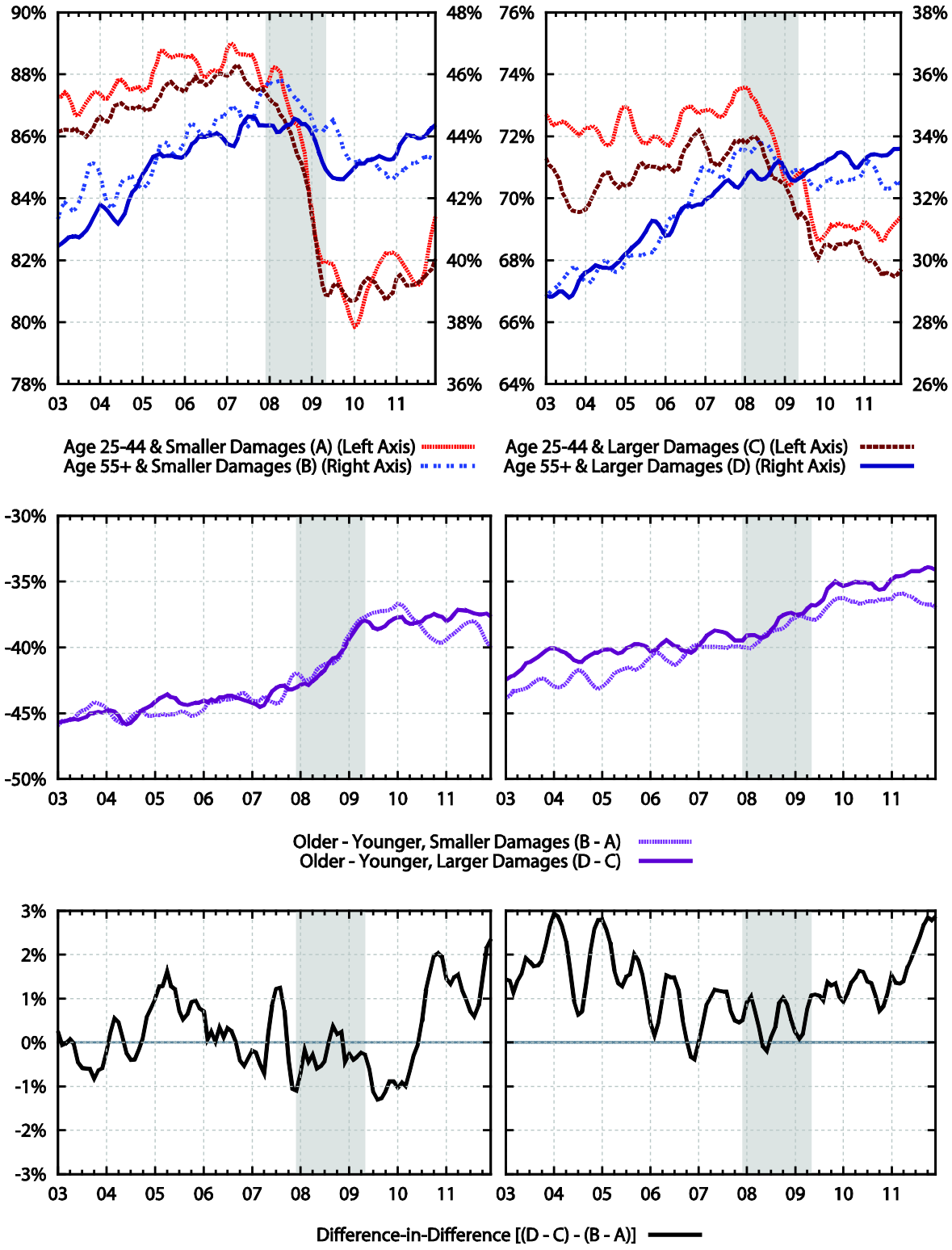
See notes to Figure 4. The only difference is that larger damages than the federal law are used instead of firm-size minimum.

