

Do Stronger Age Discrimination Laws Make Social Security Reforms More Effective?

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September 2011

Abstract

Supply-side Social Security reforms to increase employment and delay benefit claiming among older individuals may be frustrated by age discrimination. We test for policy complementarities between supply-side Social Security reforms and demand-side efforts to deter age discrimination, specifically studying whether stronger state-level age discrimination protections enhanced the impact of the increases in the Social Security Full Retirement Age (FRA) that occurred in the past decade. The evidence indicates that, for older individuals who were “caught” by the increase in the FRA, benefit claiming reductions and employment increases were sharper in states with stronger age discrimination protections.

*We are grateful to the Social Security Administration, through a grant to the Michigan Retirement Research Center (MRRC), and to the Borchard Foundation Center on Law & Aging, for financial support, and to Larry Jacobson, Joanna Lahey, and participants at the 2011 MRRC research conference for helpful comments. All conclusions and opinions are solely ours.

I. Introduction

In coming decades the share of the population aged 65 and over (“seniors”) will rise sharply – from 17 percent of those aged 20 and over in 2000, to 28 percent in 2050 (projected) – and will approach equality with the share aged 45-64 by the middle of the century (Neumark, 2008). This aging of the population will pose fundamental public policy challenges. Most significantly, the very low employment rate of seniors implies slowing labor force growth relative to population, and a rising dependency ratio. This creates an imperative to increase the employment of older individuals, thereby lowering dependency ratios, raising tax revenues, and – as programs are currently structured – decreasing public expenditures on health insurance, retirement benefits, and income support.

Population aging and the need to increase employment of seniors are most strongly tied to the solvency of Social Security, leading to numerous reforms intended to increase the employment (or hours) of those who would otherwise retire, including: raising the full retirement age (FRA) from 65 to 67 beginning with the 1938 birth cohort which reached age 65 in 2003, with the FRA rising fairly quickly to 66 for the 1943-1954 birth cohorts (American Academy of Actuaries, 2002; Munnell et al., 2004); and changes in taxation of benefits including reductions in the marginal tax rate on earnings of Social Security recipients in excess of the earnings cap, increases in the exempt amount of earnings (the cap), and broadening of the ages not subject to the earnings test (Friedberg, 2000). Additional changes are likely to be considered as part of efforts to shore up the solvency of Social Security or to reform the system.

The need to delay Social Security claiming and retirement of older workers, however, may be frustrated by age discrimination. Although the federal Age Discrimination in Employment Act (ADEA) and state age discrimination laws have helped reduce age discrimination in terminations, evidence suggests that age discrimination remains pervasive, especially with regard to hiring older workers (e.g., Adams, 2002, 2004; Bendick et al., 1996, 1999; Hirsch et al., 2000; Hutchens, 1988; Johnson and Neumark, 1997; Kite et al., 2005; Lahey, 2008a).¹ Even though research

¹ The evidence is not cut and dried, however. The following section of the paper reviews this

suggests that age discrimination laws have been effective at increasing employment of protected workers, (Neumark and Stock, 1999; Adams, 2004), the ADEA may be less effective at combating hiring discrimination because in hiring cases it is difficult to identify a class of affected workers, and economic damages are smaller than in termination cases (Adams, 2004; Bloch, 1994; Lahey, 2008b; Posner, 1995). This implies that age discrimination may pose particular challenges to efforts to keep workers who might otherwise retire (“seniors”) employed, because increased employment among seniors would likely come largely from new employment in part-time or shorter-term “partial retirement” or “bridge jobs,” rather than from continued employment of workers in their long-term career jobs (e.g., Cahill et al., 2006; Johnson et al., 2009).

If age discrimination deters the employment of older workers, especially beyond what has until recently been the “normal” retirement age of 65, then supply-side incentives – via changes to Social Security as well as other policies – may be rendered less effective or ineffective. A key policy question, then, is whether there are policy complementarities between supply-side efforts to increase labor supply and demand-side efforts to deter age discrimination. In this paper, we consider the specific question of whether stronger age discrimination protections at the *state* level enhanced the impact – in terms of reducing retirement and encouraging employment – from the increases in Social Security’s FRA that occurred in the past decade. We focus on state-level age discrimination laws because, during the period when the FRA began to increase, there were no changes in federal age discrimination law that could be used as identifying variation. Other research – on both age discrimination and discrimination along other dimensions – has found that state discrimination laws can have important effects on labor market outcomes.²

II. Research on Age Discrimination and Age Discrimination Laws

Age Discrimination

Economic research on discrimination – whether on the basis of age or other characteristics – has been and remains controversial. Nonetheless, there is plenty of

evidence fully, and discusses some of its limitations.

² Research on age discrimination is discussed in the next section. Papers looking at state race or sex discrimination laws include Chay (1998) and Neumark and Stock (2006).

evidence consistent with age discrimination, both before and after the ADEA. Although this evidence does not decisively rule out alternative interpretations, it is sufficiently compelling that we have to view age discrimination as plausibly continuing to exert an adverse influence on labor market outcomes for older workers. This, in turn, motivates consideration of the role of laws protecting older workers from discrimination in removing barriers to increasing the employment of seniors.

In the Becker (1957) model, a group suffers from discrimination if employers, other workers, or consumers dislike interactions with members of the group, which is reflected in market transactions. In this model, discrimination stems from “animus.” An alternative that may be more relevant to the case of older workers, yet have similar observable consequences, is that employers hold negative stereotypes about the abilities of older individuals, which may be incorrect.

Finally, much of the economics literature on age discrimination is framed in terms of the Lazear (1979) long-term incentive contract (LTIC) model, in which employers pay young, low-tenured workers less than their marginal product, and older, high-tenured workers more than their marginal product, to create incentives for workers to work hard and avoid losing their jobs. The LTIC model provides potential explanations for differential treatment of older (more-tenured) workers, along a number of dimensions. First, the model suggests an explanation of mandatory retirement, because older workers have wages in excess of the marginal value of their leisure at the date at which, over their career with the firm, the discounted stream of wage payments catches up to the discounted stream of marginal productivity – a date at which, *ex ante*, workers are willing to accept mandatory retirement (Lazear, 1979). Second, LTICs may deter hiring of older workers, because these contracts likely impose fixed costs that can be amortized only over a shorter period for older workers (Hutchens, 1986). (Barriers to paying newly-hired older workers much lower wages than current older workers can lead to the same result.) And third, LTICs provide incentives for employers to renege on the implicit contract, discharging workers unfairly (i.e., not for “shirking”) before their retirement date, so as to pocket some of the difference between a worker’s productivity and compensation to that point. Whether the differential treatment of workers based on

age implied by this model represents discrimination may be a semantic issue; however, it has been interpreted as such from a legal perspective (Issacharoff and Harris, 1997) – including, one might argue, the prohibition of mandatory retirement by the ADEA – as well as in the economics literature (e.g., Gottschalk, 1982; Cornwell et al., 1991).

Evidence from the pre-ADEA period (Miller, 1966) indicated that older workers who lost their jobs had a more difficult time finding new jobs than did “prime-age” workers, with longer durations of unemployment. In addition, survey evidence of hiring practices found that workers over age 45 were 25 percent of the unemployed, but only 8.6 percent of new hires. Shapiro and Sandell (1985) provided additional evidence of re-employment difficulties for displaced older workers, using data from the National Longitudinal Survey of Older Men (NLSOM). Finally, there was extensive evidence of explicit age restrictions in hiring. For example, in five cities in states without anti-age discrimination statutes, nearly 60 percent of employers imposed upper age limits (usually between ages 45 and 55) on new hires (U.S. Department of Labor, 1965).

The upper age limits for hires may have been due to negative stereotypes. Rosen and Jerdee (1977) provided evidence that managers perceived older workers as less flexible and more resistant to change, and suggested that these attitudes likely had real impacts in denying older workers opportunity, although their evidence is based only on managerial responses to hypothetical scenarios. Subsequent research corroborated the importance of negative stereotypes about older workers with potentially adverse effects on their labor market outcomes (e.g., Finkelstein et al., 1995; Kite et al., 2005).

The enactment of the ADEA has surely resulted in the elimination of explicit upper age limits for jobs. However, older workers are still considerably more likely than younger workers to have long unemployment durations (U.S. Department of Labor, Bureau of Labor Statistics, 2007). Of course, as in the earlier period, longer durations do not necessarily reflect discrimination against older workers. Moreover, unemployment *rates* of older individuals are lower than those of other age groups, although some older individuals may not show up as unemployed because they face poor job prospects and therefore simply decide to retire.

Research on age discrimination and its consequences based on self-reported data

suggests that employers may discriminate against older workers and that this discrimination has adverse consequences. Using NLSOM data, Johnson and Neumark (1997) study responses to the question “During the past five years, do you feel that so far as work is concerned, you were discriminated against because of your age?” To avoid the effects of unobserved individual differences in the propensity to report discrimination that might be correlated with labor market behavior, they focus on individuals who switch from reporting no age discrimination to reporting age discrimination. And to account for adverse changes in job characteristics or experiences on the job that might cause a worker to begin reporting age discrimination, they control for job satisfaction. Among those with jobs, for whom the question is asked, 7 percent of older men reported age discrimination; this would not include discrimination in hiring experienced by the non-employed. Moreover, Johnson and Neumark find that workers who start to report age discrimination are more likely to separate from their employer and less likely to be employed subsequently, and that those who separate for this reason suffer a wage loss of 10 percent. Adams (2002) studies self-reported age discrimination in the Health and Retirement Study (HRS). He finds that older workers reporting that their employer gives preference to younger workers in promotions have lower wage growth and a reduced expectation of working past the early or full Social Security retirement age. Again, because self-reports can reflect negative outcomes other than discrimination, Adams includes controls for the perceived work environment and fairness of pay.

Other research focuses explicitly on hiring, although typically those aged 65 and over are excluded. Based on Current Population Survey (CPS) data, Hutchens (1988) shows that newly-hired older workers were clustered in a smaller set of industries and occupations than were either newly-hired younger workers or older workers in general, and he suggests that this clustering reflected hiring discrimination. Hirsch et al. (2000) present similar results (by occupation) for more recent data. They report only slight improvement over time in the occupational segregation facing “new older hires,” and hence suggest that the problem did not diminish in the period they studied.

“Audit studies” and “correspondence studies,” while more commonly used to study race or sex discrimination in hiring (e.g., Fix and Struyk, 1993; Neumark, 1996;

Heckman, 1998; Riach and Rich, 2002), have also been used to study age discrimination. Two studies by Bendick and co-authors (1996, 1999) find evidence consistent with age discrimination against older workers. However, a fundamental problem with using this method to study age discrimination is that there is no natural way to make older and younger workers look identical in all respects other than age. One would expect older workers to have more experience than younger workers (overall, and in a particular industry or occupation), but if the information used in the study reflected this, then the extra experience might favor older workers. Consequently, the authors tried to hold human capital constant by giving the older and younger applicants (aged 32 and 57) the same number of years (10) in the occupation for which they were applying, with the older applicants indicating that they had been out of the labor force raising children, working as a high school teacher, or in the military, depending on the job opening. However, this solution could bias the results in the opposite direction, with the fictitious history negatively affecting the employer's assessment of the older applicants, as perhaps suggesting that interests lie elsewhere, work is not a priority, etc., generating spurious evidence of discrimination.

To try to address this problem, Lahey (2008a) studies hiring of women, for whom, she suggests, time out of the labor force (even if only inferred by the employer) is less likely to be a negative signal of ability, motivation, etc., than for men. She also studies entry-level jobs, so that previous experience might be a bit less of an issue, and therefore includes only a 10-year job history. Lahey also finds evidence consistent with age discrimination, with older women (aged 50-62) significantly less likely to get a positive response or an interview than younger women (aged 35-45). Although Lahey's modifications are likely helpful, it is not clear that they fully solve the fundamental problem of making older and younger applicants look the same on paper. As a result of this problem, these studies should be viewed as providing at best suggestive evidence of age discrimination in hiring – much like the other evidence on age discrimination.