Figure 6: Employment-to-Population by Age and Firm-Size Minimum, Men (Left) and Women (Right)



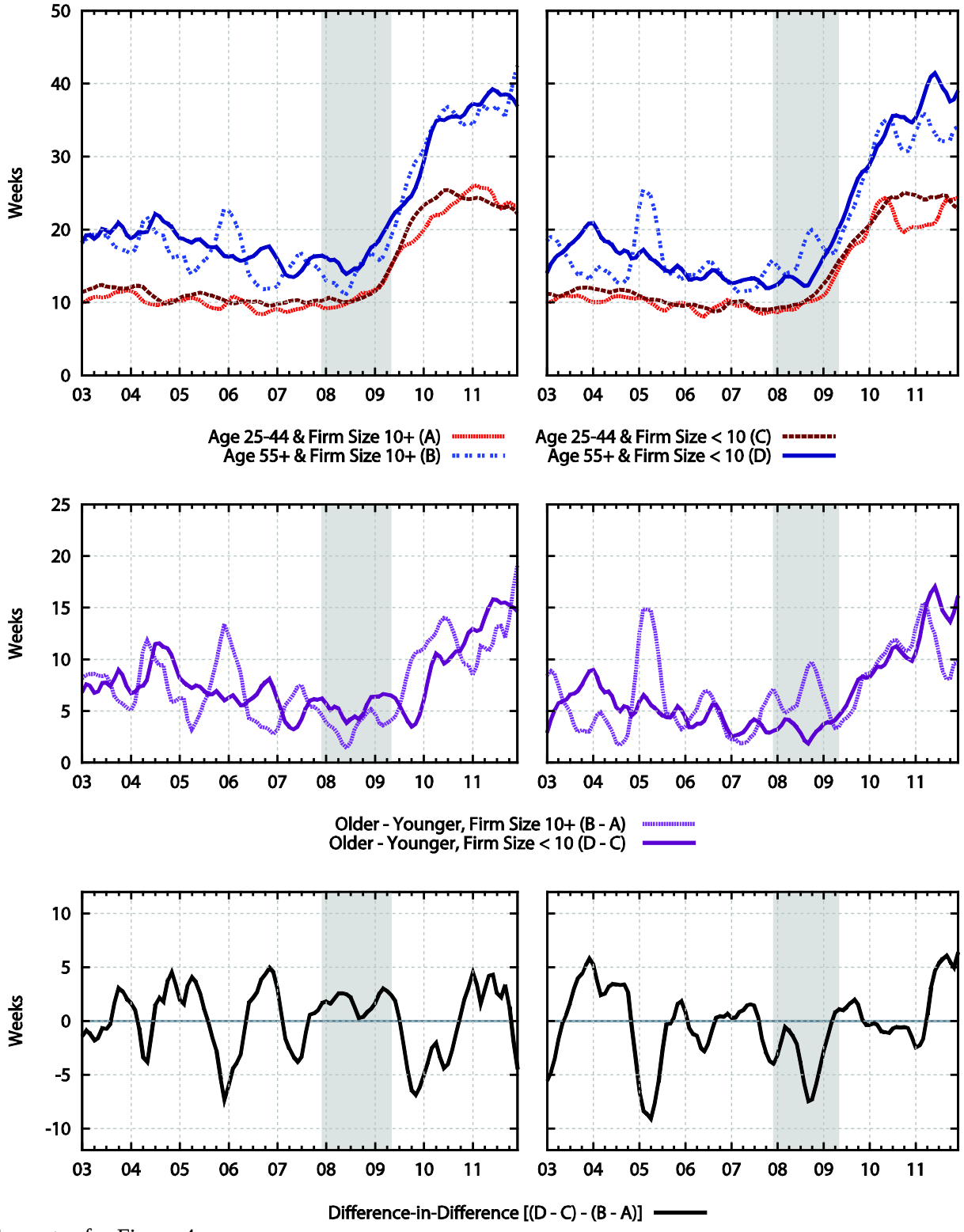
See notes to Figure 4.

Figure 7: Employment-to-Population by Age and Size of Damages, Men (Left) and Women (Right)



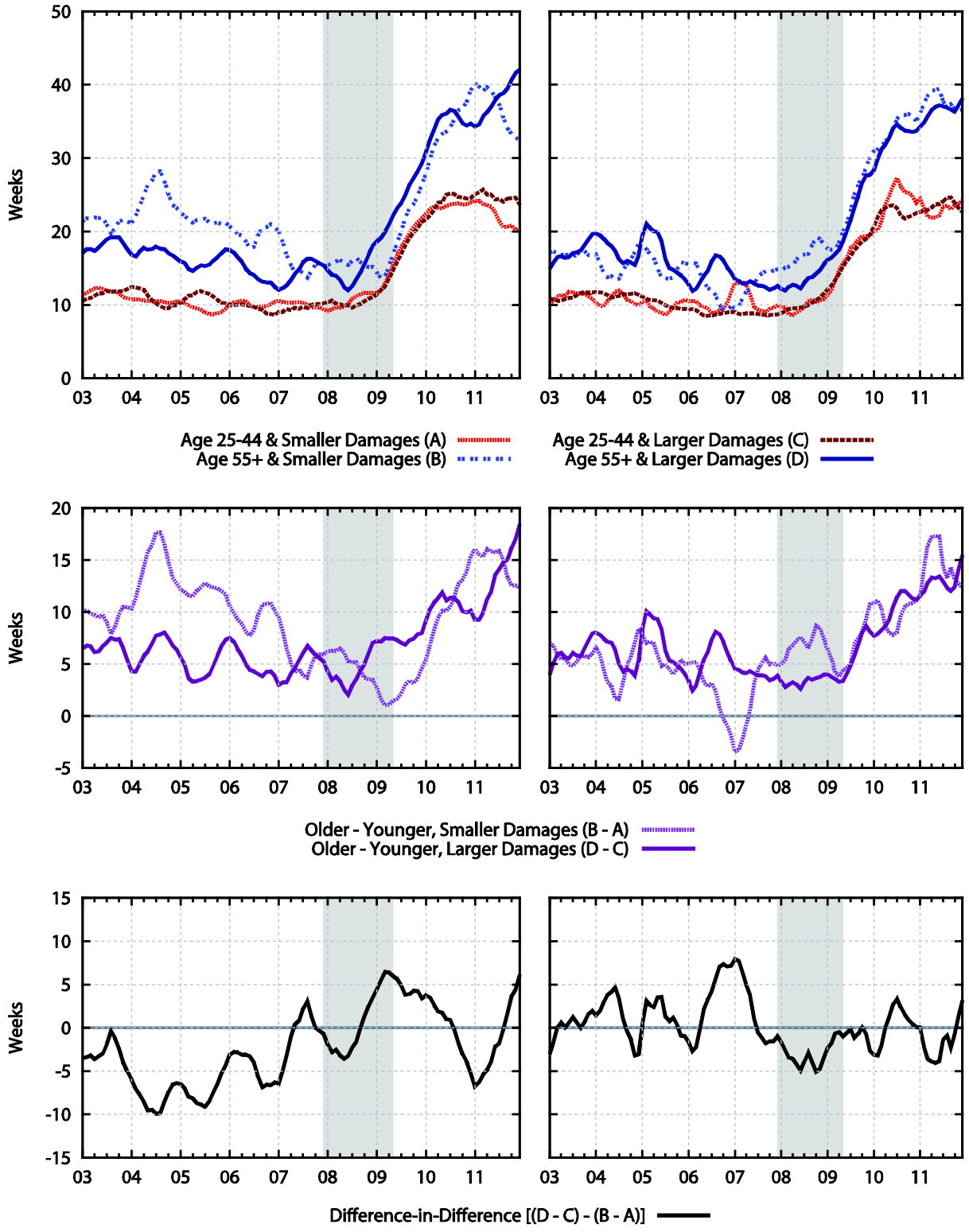
See notes to Figures 4 and 5.