Research has also considered evidence on behavior emanating from the LTIC model that is more explicitly tied to age discrimination – most notably the hypothesis that LTICs create incentives for employers to renege on long-term implicit commitments to

workers.³ Although Lazear (1979) suggested that reputation effects should deter this opportunistic behavior, reputation effects require strong conditions to work. For example, Neumark and Stock (1999) suggest that information asymmetries between workers and firms allow employers to claim that layoffs of older workers are due to changed economic conditions, which workers cannot fully verify. In addition, institutional innovations may arise that allow employers to “circumvent” damages to reputation stemming from opportunistic behavior. For example, abrogations of LTICs can occur following hostile takeovers, because when the company is subsequently resold the new owner suffers no loss of reputation (Shleifer and Summers, 1988; Gokhale et al., 1995). Indeed, there is evidence suggesting that firms renege on LTICs with older workers.⁴ And looking at hiring in the context of the LTIC model, Hutchens (1986) constructs an index for industry-occupation pairs measuring the hiring of older workers relative to the employment of older workers, so that lower values of the index indicate jobs that tend to employ but not hire older workers. He finds that the index is negatively related to job characteristics associated with LTICs, such as pensions and mandatory retirement, indicating that in such jobs hiring of older workers is suppressed.

Age Discrimination Laws

Neumark and Stock (1999) studied the effects of both the federal ADEA and

³ Of course a precursor to this research is whether the model is, more generally, applicable to understanding the labor market for older workers – in particular the age-earnings (and age-productivity) profile, from which the other implications issue. Although there is evidence pointing to other explanations of the age-earnings profile – in particular, the human capital model (Neumark and Taubman, 1995) and the forced-saving model (Neumark, 1995) – there is also a good deal of evidence that is most consistent with the LTIC model. Using data on earnings and productivity in manufacturing establishments, Hellerstein and Neumark (2007) find that wage profiles are steeper than productivity profiles, consistent with the LTIC model. Evidence on earnings profiles from firm-level data is also consistent with this model (Kotlikoff and Gokhale, 1992). Of course elements of each model may partially account for the age-earnings profile. The key point is that there is evidence consistent with wages being set in part according to the LTIC model, with wages higher than marginal product for older workers; this can create incentives for employers to treat older workers adversely.

⁴ Consistent with the argument regarding hostile takeovers, Gokhale et al. (1995) find that such takeovers are associated with reductions in employment of more senior workers, particularly where older workers earn relatively high wages. Other research finds evidence that hostile takeovers lead to terminations and reversions of pensions, interpreted as breaches of implicit contracts with long-term workers (Pontiff et al., 1999; Ippolito and James, 1992; Mitchell and Mulherin, 1989; Petersen, 1992).

state laws barring age discrimination. Using Decennial Census data covering 1940-1980, for white men, they find that age discrimination laws boosted employment rates of the entire group of protected workers, but only slightly. However, the employment rates of protected workers aged 60 and over were increased substantially (by about 6 percentage points).⁵ Adams (2004) uses a similar research design, focusing on the mid-1960s when a number of states passed age discrimination statutes, and then the federal legislation took effect. He uses CPS data, and looks at a richer set of outcomes. Adams finds somewhat larger overall employment effects for protected workers, with an increase of 2.75 percentage points in their employment rate. When he focuses on either those aged 60 and older or 65 and older, he finds effects of around 3.6 to 4.1 percentage points. Using the same strategy, he finds that age discrimination laws are associated with lower probabilities that older protected individuals are retired. Overall, the evidence indicates that both state and federal age discrimination laws increase employment of older individuals, providing a basis for the inquiry in the present paper as to whether age discrimination laws enhanced the effects of increases in the FRA.

A different perspective on age discrimination laws stems from Lazear's (1979) LTIC model. This model implies that mandatory retirement arises as an outcome of firms and workers solving the incentive problem, and although mandatory retirement is acceptable ex ante to workers, when the mandatory retirement date arrives the wages that workers are paid exceed the value of their leisure time, so that, for workers, mandatory retirement is undesirable ex post. Based on this reasoning, Lazear argued that the central effect of the ADEA was the increase and subsequent elimination of mandatory retirement, which in his view would serve mainly to give a windfall to older workers

⁵ Because some states enacted age discrimination laws prior to the ADEA, the effects of these state laws in this period are identified from changes in states passing laws relative to changes in other states in the same period, allowing changes for older relative to younger individuals common to all states to control for nationwide changes in the behavior of older workers. Subsequently, the effects of the federal legislation are identified from the changes that occurred at the time of enactment of the ADEA in states that previously did not have their own law, relative to those states that previously had an age discrimination law. A potential limitation of this approach is that state laws and the federal law may not necessarily have the same effects. However, the results of Neumark and Stock suggest – as do those of Adams (2004) (discussed below) – that quite similar answers are obtained from examining variation in state and federal laws.

through the elimination of mandatory retirement, while imposing longer-run efficiency costs by reducing the ability of workers and firms to enter into the LTICs that arise in his model.

Neumark and Stock's (1999) research on age discrimination laws re-examined this critique of the ADEA. They first considered the other problem posed by LTICs – specifically, that firms have an incentive to renege on these long-term contracts when workers are relatively older. They argued that the main effect of the ADEA may have been to deter this kind of reneging, *strengthening* the ability of workers and firms to take advantage of these contracts. Indeed, Neumark and Stock suggest that firms would not necessarily have been opposed to this function of the ADEA, as it provided them with a credible way to make the promises implicit in LTICs to retain older workers even when their current earnings rose above their current marginal product. They present evidence suggesting that this was, in fact, the effect of age discrimination laws, as these laws led to steeper earnings profiles for cohorts entering the labor market subsequently. Thus, the evidence presented in Neumark and Stock (1999) casts the effects of age discrimination laws in a more favorable light, arguing that such laws help to resolve problems with respect to the incentives for firms to behave opportunistically in ways that might be viewed as discriminating based on age – in particular, protecting workers, at older ages, from involuntary terminations on their career jobs.

However, the evidence on age discrimination laws is not entirely reassuring with respect to the pending challenges associated with population aging in coming decades. Regarding LTICs, deterring age-based terminations of long-term employees would no doubt contribute to increasing employment of older individuals. But increasing the employment of seniors to help meet the challenges of population aging is likely also to require increased *hiring* of these older workers. And the broader literature on age discrimination laws fails to establish that these laws have helped older workers get hired. Indeed there are some claims that they have the opposite effect.

The evidence that most directly counters the conclusion that age discrimination laws help older workers is probably Lahey's (2008b) study of the effects of state age discrimination laws. She argues that workers in states with their own age discrimination

laws are protected by stronger laws than are workers in states without their own laws, for two reasons. First, in states with their own laws workers have longer to file age discrimination claims.⁶ And second, fair employment practices agencies in these states may be able to process claims more quickly than the EEOC (although Lahey presents no evidence that states are more effective or efficient than the EEOC).⁷

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, reduced their hours (including zero hours for the non-employed), made them more likely to be retired, and reduced their hiring.⁸ 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 earlier.

The evidence is less than clear-cut, however. Lahey characterizes the pre-1978 period as one in which the ADEA had little effect, which is why she splits the sample into the pre-1978 period and the subsequent period. 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. Her evidence shows that between the pre-1978 and the 1978-1991 period, hiring and hours of workers over 50 years of age *fell* in states with their own age discrimination laws, relative to the states

⁶ In particular, in states that do not have their own statutes, workers must file a claim with the EEOC within 180 days, whereas when the state has its own statute and an FEP commission or agency, the worker has 300 days to file a claim under federal law with the state's FEP agency or the EEOC.

⁷ She also notes that some states protect workers in smaller firms than those covered by the ADEA, but – unlike in the present paper – does not use state variation in the firm-size cutoff for state age discrimination laws.

⁸ Note that the employment (actually, weeks worked) results and the retirement results are the opposite of those in Adams (2004), and the employment results also contrast with those in Neumark and Stock. In addition, the conclusions about adverse hiring effects are stronger than those Adams draws, although she measures hiring better than Adams by using matched CPS data files.

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 and hiring 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 the differences – post-1978 – 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.

Overall, then, this study does not establish that age discrimination laws deter employment or specifically hiring of older workers. However, the logic of the argument, and hence the hypothesis that age discrimination laws deter hiring of older workers, may still be correct. And the evidence in Adams (2004) does not suggest any beneficial hiring effects of age discrimination laws, and perhaps the opposite, especially for those aged 65 and over. Finally, it is worth pointing out that the present paper builds on Lahey’s (2008b) analysis in an important way, by exploiting variation in state age discrimination laws. However, it differs by looking at many dimensions of variation in these laws, rather than simply whether there was or was not a state law. And it turns out that a feature of state laws that Lahey emphasized – the longer time to file a claim (statute of limitations), although in this case the statute of limitations under *state* law – is the one feature of state laws that does not matter.

The following sections describe our approach and findings regarding how age discrimination laws influenced the effects of increasing the FRA on retirement and employment. As noted earlier, perhaps the most natural perspective is that if age discrimination deters the employment of older workers, then demand-side efforts to deter age discrimination may boost the effectiveness of supply-side efforts to increase labor supply. As the immediately preceding discussion suggests, however, the opposite is also

possible. In particular, if much of the adjustment to a higher FRA occurs via hiring, and age discrimination laws deter hiring of older workers, then in states with stronger age discrimination protections for older workers the response to increases in the FRA could have been weaker, rather than stronger.

III. The Increase in the Full Retirement Age (FRA)

The basic empirical strategy is to ask whether, as the FRA increased, the changes in Social Security claiming or employment were stronger where state age discrimination laws provide greater protections to older workers. The strategy therefore rests on the effects of the increase in the FRA on Social Security claiming and employment.

The original Social Security Act of 1935 set the FRA – the minimum age for receiving full Social Security retirement benefits – to be 65, but the Social Security Amendments of 1983 implemented increases in the FRA starting with people born in 1938 or later (Svahn and Ross, 1983). Beginning with this cohort, the FRA was slated to gradually increase by two months per cohort year until it reaches 67, as shown in Table 1. The sample period we study covers most of the first round of phased increases in the FRA.

The increases in the FRA create incentives for changes in Social Security benefit claiming and employment behavior in a number of ways. First, as the age at which a Social Security recipient can receive full benefits increased, the actuarial adjustment of early benefits (for which people are still first eligible at age 62) lowered benefits at age 62 by one percent for each two-month increase in the FRA. This presumably reduces incentives to claim benefits early, and increases incentives to work after age 62. Second, behavior may change around the FRA, with those who reach age 65 *after* the FRA increases delaying their Social Security claiming to their new FRA, and increasing their employment in the interim, for three reasons. First, full benefits are paid at the FRA, and even though the adjustment of benefits based on Social Security claiming date is actuarially fair, some may want to attain this “full” benefit level. Second, the earnings test applies before the FRA, reducing incentives to start drawing Social Security benefits before the FRA for those with earnings. And third, once a person reaches the FRA, delaying Social Security claiming triggers the Delayed Retirement Credit (DRC), which

increases benefits by a given percentage for each year of delay (currently, only up to age 70). This adjustment (implemented in 1972) has historically been smaller than the increase in benefits from waiting until the FRA to claim benefits, introducing a kink in the budget constraint that induces Social Security claiming at the FRA (Pingle, 2006); although the DRC has increased over time, for most of the cohorts considered in this paper (through the 1940 cohort) the rate of increases in benefits for delaying Social Security claiming after the FRA was lower than the rate of increases before the FRA, implying a kink in the budget constraint even before one takes account of the earnings test.⁹

Given that the empirical strategy rests on the effects of increases in the FRA on Social Security benefit claiming and employment, we begin with a more limited analysis that estimates these effects, after describing the HRS data.