Figure 8: Median Unemployment Duration by Age and Firm-Size Minimum,
Men (Left) and Women (Right)



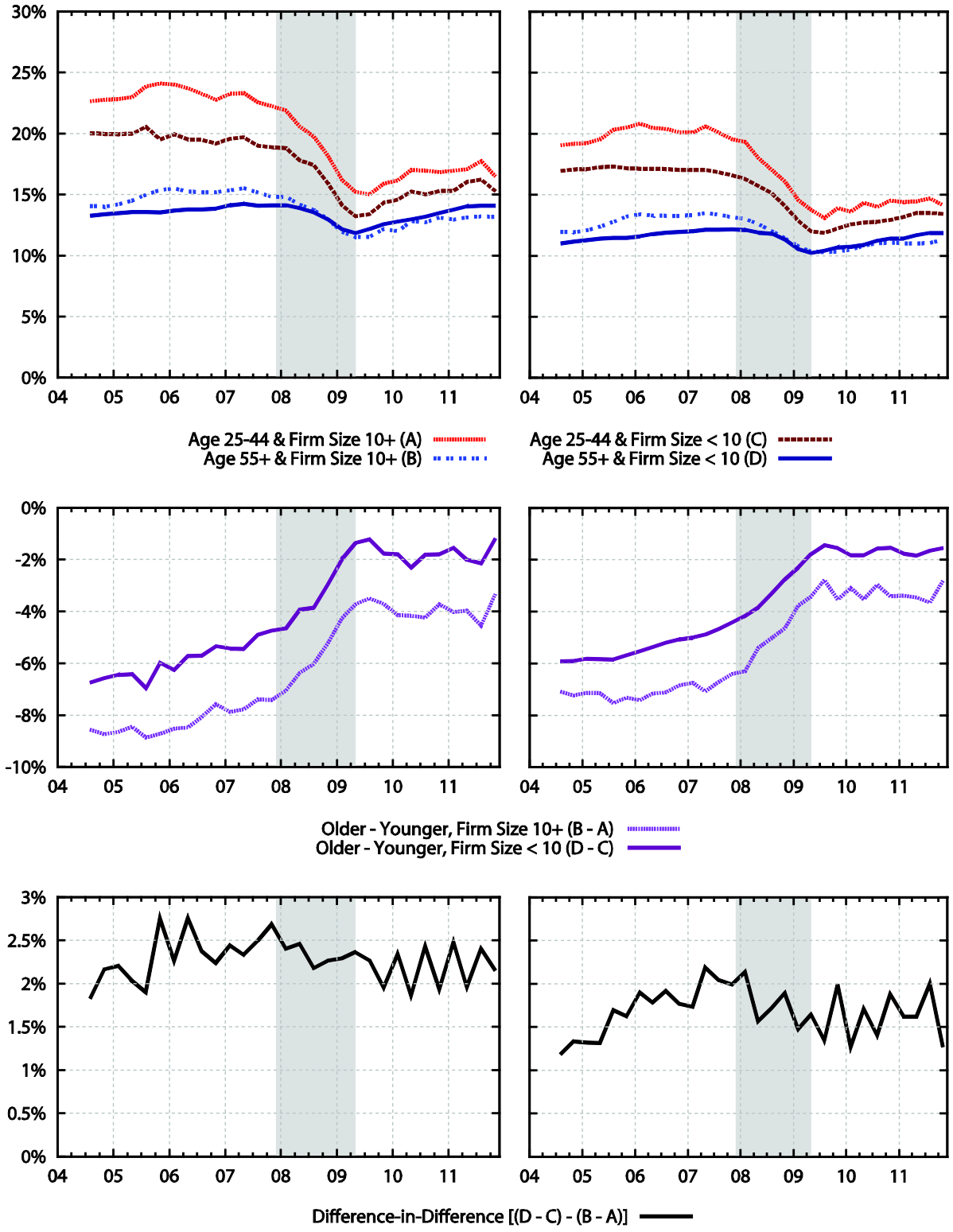
See notes for Figure 4.

Figure 9: Median Unemployment Duration by Age and Size of Damages, Men (Left) and Women (Right)



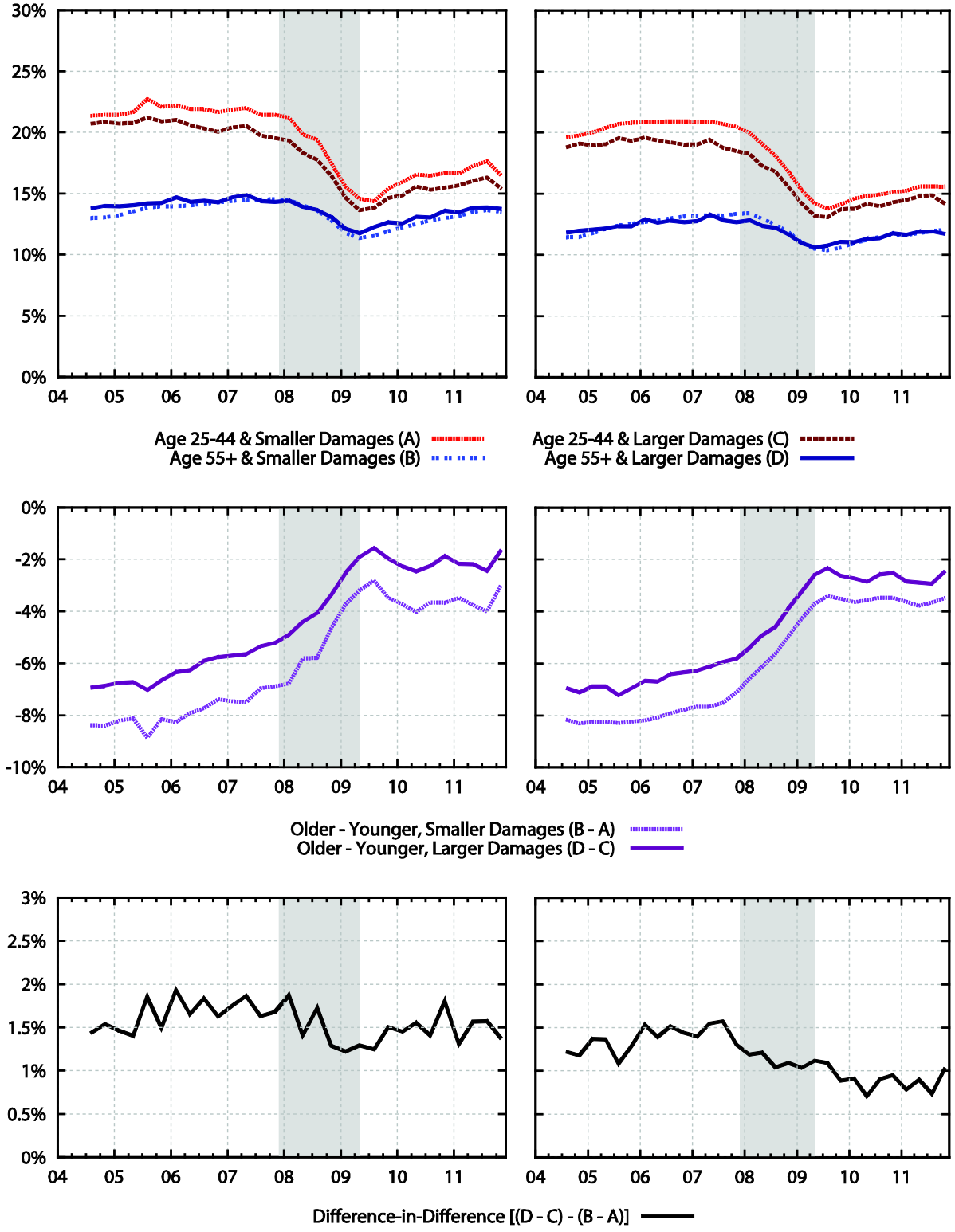
See notes to Figures 4 and 5.

Figure 10: Hiring Rates by Age and Firm-Size Minimum, Men (Left) and Women (Right)



See the notes to Figure 4. States are divided into two groups based on the minimum firm size required for age discrimination laws to apply. Each series in the top figure 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. For most of the hiring series, X-12-ARIMA smooths using the two nearest quarters, resulting in 2004:Q2 being missing. However, when the data are smoother it can use fewer quarters to smooth, so in some cases data for 2004:Q2 are available.

Figure 11: Hiring Rates by Age and Size of Damages, Men (Left) and Women (Right)



See notes to Figure 4. The only difference is that larger damages than under the federal law are used instead of smaller firm-size minimum.