IV. HRS Data

Our analysis uses the Health and Retirement Study (HRS), a large, longitudinal dataset that covers older individuals biennially starting in 1992. We use data from nine waves from 1992 until 2008, which extends through the first phase of increases in the FRA. The initial HRS cohort was born from 1931 to 1941, but newer cohorts have been added to the study, so that currently the oldest cohort in the HRS was born in 1924 and the youngest cohort was born in 1953.¹⁰ In addition, although the criterion for inclusion in the HRS depends on the birth year of the respondents, spouses of the respondents are also included as separate respondents, with birth years that range from 1890 to 1983. Because the respondents targeted in the original HRS cohort were aged 62-72 in 2003,

⁹ See www.ssa.gov/oact/quickcalc/early_late.html and www.ssa.gov/retire2/delayret.htm (both viewed September 6, 2011). The increases in the DRC would be predicted to increase employment and delay retirement at the FRA (reducing the bunching of retirement at the FRA). Thus, there is no reason to expect the increases in the DRC to underlie evidence that retirement dates increase to the new FRA as the FRA increases. There were changes in the earnings test in 2000, after which it only applied to those between age 62 and their FRA (Pingle, 2006), but this does not generate any confounding change with increases in the FRA.

¹⁰ The Study of Asset and Health Dynamics among the Oldest Old (AHEAD) cohort, born before 1924, was first interviewed in 1993. The Children of Depression (CODA) cohort, born between 1924 and 1930, and the War Baby (WB) cohort, born between 1942 and 1947, were first interviewed in 1998. The youngest Early Baby Boomer (EBB) cohort, born between 1948 and 1953, was first interviewed in 2004.

the HRS data cover exactly the right ages to study the effect of first phase of increases in the FRA. We restricted our data to the 1931-1943 birth cohorts. Although no one in the 1943 birth cohort reaches age 66 by 2008, the extension from the original cohort for a couple of additional years provides a substantial number of observations in the 65th year on those for whom the FRA increased, hence providing information on how changes in the FRA affect behavior relative to those of very similar ages in earlier years. We omitted both younger and older respondents and spouses to avoid issues relating to sharp differences in Social Security claiming at much older or much younger ages.

We study men only, to minimize complexity from issues pertaining to eligibility for Social Security retirement benefits. Everyone born in 1929 or later needs 40 covered quarters to be eligible.¹¹ In 1950, the labor force participation of men aged 16 years and older was 86.4 percent, versus 33.9 percent for women, and by 1960 the difference had narrowed only slightly, to 83.3 percent for men and 37.7 for women (Fullerton, 1999). These differences imply that eligibility concerns for women, among the cohorts in the HRS, can be severe, whereas for men they are likely negligible. Although we could in principle identify women who are eligible, they would represent a highly selective sample.

Our analysis requires the precise measurement of when a person reaches the FRA, down to the level of detail of the two-month increases in the FRA shown in Table 1. The HRS only provides respondents' month and year of birth, and not the exact date, but this generates virtually no measurement error because the FRA depends *only* on the month and year in which the respondent was born. For example, all respondents born between March 2, 1937 and April 1, 1937 reached the FRA at the beginning of March, 2002.¹² Thus, except for this one-day shift, month and year of birth is sufficient to determine whether a person has reached the FRA at the time of an HRS interview. The HRS oversamples Hispanic, blacks, and residents of Florida, but we do not use the sampling weights since the oversampling can increase efficiency of the estimates.

¹¹ See <http://www.socialsecurity.gov/retire2/credits2.htm> (viewed March 17, 2011).

¹² See <http://www.socialsecurity.gov/retire2/agereduction.htm> (viewed March 21, 2011). (This was also confirmed in a query to the Social Security Administration, response 3796284, April 26, 2010.)

The dependent variables we study are Social Security claiming and employment. We pin down Social Security claiming precisely, based on the month in which a person started to collect Social Security benefits. We report results for full-time employment (35 hours or more per week), which is, in a sense, most “opposed” to Social Security claiming, and generally results in higher Social Security payroll tax payments. We also report results for any employment, which can include some of the part-time employment through which older individuals often transition on the way to full retirement (e.g., Cahill et al., 2006). Appendix Table A gives descriptive statistics for the HRS data used in the regressions.

Our empirical analyses utilize fine age distinctions among HRS respondents based on month of birth, which is best explained with reference to Table 1. Consider those aged 65 years and 4 months in different years of HRS data. Those observed at this age *before* the FRA increased to 65 years and 4 months are *not* “caught” by the increase in the FRA, while those observed after the FRA increased to 65 years and 4 months *are* “caught” by the increase. Table 2 shows that we have many observations in the HRS, in the 2002-2008 waves, on individuals over age 65 who are caught by the increase in the FRA. We have many more observations, of course, on those aged 62-65 who face an FRA greater than age 65 and 0 months.

V. The Effects of Increases in the Full Retirement Age on Social Security Claiming and Employment

Empirical Approach

As a preliminary to our main analysis, we first estimate the effects increases in the FRA on Social Security claiming and employment behavior, without regard to *variation* in these changes in behavior with state age discrimination laws that is our question of interest. To some extent we focus on changes in behavior of those 65 and a little bit older who are caught by increases in the FRA. But responses among those aged 62-65 whose FRA increases are also important. Mastrobuoni (2009) focuses on this latter group only, and finds that those who faced reduced benefits at the early retirement age because of increases in the FRA retired later.¹³ And he focuses only on the aggregate

¹³ Pingle (2006) finds that the FRA increased labor supply among those aged 60-64, but not

variation over time induced by the increase in the FRA, rather than any variation across states based on their laws. One reason to emphasize the results for those aged 65 and over is that identification of the effects of increases in the FRA on behavior at the FRA (or what *was* the FRA) may be cleaner because we rely on changes in behavior across very narrow age ranges (defined in months), making it easier to rule out coincident changes in Social Security claiming or employment behavior by age as an explanation of our findings.¹⁴

We estimate linear probability models for our benefit claiming and employment outcomes, with a rich set of age dummy variables, and variables capturing whether one was “caught” by the increase in the FRA –for those above age 65 but less than the FRA, and for those in the 62-65 age range. The regression model is

$$R_{ist} = \alpha + \beta A65FRA_{ist} \cdot IFRA_t + \gamma A6265_{ist} \cdot IFRA_t + \lambda IFRA_t + \sum_k A^k_{ist} \delta_k + X_{ist} \theta + \varepsilon_{ist}. \quad (1)$$

In equation (1), i , s , and t denote individual, year, and state. A is a vector of age dummy variables in two-month cells, and X is a vector of individual-level demographic and other controls. $IFRA$ is a dummy variable equal to one for cohorts that faced an FRA higher than age 65 (cohorts born 1938 and later). $A6265$ is a dummy variable for those in the age range 62-65 (exclusive of 65). And $A65FRA$ is a dummy variable for those aged 65 to the FRA for their cohort. Given these definitions, $A65FRA \cdot IFRA$ and $A6265 \cdot IFRA$ capture those “caught” by the increase in the FRA, in the affected age ranges.

Equation (1) can be interpreted as embedding two difference-in-differences, one for those between age 65 and their FRA, and one for 62-65 year-olds. The corresponding estimators – β and γ – measure the difference in the dependent variable for either of these

among those aged 65-69. However, his findings are fragile, likely due to using data from a period with very few workers subject to a higher FRA.

¹⁴ Mastrobuoni (2009) uses CPS data rather than HRS data, arguing that the CPS data are preferable because of larger sample sizes. Although this is true, the HRS offers the advantage of being able to pin down almost exactly who is caught and when by increases in the FRA, as explained in the previous section.

two age groups for cohorts born before or after the increase in the FRA, relative to the difference for other age groups for cohorts born before or after the increase in the FRA.¹⁵

The “caught” variables shift when the change in behavior (benefit claiming or employment) occurs, with no affect on behavior outside of the affected age ranges. To see this, suppose that the data runs only through the 1938 birth cohort, for which the FRA shifts from 65 to 65 and two months. For the earlier cohorts not affected by the increase in the FRA ($IFRA = 0$), their change in behavior at age 62 is $(\delta_{62} - \delta_{61\&10mo.})$, their change at age 65 is $(\delta_{65} - \delta_{64\&10mo.})$, and their change in behavior at age 65 and 2 months is $(\delta_{65\&2mo.} - \delta_{65})$. For those caught by the increase in the FRA ($IFRA = 1$), their change in behavior at age 62 is $(\{\delta_{62} + \gamma\} - \delta_{61\&10mo.})$, their change at age 65 is $(\{\delta_{65} + \beta\} - \{\delta_{64\&10mo.} + \gamma\})$, and their change in behavior at 65 and 2 months is $(\delta_{65\&2mo.} - \{\delta_{65} + \beta\})$. Thus, the increase in the FRA induces one shift at age 62, another at age 65, and another at the new, higher FRA, with the sum of the cumulative shifts netting out to zero.

Results

Table 3 reports the estimates of equation (1). It does not show all of the age dummy variables, but enough to see the changes around ages 62 and 65. Note, first, that there is a distinct increase in the probability of Social Security claiming at age 62, when people are first eligible for Social Security benefits. There is also a distinct increase at age 65. Such changes are generally less pronounced for both employment measures, although the data certainly point to fairly sharp declines in the months surrounding these ages. We would not expect as distinct behavior for employment, as one can make a transition to receiving Social Security benefits without a change in employment status

¹⁵ Technically speaking, because the FRA varies by cohort, $A65FRA$ is not a simple dummy variable for an age range, but is instead defined to equal one when (i) a person is in a cohort affected by the increase in the FRA, and (ii) that person is between 65 and the FRA for his cohort. As a result, the interaction with $IFRA$ is redundant. However, we leave it in to make clear the parallel to a standard difference-in-differences estimator. Another way to see this is to suppose we had data only for the first birth cohort affected by the increase in the FRA (born in 1938). Then we would not have the problem of a changing age range based on cohort, $A65FRA$ would be defined to simply equal one for age 65 to 65 and 2 months (exclusive), and the equation would be a standard difference-in-differences specification. Equation (1) can be motivated by expanding it to allow separate estimates corresponding to β for each affected cohort, and then constraining these estimates to be equal across the affected cohorts. This is spelled out more explicitly in the discussion of the main empirical analysis below.

(being either non-employed in the period before and after starting to receive benefits, or employed).

Of more direct interest are the estimates in the first two rows of the table. For Social Security claiming, for those aged 65 and over, but below the FRA, there is a sharp change induced by the increase in the FRA, lowering the probability of Social Security claiming (in percentage terms) by 21.9 percentage points. Recall that this is measured relative to Social Security claiming probabilities for the very narrow age range that is affected by the increase in the FRA in our sample period. The magnitude corresponds quite closely to the overall increase in the probability of Social Security claiming for those not affected by the increase in the FRA. For example, measuring the change from age 64 and 8 or 9 months to age 65 and 4 or 5 months, the increase in Social Security claiming probability is 22 percentage points, indicating that the decline in Social Security claiming for those caught by the increase in the FRA largely offsets the increase over the age range just surrounding the 65th birthday.

For those aged 62-65 the change associated with being caught by the increase in the FRA is less pronounced, lowering the probability of claiming by 5.1 percentage points. In contrast to the corresponding results for those aged 65 and over, for 62-65 year-olds this effect is smaller relative to the increase in claiming around age 62.