Table 1: Summary Statistics for CPS Data

	Younger (25-44) Men				Younger (25-44) Women			
	Unweighted	Weighted	Min	Max	Unweighted	Weighted	Min	Max
Unemployment rate	6.1	6.5	0	21.1	5.6	6.1	0	19.2
	(3.1)	(3.1)			(2.5)	(2.5)		
Employment-to-population ratio	85.8	85.2	67.9	97.1	72.4	70.5	54.7	87.9
	(4.5)	(4.2)			(5.3)	(4.6)		
Median unemployment duration	13.6	14.2	0	104	13.0	13.8	1	64
	(9.7)	(9.2)			(9.1)	(8.8)		
	Older (55+) Men				Older (55+) Women			
	Unweighted	Weighted	Min	Max	Unweighted	Weighted	Min	Max
Unemployment rate	4.4	4.9	0	24.1	3.9	4.3	0	18.3
	(2.8)	(2.8)			(2.5)	(2.4)		
Employment-to-population ratio	44.2	43.3	23.4	62.4	33.0	31.8	17.4	50.5
	(5.7)	(4.6)			(5.0)	(3.8)		
Median unemployment duration	21.5	22.0	0	119	19.8	20.5	0	119
	(21.1)	(18.9)			(21.4)	(19.4)		
	Controls				Controls			
Compositional control	0.009	0.007	-1.7	1.4	-0.002	-0.002	-2.0	1.7
	(0.379)	(0.348)			(0.386)	(0.395)		
Extra UI weeks available	19.4	21.1	0	73	19.4	21.1	0	73
	(25.5)	(27.4)			(25.5)	(27.4)		

Standard deviations are in parentheses. These statistics were generated for each state and month from 2003 to 2011 using the Current Population Survey monthly micro-data, weighting by population. There are 5,508 observations for each age group and sex. The weighted estimates use state population estimates generated from the CPS. These 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 zeros. Data are not seasonally adjusted.

Table 2: Summary Statistics for QWI Data, Hires Relative to 2005 Employment (%)

	Younger (25-44) Men				Younger (25-44) Women			
	Unweighted	Weighted	Min	Max	Unweighted	Weighted	Min	Max
Hires	19.3	18.5	8.7	40.7	17.6	17.3	8.3	37.3
	(4.9)	(4.2)			(4.2)	(3.9)		
	Older (55+) Men				Older (55+) Women			
	Unweighted	Weighted	Min	Max	Unweighted	Weighted	Min	Max
Hires	13.8	13.6	6.6	33.4	11.9	12.1	6.1	27.4
	(3.6)	(3.0)			(3.2)	(2.9)		

Standard deviations are in parentheses. These statistics were generated for each state, quarter, and age group from 2004:Q2 to 2011:Q4 using the Quarterly Workforce Indicators. There are 1,519 observations for each age group and sex. The weighted estimates use Census-based population estimates to weight each state observation by state population. 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

Standard errors, clustered at the state level, are in parentheses. *, **, and *** mean statistically significant from zero at the 10%, 5%, and 1% levels, respectively. The sample period is 2003-2011. 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 zero to 100. For both 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 do 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)**
OLD	-45.47***	…	-40.86***	…	-44.58***	…	-41.60***	…
	(1.28)		(1.18)		(0.77)		(0.66)	
LAW	-0.52	…	0.94	…	-0.76	…	-1.52	…
	(0.58)		(1.42)		(0.58)		(1.20)	
GR	-2.24***	…	-0.29	…	-2.26***	…	-0.31	…
	(0.66)		(0.35)		(0.45)		(0.39)	
AfterGR	-6.18***	…	-3.34***	…	-6.47***	…	-3.26***	…
	(0.94)		(0.30)		(0.44)		(0.47)	
GR × OLD	3.47***	…	2.39***	…	4.00***	…	2.90***	…
	(1.04)		(0.49)		(0.67)		(0.42)	
AfterGR × OLD	6.67***	…	5.62***	…	6.33***	…	5.05***	…
	(0.96)		(0.45)		(0.57)		(0.57)	
GR × LAW	0.01	…	0.44	…	0.04	…	0.43	…
	(0.70)		(0.44)		(0.58)		(0.47)	
AfterGR × LAW	0.02	…	0.81*	…	0.43	…	0.60	…
	(1.02)		(0.43)		(0.73)		(0.54)	
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

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)**
OLD	6.91***	…	5.53***	…	10.30***	…	4.11***	…
	(0.95)		(1.28)		(0.52)		(1.11)	
LAW	0.83	…	0.63	…	0.15	…	-0.42	…
	(0.62)		(0.59)		(0.56)		(0.49)	
GR	0.56	…	0.45	…	0.58	…	0.24	…
	(0.71)		(0.42)		(0.39)		(0.54)	
AfterGR	12.67***	…	11.34***	…	11.91***	…	11.86***	…
	(2.21)		(1.56)		(0.82)		(1.17)	
GR × *OLD*	-3.36***	…	0.22	…	-6.18***	…	1.62	…
	(1.22)		(1.54)		(1.50)		(1.94)	
AfterGR × *OLD*	4.25***	…	4.64***	…	-0.03	…	7.56***	…
	(1.29)		(1.13)		(1.49)		(1.61)	
GR × *LAW*	-0.33	…	0.09	…	-0.34	…	0.39	…
	(0.83)		(0.56)		(0.64)		(0.63)	
AfterGR × *LAW*	-0.21	…	1.00	…	0.90	…	0.15	…
	(2.35)		(1.74)		(1.56)		(1.54)	
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

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

Standard errors, clustered at the state level, are in parentheses. *, **, and *** mean statistically significant from zero at the 10%, 5%, and 1% 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 both 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 do 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: Robustness of Key Results

	Men, Larger Damages, Unemp. Rate	Women, Lower Firm-Size Minimum, Empl.-to-Pop.	Men, Larger Damages, Unemp. Durations	Women Larger Damages, Unemp. Durations	Women Larger Damages, Hires
	(1)	(2)	(3)	(4)	(5)
(a) Original Results (even-numbered columns in Tables 3-6)					
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 ×	1.03***	-1.98**	5.04*	-3.13	-1.07**
OLD × LAW	(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 ×	1.03**	-0.98	5.10**	-2.93	-0.41
OLD × LAW	(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 ×	…	…	5.39**	-2.57	…
OLD × LAW			(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 ×	0.55	-1.30	2.10	-3.81	-0.40
OLD × LAW	(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 ×	0.87*	-1.47	6.14*	-1.43	-0.89
OLD × LAW	(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 ×	1.28***	-2.37**	5.75*	-4.85	-0.85**
OLD × LAW	(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 ×	1.10**	-1.71	6.70*	-2.85	-0.67*
OLD × LAW	(0.50)	(1.16)	(3.75)	(3.28)	(0.35)

Estimates are based on the specification used in the even-numbered columns in Table 3-6. We include only the outcome variables and the law characteristics combinations that led to statistically significant results in Tables 3-6. 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 8: Falsification Test

	Men, Larger Damages, Unemp. Rate	Women, Lower Firm-Size Minimum, Empl.-to-Pop.	Men, Larger Damages, Unemp. Durations	Women Larger Damages, Unemp. Durations	Women Larger Damages, Hires
	(1)	(2)	(3)	(4)	(5)
(a) Original Results (even-numbered columns in Tables 3-6)					
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 ×	1.03***	-1.98**	5.04*	-3.13	-1.07**
OLD × *LAW*	(0.38)	(0.81)	(2.52)	(2.89)	(0.52)
(b) Falsification: 2002-2007					
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)	

See notes to Table 7. Estimates are based on the specification used in the even-numbered columns in Table 3-6. In panel (b), we define 2002-2004 as the "pre-recession" period and 2005-2007 as the "recession" period. Since many states are missing from the QWI before 2004, we cannot conduct this falsification test for hires.

Table 9: 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 ×	Mean	0.99	-1.45	**5.23**	-3.09	**-0.74**
OLD × LAW	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**

This table summarizes the results for the regressions for the even-numbered columns in Tables 3-6, the robustness analyses in Table 7, 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% 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. Shaded estimates are those that indicate the opposite. Column 3 and Column 5 contain boldfaced sets of estimates -- those where the main effect indicates that prior to the Great Recession stronger age discrimination protections helped older workers, but generated relatively worse outcomes for them during or after the Great Recession.

Appendix Table B1: DDD Regressions for 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

See notes to Table 6.