For employment, the estimated shifts associated with the increases in the FRA, as expected, are positive rather than negative. The estimates are smaller, and statistically significant only for full-time employment for those aged 65 and over, and only at the 10-percent level. For this age group, again, the magnitudes – a 4.3 percentage point increase in full-time employment (and 2.8 for any employment) – offset a good share of the declines of 6.6 (4.8) percentage points that otherwise occur over this same age range. Again, though, we would anticipate that the effects would be stronger for Social Security claiming.¹⁶ For 62-65 year-olds the estimates are very small and statistically insignificant.

In Table 4 we look more closely at the effects for 62-65 year-olds, modifying the

¹⁶ Note, though, that the smaller average changes in employment for those caught by the increases in the FRA do not imply that we will not find strong interactions of age discrimination protections and being caught by increases in the FRA for employment.

analysis in two ways that might detect a stronger impact for those more strongly affected by the increase in the FRA. First, in columns (1), (3), and (5) we break up this age range into three single-year cells, to see whether the effects are largest for 62 year-olds, who might have greater flexibility to respond. However, there is no such evidence. More interestingly, in columns (2), (4), and (6) we distinguish people by how large an increase in the FRA they were caught by – 2 or 4 months, 6 or 8 months, or 10 or 12 months – on the presumption that the response at younger ages might be larger the larger the increase in the FRA. For Social Security claiming, the results are strongly consistent with this expectation, with much sharper and statistically significant changes for those facing increases in the FRA of 6 months or more.

VI. Data on Age Discrimination Laws

Our main empirical analysis relies on the creation of a database of state age discrimination laws. The compilation of our data on state age discrimination laws required extensive background research on state statutes and their histories, culled from legal databases including Lexis-Nexis, Westlaw, and Hein Online, as well as many other sources. The first step in assembling information on state age discrimination laws was to identify the appropriate state statute, which can be complicated because the age discrimination law can be listed under various sections of state laws. For example, depending on the state, the age discrimination law may be classified as a human rights law, a fair employment act, or a separate age discrimination act. After the appropriate statute was identified, we traced the history of the statute using the legal databases, recording changes in content and the year of any amendments. Furthermore, in some cases we had to look beyond the statutes to information from state agencies. For example, for Alaska and Vermont information on the statute of limitations was not found in the state statutes, but instead came from state agency websites.¹⁷

Because it is complicated to read and interpret the law correctly solely based on statutes, we cross-checked our understanding of the statute with other legal references or

¹⁷ See <http://humanrights.alaska.gov/html/services/complaints.html> and http://hrc.vermont.gov/sites/hrc/files/pdfs/laws/vhrc_rules.pdf (both viewed March 17, 2011).

treatises and additional sources of information on state laws.¹⁸ The other sources were also useful because of a further challenge in reading statutes. In particular, one section may define what a discriminatory act is, while the authorization to set rules on filing periods may be delegated to the Civil Rights Commission, or the remedies or means of enforcement may be listed under a different section of the statute. Michigan provides a good example illustrating both this complexity and how using multiple sources helped in fully understanding the state's law and its evolution. Article 6(f) of the Elliott-Larsen Civil Right Act in Michigan authorizes the Civil Rights Commission to promulgate rules, and on October 2, 1979, the Commission filed the current rules with the Secretary of State. Thus, Michigan's 180-day period for filing a complaint is not specified in the statute. If we had relied solely on the state statutes, we would not have obtained this information because the actual statute does not record and trace the changes of the specific rules the Civil Rights Commission filed.

Furthermore, to minimize inaccuracies, once all the necessary information was obtained from the statute, we compared and validated this with information from other sources. If information obtained from different sources coincided, we were confident that the information was correct. In cases of what should be unambiguous information – in particular the employment level at or above which the law applies – we use the information from the statute regardless. However, in cases of information that can be more easily misinterpreted from the statute – in particular, regarding remedies or statutes of limitations (like in the Michigan example discussed above), when we found discrepancies we turned to the state agencies for corroborating information (including both checking websites and direct contacts). Despite all these efforts, there are a few cases where we could not fill in the history of the state statutes for our sample period.

Table 5 reports the summary of state laws for 1992 and 2008 – the years that bracket our sample.¹⁹ We focus on four aspects of age discrimination laws that, based on

¹⁸ These included Fitzpatrick (2005-2007), Fitzpatrick and Perine (2008), Fitzpatrick et al. (2009), Leiter (1993, 1997, 1999, 2003, 2005, 2008), Nelson (1993-2003), Nelson and Fitzpatrick (2004), Northrup (1980), and Ross and Barcher (1983).

¹⁹ We assembled data for all the intervening years as well as earlier years. However, the data for the earlier years do not play a role in this paper. And there are few changes of relevance in the intervening years. Nonetheless, there are some changes, and in the empirical analysis we use these

our research, seem to have significant variation above and beyond what is specified in the federal law, hence providing variation in the strength of age discrimination protections across states. The first is the firm-size cutoff for applicability of the law. If the employer does not have a number of employees greater than or equal to the number of employees specified in the first two columns of Table 5, the state law is not applicable.²⁰

Second, we use information on remedies allowed under the state laws. We focus on whether compensatory or punitive damages are allowed, which they are not under federal law.²¹ Some states require proof of intent to discriminate in order for compensatory or punitive damages to be awarded, whereas others require “willful” violation. Because the federal law allows additional liquidated, non-punitive damages (double back pay and benefits) when there is “willful” violation, the question of whether the state requires intent or willful violation may seem to be potentially relevant in deciding whether a state law offers greater protection. However, willful violation is a much stricter standard than intent (Moberly, 1994). Moreover, compensatory or punitive damages are almost certainly greater than liquidated damages, and they can be much greater. As a consequence, a state law that provides compensatory or punitive damages, whether or not this requires proof of intent or willful violation, clearly entails stronger remedies than the federal law.

Third, we focus on the statute of limitations, or the period in which a claim must be filed. Under the ADEA, if the state does not have a state agency charged with enforcing discrimination laws, the ADEA charge must be filed within 180 days; it has to be filed within 300 days in a state that has a state law and agency (Gold, 1993; O’Meara, 1989). We focus on whether the statute of limitations under state law extends longer.

Finally, we also use the variation in recoverability of attorneys’ fees. We would expect more age discrimination claims in states that allow the recovery of the attorneys’

laws by state and year.

²⁰ For example, in Florida a worker who works at a firm that employs fewer than 15 employees is not covered under the Florida state law. On the contrary, all employees in Colorado are covered by state law because it is applicable to all firms with at least 1 employee.

²¹ See United States Equal Employment Opportunity Commission (2002). In addition, O’Meara (1989) states that damages for pain and suffering are occasionally permitted in ADEA in federal court when they arise out of state claims although pain and suffering are not allowed under the ADEA (pp. 334-5).

fees because this provides greater financial incentives for plaintiffs and their attorneys, as fees can be awarded as part of the judgment on behalf of plaintiffs. The law on recovery of attorneys' fees is not straightforward, and it *cannot* be obtainable solely from state age discrimination statutes. Federal courts follow the American Rule that each side bears its own attorneys' fees in litigation.²² Thus, the general rule is that a specific statute is necessary to authorize recovery of attorneys' fees. The ADEA specifically allows the recovery of attorneys' fees if an ADEA plaintiff is successful; it states that the "court in such action shall [...] allow a reasonable attorneys' fee to be paid by the defendant, and costs of the action."²³

Classifying a state age discrimination law as allowing recovery of attorneys' fees would be most clear if a state age discrimination statute specifies this recoverability. Things are more complex, however, because some states instead have a general statute authorizing fee-shifting in whole categories of cases. Thus, accurate information on the recovery of attorneys' fees required research beyond state age discrimination laws. For example, in Alabama, age discrimination is a violation of the Alabama Age Discrimination in Employment Act (AADEA) under Title 25, Industrial Relations and Labor, Chapter 1, Article 3. The statute governing the AADEA does not specifically authorize the recovery of attorneys' fees for the prevailing party. However, Alabama's general statute governing civil actions allows the recovery of reasonable attorneys' fees. It states that "in *any* civil action commenced or appealed in any court of record in this state, the court shall award, as part of its judgment ... reasonable attorneys' fees and costs ..."²⁴ Thus, although Alabama's age discrimination law does not specify the recovery of attorneys' fees, the plaintiff may still be allowed to recover attorneys' fees pursuant to the general statute on civil actions. In contrast, Idaho's Human Rights Act does *not* allow an award of attorneys' fees to a prevailing plaintiff in age discrimination case because the

²² See West Group and Lawyers Cooperative Publishing (2001, Section 55, p. 194). However, some legal scholars argue that American Rule has become the exception because of nearly 100 federal fee-shifting statutes such as Civil Rights Attorney's Fees Awards Act of 1976 (Larson, 1982).

²³ This language is stated under Title 29, Section 216 (b), which is incorporated in the ADEA by Title 29, Chapter 14, Section 621 (b).

²⁴ This is from Title 12, Chapter 19, Article 6, Section 292.

Idaho Human Rights Act does not grant an award of attorneys' fees.²⁵

In collecting information on provisions regarding attorneys' fees, we relied first on state age discrimination statutes. If recoverability of attorneys' fees is specified under the state age discrimination statute, we are confident that the state allows the recovery of attorneys' fee. If it is not specified under the age discrimination statute, we had to research general statutes authorizing fee-shifting. To do this, we looked for age discrimination cases filed under the state age discrimination law, relying on court decisions and the language used in those decisions. Even for the states where recoverability of attorneys' fees is specified, we searched court decisions to cross-check our understanding of the provision. For example, in New Mexico the state age discrimination statute only specifies the recovery of attorneys' fees for appeal. However, in *Smith v. FDC Corporation v. Cox*,²⁶ the court states that "reasonable attorney's fees may be awarded at court's discretion to prevailing complainant pursuant to New Mexico Human Rights Act." When we can find these explicit statements in a court ruling, we determine that the state allows the recovery of the attorneys' fees. Similarly, in New York, the recoverability of attorneys' fees is not codified. However, the ruling *Lightfoot v. Union Carbide Corp*²⁷ states that "attorney fees are *not* recoverable under New York State Human Rights Law which prohibits employer from engaging in unlawful discriminatory practices because of age."²⁸

Table 6A shows our coding of the state laws for use in our empirical analysis, and the comparison with the federal law. We use a firm-size cutoff of lower than 10 workers to capture states where small firms not covered by the ADEA *are* covered by the state age discrimination law. As shown in Table 5, we could have simply used whether

²⁵ *Stout v. Key Training Corporation* (144 Idaho 195, 158 P. 3d 971, 2007) exemplifies the stringency of the courts in awarding attorney's fees in Idaho. The jury's verdict was in favor of former employees on a statutory discrimination claim under the Idaho Human Rights Act. Although the former employee may have been able to recover attorneys' fees under a contract claim, the court did not award the fees under a Human Rights statutory claim because the act does not make explicit allowance and the plaintiff prevailed under the Human Rights statutory claim.

²⁶ *Smith v. FDC Corporation v. Cox*, 109 N.M. 514, 787 P.2d 433 (1990)

²⁷ *Lightfoot v. Union Carbide Corporation*, 901 F. Supp. 166 (1995)

²⁸ An appendix table available from the authors provides extensive documentation of our information on recoverability of attorneys' fees.

the cutoff was below the ADEA's cutoff of 20, but this would have included nearly all states in the group with lower firm-size cutoffs, rather than generating reasonable sample sizes in the two groups we want to compare. Note that we use a statute of limitations variable that codes whether a worker has more than 300 days to file a claim. This captures whether a state law allows a longer statute of limitations than the ADEA establishes in states with age discrimination laws and enforcement agencies. Another potential coding of this variable would be simply whether there is a state age discrimination law and enforcement agency, since in that case the statute of limitations for federal claims is longer (300 days) than if there is not a state law and agency (180 days).²⁹ However, as Table 5 shows, only a few states do not have age discrimination laws. Table 6B shows the means for these coded variables. And Table 6C documents the very limited variation in state age discrimination laws – variation that is *not* needed for our identification strategy discussed below.

VII. State Age Discrimination Laws and the Increase in the Full Retirement Age

Empirical Approach

We now turn to our main analysis that asks whether responses to the higher FRA for older individuals caught by the increases in the FRA differed in states with stronger age discrimination laws. This analysis can be thought of as expanding the difference-in-differences estimators in equation (1) to difference-in-difference-in-differences (DDD) estimators, allowing the estimated effects of being caught by the increase in the NRA – captured in the equation by the interactions $A65FRA \cdot IFRA$ and $A6265 \cdot IFRA$ – to vary with state age discrimination laws. Expanding on equation (1), then, the estimating equation is

$$\begin{aligned}
 R_{ist} = & \alpha + \beta_1 A65FRA_{ist} \cdot IFRA_t \cdot PA_s + \gamma_1 A6265_{ist} \cdot IFRA_t \cdot PA_s \\
 & + \beta_0 A65FRA_{ist} \cdot IFRA_t + \gamma_0 A6265_{ist} \cdot IFRA_t + \kappa IFRA_t \cdot PA_s + \lambda IFRA_t + \psi PA_s \\
 & + \sum_k A_{ist}^k \cdot PA \delta_{1k} + \sum_k A_{ist}^k \delta_{0k} + X_{ist} \theta + \varepsilon_{ist}.
 \end{aligned} \tag{2}$$

In equation (2), PA is a dummy variable for a particular feature of state age discrimination laws that provides greater protection for older workers than the ADEA. In

²⁹ This would be more in line with Lahey's (2008b) analysis.

the actual implementation, we also estimate specifications with multiple features of state age discrimination laws considered simultaneously.

Equation (2) embeds two DDD estimators. One (β_1) is the difference in the dependent variable for those between 65 and their FRA born in cohorts affected by the increase in the FRA in states with stronger age discrimination protections relative to the same ages and cohorts in states without the stronger protection, in turn relative to the same difference-in-differences for those outside the ages from 62 to the FRA. The second DDD estimator (γ_1) is the parallel estimator, but for 62-65 year-olds relative to those outside the age ranges from 62 to the FRA.³⁰

Equation (2) can be made a bit more flexible, and we do this in an extension of the empirical analysis. First, we can include state dummy variables rather than simply PA , to allow more heterogeneity across states in the intercept of the model.³¹ Second, we can include birth-cohort dummy variables by year rather than simply $IFRA$, to allow more heterogeneity in the intercepts across birth cohorts. And third, we can introduce interactions between these birth-cohort dummy variables and PA , to allow cohort profiles to differ across states with and without (or with different kinds of) stronger age discrimination protections. We report estimates with the first modification – adding the state dummy variables – and then with all of the modifications. Identification of the

³⁰ Paralleling the discussion of equation (1), for the older group this setting is more complex than a conventional DDD estimator because the age range (65 to the FRA) varies by birth cohort for those born in 1938 or later; the earlier comment about the redundancy of the $IFRA$ interaction for this older group applies here as well. One way to motivate how this leads to a conventional DDD estimator is to expand the equation so that there are separate $A65FRA \cdot IFRA \cdot PA$ and $A65FRA \cdot IFRA$ variables for each affected birth cohort, with the age range 65 to 65 and 2 months for the 1938 birth cohort, 65 to 65 and 4 months for the 1939 birth cohort, etc. With six such cohorts in our sample, we would then effectively have *seven* DDD estimators, one for each of these cohorts and one for the 62-65 year-olds. We can then constrain the effects (corresponding to β_1 and β_0 in equation (2)) to be the same for each of the over 65 age groups. This is equivalent to redefining those over age 65 and caught by the increase in the FRA based on age and birth cohort. For example, suppose we had only the 1938 and 1939 birth cohorts to consider. Then an individual would be classified as over 65 and caught by the increase in the FRA if age is between 65 and 65 and 2 months (exclusive) and birth year equals 1938, *or* if age is between 65 and 65 and 4 months (exclusive) and birth year equals 1939. Collapsing the larger model in this way yields equation (2).

³¹ The state dummy variables do not quite subsume the main effect of the state age discrimination protections because of the handful of states with variation in these protections over time. As a short-hand, though, PA in equation (2) only has an s subscript.

effect of increases in the FRA, and how this effect varies with state age discrimination laws, seems quite compelling because it comes from differences in behavior of those at very similar ages in nearby years, and in that sense has the same flavor as a regression discontinuity design.

We incorporate information on the recoverability of attorneys' fees differently from the other features of state age discrimination laws in equation (2). Because the federal ADEA permits recoverability of attorneys' fees, recoverability under state statute should only matter with respect to provisions of state laws that strengthen age discrimination protections relative to the federal laws, in which case recoverability under state laws might enhance the effect of such provisions of state age discrimination laws. Therefore, when we study attorneys' fees we introduce interactions between recoverability of these fees and the other three features of state age discrimination laws.³² That is, whenever *PA* appears in equation (2), we add another corresponding variable involving *PA·REC*, where *REC* denotes recoverability of attorneys' fees.

Main Results

Our first set of estimates of versions of equation (2), using the more parsimonious specification described above, is reported in Table 7.³³ The table reports the main interactions of interest, along with the main effects of being caught by the increase in the FRA.

Columns (1)-(4) report the results for Social Security claiming. The estimates in columns (1)-(3) consider the different features of age discrimination laws one at a time. The evidence for those between age 65 and the FRA, in the top three rows, indicates that in states with stronger remedies the effect of the increase in the FRA in reducing benefit claiming is stronger, with the difference statistically significant at the 5-percent level.³⁴ The estimated interactions for the lower firm-size cutoff or longer statute of limitations

³² In other words, there is no separate main effect of *REC*. Rather, there are two treatments associated with *PA* – one when *REC* equals zero, and one when *REC* equals one.

³³ The “caught by increase in FRA × age range × age discrimination law feature” variables reported in the table correspond to *A65FRA·IFRA·PA* and *A6265·IFRA·PA* in equation (2). The “caught by increases in FRA × age range” main effects correspond to *A65FRA·IFRA* and *A6265·IFRA*.

³⁴ Henceforth, unless otherwise specified, statements that estimates are statistically significant refer to the 5-percent level.

are also negative, but much smaller and statistically insignificant. In the case of stronger remedies, the differential impact on Social Security claiming is 10.4 percentage points. The estimated main effect of being caught by the increase in the FRA, for those between age 65 and the FRA, is always negative and statistically significant, implying that in states without these kinds of stronger age discrimination protections the increase in the FRA still lowers Social Security claiming among those over age 65 and caught by the increase in the FRA; but not surprisingly, this estimated main effect is smaller in column (2), consistent with the difference in the effect of the increase in the FRA induced by stronger remedies under state age discrimination laws.

In column (4) all of the interactions are entered simultaneously. The estimates are similar to those in columns (1)-(3), and the standard errors do not increase much, indicating that we can identify the effects of the different types of state age discrimination protections. The estimated interactions for a lower firm-size cutoff and longer statute of limitations are still small and statistically insignificant. But the estimated interaction with stronger remedies is a shade larger, and still statistically significant.

The next three rows of estimates report the same type of evidence for 62-65 year-olds. All of the estimates are smaller for this age group, although the signs are still always negative for a lower firm-size cutoff and stronger remedies, and all are statistically insignificant (although the estimated interaction for stronger remedies in column (4) is nearly significant at the 10-percent level). Thus, with regard to claiming Social Security benefits, state age discrimination laws that offer stronger remedies enhance the response of those aged 65 and over to the increase in the FRA, and there is at best weak evidence of a similar (although smaller) effect for those aged 62-65.

Columns (5)-(12) instead look at employment – full-time employment and any employment. For those aged 65 and over, when the state age discrimination protections are included one at a time, the estimates indicate that stronger state age discrimination laws with respect to both firm size and stronger remedies enhance the employment response to increases in the FRA. The estimated magnitudes are fairly similar, and all are statistically significant. There is never any evidence that statutes of limitations matter.

When all of the age discrimination protections are included simultaneously, in

columns (8) and (12), it appears that the evidence regarding the effects of stronger remedies persists for full-time employment, whereas for any employment the lower firm-size cutoff is more important. This could reflect sampling variation, or it could reflect a real difference. For example, older workers often make transitions from full-time work, to bridge jobs that are often part-time, to retirement. We might therefore suspect that the effects on full-time employment come mainly from reductions in terminations, whereas the effects on any employment reflect hiring to a greater extent, in which case the evidence might imply that stronger remedies have more to do with deterring discriminatory terminations, whereas lower firm-size cutoffs have more to do with reducing discrimination in hiring. However, any such conclusions require further research.

In the HRS data, as individuals age they are more likely to be employed at smaller firms. Using the firm size categories available in the HRS, at ages younger than 62, 11.6 percent of workers in our sample were employed at firms with 14 or fewer employees. This percentage rises to 15.6 for 62-65 year-olds, and 23 percent for those aged 65 and over.³⁵ Thus, there is likely more hiring of older workers at smaller firms. In addition, these percentages indicate that the effects of lower firm-size cutoffs in state age discrimination laws are not implausible, as – especially for those aged 65 and over – a sizable proportion of workers are employed at small firms.

Again, we report parallel results where we test for changes at ages 62-65 for those who are caught by the increase in the FRA, in the next three rows. In these estimates, there is a positive and statistically significant result for stronger remedies for full-time employment whether the age discrimination laws are entered one at a time or simultaneously. Aside from that, all of the estimates for 62-65 year-olds are smaller and statistically insignificant.

In columns (5)-(12) most of the estimated main effects are small and statistically insignificant, although most are negative, suggesting that *only* where there were stronger age discrimination protections did the increase in the FRA result in higher employment.

³⁵ We cannot do the computation for firms with fewer than 10 employees, as the bottom two size categories available in the HRS are fewer than 5 and 5-14 employees. But this same pattern with age appears for both of these categories.

Given the evidence in Table 3 of rather weak overall effects of the increase in the FRA on employment, this finding is plausible. Finally, note that in columns (8) and (12), when all of the state age discrimination protections are included simultaneously, the main effects for those aged 65 to the FRA are larger and statistically significant. However, once we include all three state age discrimination protections, the main effects are identified only from states with *none* of these protections, which Table 5 shows is a very small set of states (Alabama, Arkansas, Mississippi, Nebraska, and South Dakota) with small populations as well. Thus, in these specifications the main effects are identified from very few observations (only those in these states in the narrow age range over 65 and caught by the increase in the NRA), and hence the estimated main effects are likely unreliable.

Thus, to summarize, there is evidence that stronger remedies in state age discrimination laws enhanced the effects of increases in the FRA on claiming Social Security benefits and on employment (especially full-time employment) for those 65 and over. There is also evidence that a lower firm-size cutoff enhanced the effect of increases in the FRA on overall employment. And finally, there is evidence that stronger remedies enhanced the effect on full-time employment for 62-65 year-olds. Overall, then, the strongest and most consistent evidence arises for stronger remedies.³⁶

Additional Analyses and Extensions

Next, we extend the results in a few ways. First, Table 8 reports results paralleling Table 7, but from a more saturated model that includes birth-cohort dummies and their interactions with state age discrimination law features. The estimates are very similar, and in some cases the evidence is a bit stronger.

Second, given that the choice of firm-size cutoff to use is somewhat arbitrary, Table 9 reports estimates where we use instead a cutoff of 15 employees. In this table, the odd-numbered columns repeat estimates from Table 7, and the even-numbered

³⁶ These conclusions could be invalid if individuals who want to delay Social Security claiming or work longer when the FRA increases migrate to states with stronger age discrimination protections. This does not seem particularly plausible, given the very narrow age range over which the effects are identified. Moreover, past work looking at migration in this age group in response to economic incentives (variation in Supplemental Security Income benefits) fails to detect evidence of migration responses (Neumark and Powers, 2006).

columns show the estimates using the 15 cutoff instead. In every case, the estimated interactions using the higher cutoff are smaller. In our view, this bolsters the validity of the conclusion that firm-size cutoffs matter. As we include states whose cutoffs for age discrimination protection are more similar to the ADEA, and hence the protections are weaker, we find less evidence of an effect of these protections. Nonetheless, the estimated firm-size interactions remain large, and in many cases statistically significant at the 10-percent level, suggesting that the conclusions about firm-size cutoffs are not fragile.

Finally, we incorporate the information on the recoverability of attorneys' fees, in Table 10. The key question is whether the effects of stronger age discrimination protections are enhanced when attorneys' fees are recoverable, because attorneys should have greater incentive to take on plaintiffs' cases under these circumstances. We report results only for the state age discrimination protections that the preceding analyses suggested matter – a lower firm-size cutoff than the ADEA, and stronger state remedies. And we focus on the simpler specifications studying one age discrimination protection at a time, to keep the specification tractable.

In columns (1) and (2), for claiming Social Security benefits, there is no indication of a difference associated with recoverability of attorneys' fees. Recall that, for this outcome, it was only stronger remedies that mattered, and only for those aged 65 and over, so we should direct attention to the estimates in column (2) for this age group. The estimated interaction between stronger remedies and recoverable fees (-0.013) is small and statistically insignificant, whereas the estimate without the interaction with recoverability of fees (-0.092) is nearly as large as the corresponding estimate in column (2) of Table 7 (-0.104).

In contrast, for the two employment outcomes, there is some evidence that recoverability of attorneys' fees bolster the effect of state age discrimination protections. In only one case is the estimated interaction with recoverability of fees statistically significant (column (4), for 62-65 year-olds, and only at the 10-percent level). But in three of four cases for those between 65 and the FRA, the estimated effect for state age discrimination protections when fees are recoverable – reported as “Sum” – is about

twice as large as when fees are not recoverable (the exception is column (5)). And in these cases, as well as for stronger remedies for full-time employment for 62-65 year-olds caught by the increase in the FRA, the effect of the stronger age discrimination protections is statistically significant *only* in states where attorneys' fees are recoverable.

VIII. Discussion and Conclusions

The evidence indicates that in states with stronger protections against age discrimination in the labor market, older individuals were more responsive to increases in the Social Security Full Retirement Age (FRA). Specifically, where the state laws applied to small firms not covered by the ADEA, employment increased more at ages that were initially beyond but subsequently lower than the FRA – i.e., for those older individuals “caught” by increases in the FRA. Where the state laws provided stronger remedies (harsher penalties), the response to the increase in the FRA was stronger for both employment and claiming Social Security benefits. And we find some evidence that these impacts of state age discrimination laws were stronger when, under state law, attorneys' fees are recoverable. We find some parallel results for those beyond the earlier Social Security claiming age (62) but younger than 65, but only for full-time employment and only for stronger remedies.

The employment findings are particularly significant. Because benefits taken before the FRA are actuarially adjusted, whether or not workers begin to take benefits before the FRA may have little impact on the financial solvency of Social Security. However, if people work longer, they pay taxes into the system for a longer period, which has direct financial implications. As Mastrobuoni, studying the same policy change, puts it, “An increase in labor force participation generates more contributions, which are the trust fund's main source of revenue” (2009, p. 1224). Interestingly, the results suggest that *only* in states with stronger age discrimination protections was there a positive employment effect from increases in the FRA.

This conclusion suggests that Social Security reforms on the supply side intended to enhance incentives for older individuals to remain in the workforce – whether in the form of the second scheduled phase of increases in the FRA, or other changes in incentives – will be more effective if public policy reduces demand-side barriers to the

employment of older workers that stem from discrimination. The states that currently provide stronger age discrimination protections may provide a model for changes in the ADEA that could enhance the effectiveness of future Social Security reforms. Given that these supply-side reforms impose costs on older individuals, it seems reasonable to try to eliminate demand-side barriers to older workers' employment that would otherwise necessitate stronger supply-side changes to achieve solvency of the Social Security system.

Finally, note that the empirical conclusion that stronger state age discrimination laws can enhance the effects of Social Security reforms does not hinge on whether there is in fact age discrimination that is combated by stronger age discrimination laws, or whether instead there is no age discrimination but these laws lead to favoritism for older workers. Of course if the latter characterization is true, there might be other arguments against increasing the strength of age discrimination laws.

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Table 1: Increases in the Full Retirement Age (FRA)

Year of birth	FRA
1937 or earlier	65
1938	65 + 2 months
1939	65 + 4 months
1940	65 + 6 months
1941	65 + 8 months
1942	65 + 10 months
1943-1954	66
1955	66 + 2 months
1956	66 + 4 months
1957	66 + 6 months
1958	66 + 8 months
1959	66 + 10 months
1960 and later	67

Source: U.S. Social Security Administration

(www.ssa.gov/oact/progdata/nra.html, viewed March 11, 2011).

Note: Our sample includes cohorts from birth year 1931 through 1943.

The birth cohorts above the dark line reach the FRA in our sample period.

Table 2: Number of Individuals “Caught” by Increase in Full Retirement Age, by Age and Year of Interview

	65 and 0 or 1 month		65 and 2 or 3 months		65 and 4 or 5 months		65 and 6 or 7 months		65 and 8 or 9 months		65 and 10 or 11 months	
	Caught	Not caught	Caught	Not caught	Caught	Not caught	Caught	Not caught	Caught	Not caught	Caught	Not caught
1992-2002	0	220	0	210	0	187	0	184	0	151	0	181
2003	1	0	0	1	0	1	0	2	0	0	0	2
2004	55	0	48	6	0	59	0	65	0	47	0	64
2005	2	0	1	0	0	0	0	1	0	0	0	2
2006	65	0	48	0	52	0	22	36	0	50	0	49
2007	2	0	1	0	0	0	3	0	0	1	0	3
2008	24	0	29	0	32	0	39	0	21	0	1	19
2009	0	0	2	0	0	0	0	0	0	0	1	0

Note: “Caught” by increase in FRA is a dummy variable equal to 1 for individuals who were subject to an FRA beyond their 65th birthday but have not yet reached the FRA, and 0 otherwise. In this table, which includes only people above age 65, it captures those in age ranges older than the original FRA of 65 before the FRA started to increase, but younger than the FRA given their year and month of birth. In each age-year cell the left column shows the number of people who are “caught” and the right column show the number of people who are not “caught” by the increase in FRA. We can observe both people who are caught and not caught in some age-year cells because they can be interviewed in different months. For example, person A born in May 1939 (whose FRA is 65 years and 4 months) and interviewed in August 2004 is caught because his age at interview is 65 years and 3 months but he has *not* reached his FRA yet. But person B was born in December 1938 (whose FRA is 65 years and 2 months) and interviewed in March 2004 is not caught because his age at interview is 65 years and 3 months and he *has* reached his FRA. We do not show observations on those younger than 65, who are never caught by the increase in the FRA, or those 66 or older, who – in our sample period – are never caught by the increase in the FRA. The sample used for this table includes a total of 35,023 observations, which corresponds to our employment regressions in Table 3 and subsequent tables. Note that some interviews are in odd-numbered years that do not correspond exactly to the even-numbered-year HRS waves.

Table 3: Effects of Being Caught by Increase in Full Retirement Age on Social Security Claiming and Employment between Age 65 and the Full Retirement Age and at Ages 62-65

	(1)	(2)	(3)
	Claiming SS benefits	Employment (full-time)	Any employment
Caught by increase in FRA × Age ≥ 65	-0.219 (0.024)	0.043 (0.025)	0.028 (0.027)
Caught by increase in FRA × (Age ≥ 62 and < 65)	-0.051 (0.016)	0.001 (0.014)	0.008 (0.014)
61 and 6 or 7 months	0.101 (0.020)	-0.126 (0.024)	-0.100 (0.023)
61 and 8 or 9 months	0.065 (0.018)	-0.145 (0.025)	-0.119 (0.023)
61 and 10 or 11 months	0.067 (0.019)	-0.181 (0.025)	-0.165 (0.025)
62 and 0 or 1 month	0.221 (0.025)	-0.223 (0.027)	-0.186 (0.025)
62 and 2 or 3 months	0.489 (0.028)	-0.288 (0.028)	-0.213 (0.027)
62 and 4 or 5 months	0.474 (0.028)	-0.278 (0.028)	-0.211 (0.027)
64 and 6 or 7 months	0.627 (0.026)	-0.403 (0.026)	-0.295 (0.027)
64 and 8 or 9 months	0.633 (0.026)	-0.369 (0.026)	-0.263 (0.027)
64 and 10 or 11 months	0.659 (0.024)	-0.349 (0.026)	-0.279 (0.027)
65 and 0 or 1 month	0.756 (0.023)	-0.454 (0.025)	-0.347 (0.028)
65 and 2 or 3 months	0.788 (0.022)	-0.427 (0.026)	-0.316 (0.028)
65 and 4 or 5 months	0.853 (0.018)	-0.435 (0.025)	-0.311 (0.028)
R ²	0.617	0.283	0.254
Sample size	34,059	35,023	35,023

Note: The retirement variable is equal to 1 if an individual is collecting Social Security benefits and 0 otherwise. The employment variable is equal to 1 if an individual has a full-time job (second column) or any job (third column) and 0 otherwise. The variable “Caught by increase in FRA” is explained in notes to Table 2 for those aged 65 and over. For those aged 62-65, it is equal to 1 if they are observed at ages 62-65 and their FRA is greater than 65, and 0 otherwise. All specifications include dummy variables for age in months (by two-month increments), state dummy variables, and individual level dummy-variable controls for urban or rural residence, race, marital status, education level, and self-reported health. The omitted age group is individuals 60 years old or younger; age dummy variables are included for all other ages, but only some (around ages 62 and 65) are shown. Urban-rural status includes urban, suburban, or ex-urban residence; race includes white, black, and other; marital status includes married and married with spouse absent, partnered, separated/divorced/widowed, and never married; education includes less than high school, GED or high school graduate, some college, and college and above; self-reported health includes excellent, very good, good, fair, or poor. The linear probability model is used for estimation and the standard errors reported in parentheses are clustered at the individual level. The sample period for this analysis is from 1992 to 2008. The HRS data for timing of the start of collecting Social Security benefits and employment status are sometimes missing, which is why the sample sizes differ. We restrict the sample to males born 1931-1943.

Table 4: Effects of Being Caught by Increase in Full Retirement Age on Social Security Claiming and Employment between Age 65 and the Full Retirement Age and at Ages 62-65, with Variations for 62-65 Year-Olds

	(1)	(2)	(3)	(4)	(5)	(6)
	Claiming SS benefits		Employment (full-time)		Any employment	
Caught by increase in FRA × Age ≥ 65	-0.219 (0.024)	-0.219 (0.024)	0.043 (0.025)	0.043 (0.025)	0.028 (0.027)	0.028 (0.027)
Caught by increase in FRA × (Age ≥ 62 and < 63)	-0.051 (0.022)	...	0.012 (0.021)	...	-0.003 (0.021)	...
Caught by increase in FRA × (Age ≥ 63 and < 64)	-0.038 (0.021)	...	-0.003 (0.020)	...	0.0003 (0.020)	...
Caught by increase in FRA × (Age ≥ 64 and < 65)	-0.064 (0.022)	...	-0.005 (0.021)	...	0.028 (0.022)	...
(Age ≥ 62 and < 65) ×						
Caught by 2 or 4 month increase in FRA after age 65	...	-0.014 (0.020)	...	-0.014 (0.019)	...	-0.022 (0.020)
Caught by 6 or 8 month increase in FRA after age 65	...	-0.063 (0.021)	...	0.029 (0.020)	...	0.039 (0.020)
Caught by 10 or 12 month increase in FRA after age 65	...	-0.094 (0.027)	...	-0.022 (0.025)	...	0.007 (0.025)
R ²	0.617	0.618	0.283	0.283	0.254	0.254

Note: Except for the variables shown, the specifications and estimation are identical to those in Table 3. Sample sizes are as in Table 3.

Table 5: Age Discrimination Laws 1992 and 2008

	Firm size (number of employees)		Compensatory/punitive damages		Statute of limitations (days)		Attorneys' fees	
Federal	20	20	Does not allow compensatory or punitive damages (only liquidated damages are allowed)		180 days; 300 days if there is a state age discrimination law and enforcing agency		Allows attorneys' fees to be recovered	
	1992	2008	1992	2008	1992	2008	1992	2008
Alabama	No law	20	No law	No	No law	180	No law	Yes
Alaska	1	1	Yes	No	Unknown	Not specified	Yes	Yes
Arizona	15	15	Yes	Yes	180	180	Yes	Yes
Arkansas	No law	No law	No law	No law	No law	No law	No law	No law
California	5	5	Yes	Yes	365	365	Yes	Yes
Colorado	1	1	No	No	180	180	No	No
Connecticut	3	3	No	No	180	180	No	No
Delaware	4	4	Unknown	Yes	120	120	Unknown	Yes
District of Columbia	Unknown	1	Unknown	Yes	Unknown	365	Unknown	Yes
Florida	15	15	Yes	Yes	365	365	Yes	Yes
Georgia	1	1	Unknown	No	180	180	Yes	Yes
Hawaii	1	1	Yes	Yes	180	180	Yes	Yes
Idaho	5	5	Yes	Yes	365	365	No	No
Illinois	15	15	Unknown	Yes	180	180	Yes	Yes
Indiana	1	1	No	No	120	120	No	No
Iowa	4	4	Yes	Yes	180	300	Yes	Yes
Kansas	4	4	Yes	Yes	180	180	Unknown	Yes
Kentucky	8	8	Yes	Yes	180	180	Yes	Yes
Louisiana	8	1	Yes	Yes	180	365	Yes	Yes
Maine	1	1	Yes	Yes	180	180	Yes	Yes
Maryland	Unknown	15	Unknown	Yes	Unknown	180	Yes	Yes
Massachusetts	6	6	No	No	180	300	Yes	Yes
Michigan	1	1	Yes	Yes	180	180	Yes	Yes
Minnesota	1	1	Yes	Yes	365	365	Yes	Yes
Mississippi	No law	No law	No law	No law	No law	No law	No law	No law
Missouri	6	6	Yes	Yes	180	180	Yes	Yes
Montana	1	1	Unknown	Yes	180	180	Yes	Yes
Nebraska	25	20	No	No	300	300	Yes	Yes
Nevada	15	15	No	No	180	300	Yes	Yes
New Hampshire	6	6	Yes	Yes	180	180	Yes	Yes
New Jersey	1	1	Yes	Yes	180	180	Yes	Yes

	Firm size (number of employees)		Compensatory/punitive damages		Statute of limitations (days)		Attorneys' fees	
	1992	2008	1992	2008	1992	2008	1992	2008
New Mexico	4	4	Unknown	Yes	180	300	Unknown	Yes
New York	4	4	Yes	Yes	365	365	No	No
North Carolina	15	15	No	No	Not specified	Not specified	Yes	Yes
North Dakota	1	1	No	No	300	300	Yes	Yes
Ohio	4	4	Yes	Yes	180	180	Yes	Yes
Oklahoma	15	15	No	No	180	180	Yes	Yes
Oregon	1	1	Unknown	Yes	365	365	Unknown	Yes
Pennsylvania	4	4	Yes	Yes	180	180	Yes	Yes
Rhode Island	4	4	Yes	Yes	Unknown	365	Yes	Yes
South Carolina	15	15	No	No	180	180	No	No
South Dakota	No law	No law	No law	No law	No law	No law	No law	No law
Tennessee	8	8	Yes	Yes	180	180	Yes	Yes
Texas	15	15	No	Yes	180	180	Yes	Yes
Utah	15	15	No	No	180	180	Yes	Yes
Vermont	1	1	No	Yes	Unknown	365	Yes	Yes
Virginia	5	5	No	No	180	180	Unknown	Yes
Washington	8	8	Yes	Yes	180	180	Yes	Yes
West Virginia	12	12	No	No	180	365	Yes	Yes
Wisconsin	1	1	No	No	300	300	Unknown	Yes
Wyoming	2	2	Yes	Yes	90	180	Unknown	Yes

Notes: "No Law" indicates there is no state age discrimination law; "Unknown" means we were not able to trace back the history of the statute; "Not Specified" indicates that the relevant dimension of the law was not specified under the state age discrimination law. In the empirical analysis, given that there was little time variation within states, we artificially backfilled the information for the earlier years for the "Unknown" cases. For "Not specified" cases, we dropped observations, as there is no basis on which to fill in the missing information, and "Not Specified" does not necessarily imply either a stronger or a weaker state law. The state age discrimination law in Alabama was first enacted in 1997. For Virginia, the statute bars age discrimination in discharge only, for employers with 5-14 employees, which would appear to allow a gap in coverage between the state and federal law for employers with 15-19 employees. Because discharges are an important source of age discrimination claims (Neumark, 2008), and because we are doubtful that this narrow size range is de facto exempt from the state law, we simply treat Virginia as having a firm-size cutoff of 5 employees. In the "Statute of limitations" columns, the statute of limitations under *state* law is listed; when there is a state law (and a fair employment practices agency or commission) workers in the state have 300 days to file under federal law. California's statute of limitations may be extended by an additional 90 days to 3 years under certain circumstances listed in the statute. Under "Compensatory/punitive damages," "Yes" indicates that the state allows compensatory and/or punitive damages either with or without proof of intent, and "No" indicates otherwise. In North Carolina, individuals cannot file lawsuits under a state anti-discrimination law, but they can file a "public policy" claim in court (see http://www.workplacefairness.org/age_minimum?agree=yes#NC, viewed March 17, 2011). In some states, other forms of monetary damages can be imposed. For example, in Pennsylvania as of 1992, civil penalties from \$10,000 to \$50,000 could be imposed. In Maine as of 1992 civil penal damages from \$10,000 to \$50,000 could be imposed. In 2007, these limits were increased to \$20,000 to \$100,000 for firms with 14 or fewer employees, and compensatory or punitive damages were introduced for employers with more than 14 employees. Although civil penalties or civil penal damages differ from compensatory or punitive damages, for the purposes of our analysis we treated this case as having the stronger remedies otherwise implied by compensatory/punitive damages.

Table 6A: Coding of State Age Discrimination Laws

Variable	Coding for state	Federal law
Lower firm size	1 if state law is applicable to firms with fewer than 10 employees, 0 otherwise	ADEA covers firms with 20 or more employees
Stronger remedies	1 if state law allows compensatory and/or punitive damages either with or without proof of intent, 0 otherwise	ADEA allows back pay and benefits; it doubles this amount (“liquidated damages”) if there is willful violation
Longer statute of limitations	1 if state law allows a filing period longer than ADEA – specifically, if the state law allows longer than 300 days to file a claim and it has its own enforcement agency, 0 otherwise	Filing period for states without a law is 180 days, and 300 days for states with a state law and enforcement agency
Attorneys’ fees	1 if state law allows to recover attorney’s fees, 0 otherwise	ADEA awards a reasonable attorneys’ fees and costs of the action

Notes: Additional details on the coding are given in the notes to Table 5. Note that the states with no law are coded as 0 in the second column of this table, as are the states that have a state law but do not offer the stronger protection.

Table 6B: Means of Coded State Age Discrimination Law Variables by HRS Wave Year

Variable	1992	1994	1996	1998	2000	2002	2004	2006	2008
Lower firm size (<i>N</i> =51)	0.686 (0.469)	0.686 (0.469)	0.686 (0.469)	0.667 (0.476)	0.686 (0.469)	0.686 (0.469)	0.686 (0.469)	0.686 (0.469)	0.686 (0.469)
Stronger remedies (<i>N</i> =51)	0.569 (0.500)	0.588 (0.497)	0.588 (0.497)	0.608 (0.493)	0.627 (0.488)	0.627 (0.488)	0.627 (0.488)	0.627 (0.488)	0.627 (0.488)
Longer statute of limitations (<i>N</i> =49)	0.183 (0.391)	0.204 (0.407)	0.204 (0.407)	0.204 (0.407)	0.224 (0.422)	0.224 (0.422)	0.224 (0.422)	0.224 (0.422)	0.224 (0.422)
Attorneys' fees (<i>N</i> =51)	0.824 (0.385)	0.824 (0.385)	0.824 (0.385)	0.824 (0.385)	0.824 (0.385)	0.824 (0.385)	0.824 (0.385)	0.824 (0.385)	0.824 (0.385)

Note: Standard deviations are reported in parentheses. We do not have information on statute of limitations for Alaska and North Carolina (Table 5).

Table 6C: States with Time Variation in Age Discrimination Laws in Sample Period (1992-2008)

Lower firm size	AL – no law to 20 in 1997; NE – Decreased from 25 to 20 in 2007; LA – Increased from 8 to 20 in 1997. Decreased to 1 in 1999.
Stronger remedies	AK – 1997; TX – 1993; VT – 1999
Longer statute of limitations	LA – Increased from 180 to 365 in 1999; WV – Increased from 180 to 365 in 1994.
Attorneys' fees	AL – 1997 with enactment of law

Table 7: Effects of State Age Discrimination Laws on Impact of Being Caught by Increase in Full Retirement Age on Social Security Claiming and Employment between Age 65 and the Full Retirement Age and at Ages 62-65

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Claiming SS benefits				Employment (full-time)				Any employment			
Caught by increase in FRA × Age ≥ 65 × Lower firm size (< 10)	-0.027 (0.048)	-0.009 (0.051)	0.121 (0.049)	0.094 (0.052)	0.140 (0.053)	0.120 (0.055)
Stronger remedies	...	-0.104 (0.051)	...	-0.117 (0.058)	...	0.127 (0.051)	...	0.126 (0.060)	...	0.116 (0.056)	...	0.072 (0.064)
Longer statute of limitations	-0.027 (0.050)	0.012 (0.054)	-0.005 (0.051)	-0.043 (0.056)	0.035 (0.056)	0.009 (0.061)
Caught by increase in FRA × (Age ≥ 62 and < 65) × Lower firm size (<10)	-0.015 (0.029)	-0.011 (0.030)	-0.007 (0.026)	-0.019 (0.027)	-0.033 (0.026)	-0.037 (0.027)
Stronger remedies	...	-0.044 (0.030)	...	-0.053 (0.033)	...	0.072 (0.027)	...	0.075 (0.030)	...	0.004 (0.027)	...	0.013 (0.030)
Longer statute of limitations	-0.004 (0.030)	0.012 (0.032)	0.028 (0.027)	0.001 (0.029)	-0.008 (0.028)	-0.014 (0.030)
<i>Main effects, caught by increase in FRA ×:</i>												
Age ≥ 65	-0.184 (0.036)	-0.127 (0.042)	-0.188 (0.030)	-0.112 (0.047)	-0.034 (0.036)	-0.053 (0.042)	0.039 (0.031)	-0.095 (0.048)	-0.064 (0.040)	-0.062 (0.047)	0.009 (0.033)	-0.107 (0.054)
Age ≥ 62 and < 65	-0.019 (0.022)	-0.001 (0.024)	-0.028 (0.017)	0.011 (0.030)	-0.004 (0.020)	-0.056 (0.022)	-0.017 (0.016)	-0.047 (0.028)	0.016 (0.020)	-0.006 (0.022)	-0.002 (0.016)	0.015 (0.028)
<i>Combined effects, interaction plus main effect:</i>												
Age ≥ 65	-0.210 (0.031)	-0.231 (0.028)	-0.214 (0.040)	...	0.087 (0.032)	0.074 (0.029)	0.034 (0.041)	...	0.076 (0.035)	0.054 (0.032)	0.044 (0.045)	
Age ≥ 62 and < 65	-0.034 (0.018)	-0.045 (0.017)	-0.032 (0.025)	...	-0.011 (0.016)	0.016 (0.015)	0.011 (0.022)	...	-0.017 (0.016)	-0.002 (0.016)	-0.010 (0.023)	
R ²	0.618	0.619	0.619	0.621	0.285	0.285	0.285	0.289	0.256	0.256	0.257	0.260

Note: All specifications include dummy variable for age in months (by two-month increments), interactions between these age dummy variables and the age discrimination law features included in the column, state dummy variables, and the individual-level controls for urban-rural, race, marital status, education level, self-reported status described in the note to Table 3. See Tables 5 and 6A for information on state age discrimination laws. OLS estimates of linear probability models are reported with standard errors, reported in parentheses, clustered at the individual level. The combined effects are not reported for columns (4), (8), and (12). HRS restricted data with state identifiers are used. Sample sizes are as in Table 3.

Table 8: Effects of State Age Discrimination Laws on Impact of Being Caught by Increase in Full Retirement Age on Social Security Claiming and Employment between Age 65 and the Full Retirement Age and at Ages 62-65, with Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Claiming SS benefits				Employment (full-time)				Any employment			
Caught by increase in FRA × Age ≥ 65 ×												
Lower firm size (< 10)	-0.023 (0.046)	-0.006 (0.049)	0.124 (0.048)	0.100 (0.051)	0.126 (0.052)	0.106 (0.054)
Stronger remedies	...	-0.110 (0.049)	...	-0.115 (0.057)	...	0.131 (0.050)	...	0.127 (0.058)	...	0.124 (0.055)	...	0.084 (0.063)
Longer statute of limitations	-0.036 (0.049)	0.0002 (0.053)	-0.008 (0.050)	-0.044 (0.055)	0.025 (0.054)	-0.005 (0.059)
Caught by increase in FRA × (Age ≥ 62 and < 65) ×												
Lower firm size (< 10)	-0.010 (0.029)	-0.005 (0.030)	-0.010 (0.026)	-0.022 (0.027)	-0.042 (0.026)	-0.047 (0.027)
Stronger remedies	...	-0.046 (0.030)	...	-0.055 (0.033)	...	0.074 (0.027)	...	0.079 (0.030)	...	0.012 (0.027)	...	0.026 (0.030)
Longer statute of limitations	-0.007 (0.030)	0.009 (0.032)	0.030 (0.027)	0.001 (0.029)	-0.008 (0.028)	-0.019 (0.030)
<i>Main effects, caught by increase in FRA ×:</i>												
Age ≥ 65	-0.181 (0.035)	-0.117 (0.041)	-0.179 (0.029)	-0.105 (0.046)	-0.040 (0.035)	-0.062 (0.040)	0.034 (0.030)	-0.105 (0.047)	-0.056 (0.039)	-0.071 (0.045)	0.008 (0.032)	-0.105 (0.054)
Age ≥ 62 and < 65	-0.023 (0.022)	0.001 (0.024)	-0.026 (0.017)	0.010 (0.030)	-0.006 (0.020)	-0.061 (0.022)	-0.022 (0.016)	-0.052 (0.028)	0.017 (0.020)	-0.015 (0.022)	-0.008 (0.016)	0.009 (0.028)
<i>Combined effects, interaction plus main effect:</i>												
Age ≥ 65	-0.205 (0.031)	-0.227 (0.028)	-0.215 (0.039)	...	0.084 (0.032)	0.070 (0.029)	0.026 (0.040)	...	0.069 (0.034)	0.053 (0.031)	0.033 (0.043)	...
Age ≥ 62 and < 65	-0.032 (0.018)	-0.045 (0.017)	-0.034 (0.025)	...	-0.016 (0.016)	0.013 (0.015)	0.008 (0.022)	...	-0.025 (0.016)	-0.004 (0.016)	-0.015 (0.023)	...
R ²	0.619	0.620	0.620	0.623	0.287	0.287	0.288	0.293	0.258	0.257	0.259	0.264

Note: Note from Table 7 applies, except that all specifications also include dummy variables for birth cohort (by year) and interactions between birth-cohort dummy variables and the age discrimination law feature(s) included in the column.

Table 9: Effects of Different Firm-Size Cutoffs in State Age Discrimination Laws on Impact of Being Caught by Increase in Full Retirement Age on Employment and Any Employment between Age 65 and the Full Retirement Age and at Ages 62-65

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment (full-time)				Any employment			
Caught by increase in FRA × Age ≥ 65 × Lower firm size (< 10)	0.121 (0.049)	...	0.094 (0.052)	...	0.140 (0.053)	...	0.120 (0.055)	...
Lower firm size (< 15)	...	0.091 (0.049)	...	0.077 (0.051)	...	0.099 (0.053)	...	0.090 (0.055)
Stronger remedies	0.126 (0.060)	0.139 (0.058)	...	-0.037 (0.026)	0.072 (0.064)	0.090 (0.063)
Longer statute of limitations	-0.043 (0.056)	-0.052 (0.056)	0.009 (0.061)	-0.001 (0.061)
Caught by increase in FRA × (Age ≥ 62 and < 65) × Lower firm size (< 10)	-0.007 (0.026)	...	-0.019 (0.027)	...	-0.033 (0.026)	...	-0.037 (0.027)	-0.040 (0.027)
Lower firm size (< 15)	...	-0.014 (0.026)	...	-0.022 (0.027)	0.013 (0.030)	0.010 (0.030)
Stronger remedies	0.075 (0.030)	0.074 (0.030)	-0.014 (0.030)	-0.010 (0.030)
Longer statute of limitations	0.001 (0.029)	0.004 (0.029)	-0.014 (0.030)	-0.010 (0.030)
<i>Main effects, caught by increase in FRA ×:</i>								
Age ≥ 65	-0.034 (0.036)	-0.019 (0.038)	-0.095 (0.048)	-0.093 (0.050)	-0.064 (0.040)	-0.042 (0.041)	-0.107 (0.054)	-0.100 (0.056)
Age ≥ 62 and < 65	-0.004 (0.020)	0.0003 (0.021)	-0.047 (0.028)	-0.044 (0.030)	0.016 (0.020)	0.019 (0.021)	0.015 (0.028)	0.019 (0.029)
<i>Combined effects, interaction plus main effect:</i>								
Age ≥ 65	0.087 (0.032)	0.072 (0.032)	0.076 (0.035)	0.057 (0.034)
Age ≥ 62 and < 65	-0.011 (0.016)	-0.014 (0.016)	-0.017 (0.016)	-0.018 (0.016)
R ²	0.285	0.285	0.289	0.289	0.256	0.256	0.260	0.260

Note: Note from Table 7 applies.

Table 10: Influence of Recoverability of Attorneys' Fees on Effects of State Age Discrimination Laws on Impact of Being Caught by Increase in Full Retirement Age on Social Security Claiming and Employment between Age 65 and the Full Retirement Age and at Ages 62-65

	(1)	(2)	(3)	(4)	(5)	(6)
	Claiming SS benefits		Employment (full- time)		Any employment	
Caught by increase in FRA × Age ≥ 65 ×						
Lower firm size (< 10) × Attorneys' fees recoverable	-0.025 (0.076)	...	0.061 (0.068)	...	0.021 (0.079)	...
Lower firm size (< 10)	-0.005 (0.080)	...	0.069 (0.074)	...	0.122 (0.086)	...
Sum (effect of firm size in states with recoverable fees)	-0.030 (0.049)	...	0.130 (0.050)	...	0.143 (0.054)	...
Stronger remedies × Attorneys' fees recoverable	...	-0.013 (0.094)	...	0.082 (0.077)	...	0.062 (0.096)
Stronger remedies	...	-0.092 (0.100)	...	0.052 (0.086)	...	0.059 (0.106)
Sum (effect of remedies in states with recoverable fees)	...	-0.105 (0.051)	...	0.134 (0.052)	...	0.121 (0.057)
Caught by increase in FRA × (Age ≥ 62 and < 65) ×						
Lower firm size (< 10) × Attorneys' fees recoverable	-0.003 (0.034)	...	0.036 (0.031)	...	0.003 (0.032)	...
Lower firm size (< 10)	-0.013 (0.040)	...	-0.036 (0.036)	...	-0.036 (0.037)	...
Sum	-0.016 (0.029)	...	-0.0003 (0.027)	...	-0.032 (0.027)	...
Stronger remedies × Attorneys' fees recoverable	...	-0.026 (0.047)	...	0.071 (0.042)	...	0.057 (0.044)
Stronger remedies	...	-0.020 0.052	...	0.007 (0.046)	...	-0.048 (0.049)
Sum	...	-0.046 (0.030)	...	0.078 (0.027)	...	0.009 (0.027)
R ²	0.618	0.619	0.285	0.285	0.256	0.256

Note: See notes to Table 7. Besides the coefficients reported in the table, all specifications include the same controls as in Table 7 (for the corresponding column), plus the interaction of the dummy variable for the age discrimination law feature including in the column and the dummy variable for recoverability of attorneys' fees, and the interactions of this product with *IFRA*, and with the age dummy variables in two-month increments. The main effects and combined effects (see Tables 7-9) are not reported.

Appendix Table A: HRS Summary Statistics

	Retirement (collecting SS benefits) regression sample		Employment (full-time) regression sample		Any employment	
	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.
<i>Dependent variables:</i>						
Retirement (Collecting SS)	0.494	0.500
Employment	0.418	0.493	0.565	0.496
<i>Independent variables:</i>						
Caught by increase in FRA	0.013	0.112	0.013	0.112	0.013	0.112
High school	0.348	0.476	0.347	0.476	0.347	0.476
Some college	0.192	0.394	0.192	0.394	0.192	0.394
College and above	0.224	0.417	0.222	0.416	0.222	0.416
Very good health condition	0.292	0.454	0.290	0.455	0.290	0.455
Good health condition	0.309	0.462	0.309	0.462	0.309	0.462
Fair health condition	0.167	0.373	0.169	0.375	0.169	0.375
Poor health condition	0.078	0.268	0.080	0.271	0.080	0.271
Partnered	0.032	0.177	0.033	0.179	0.033	0.179
Separated/divorced/widowed	0.136	0.343	0.137	0.344	0.137	0.344
Single	0.030	0.171	0.031	0.173	0.031	0.173
Black	0.138	0.345	0.141	0.349	0.141	0.349
Other race	0.035	0.185	0.036	0.187	0.036	0.187
Suburban	0.268	0.443	0.268	0.443	0.268	0.443
Ex-urban	0.312	0.463	0.311	0.463	0.311	0.463
Sample size	34,059		35,023		35,023	