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Do stronger age discrimination laws make Social Security reforms more effective?☆

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ABSTRACT

Supply-side Social Security reforms intended to increase employment and delay benefit claiming among older individuals may be frustrated by age discrimination. We test for policy complementarities between these reforms and demand-side efforts to deter age discrimination, specifically studying whether stronger state-level age discrimination protections enhanced the impact of the 1983 Social Security reforms that increased the full retirement age (FRA) and reduced benefits. The evidence indicates that, for older individuals for whom early retirement benefits fell and the FRA increased, stronger state age discrimination protections were associated with delayed benefit claiming and increases in employment, with benefit claiming pushed from 65 to the new FRA, and increased employment after age 62 and age 65 that is then curtailed at the new FRA.

1. Introduction

In coming decades the share of the population aged 65 and over ("seniors") will rise sharply – from 17% of those aged 20 and over in 2000, to 28% 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: reforms that lowered benefits at the early retirement age of 62 and raised the full retirement age (FRA) at which full benefits are available from 65 to 67 beginning with the 1938 birth cohort that 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.

Efforts to delay Social Security claiming and retirement of older workers, however, may be frustrated by age discrimination. In particular, 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. Research shows that the federal Age Discrimination in Employment Act (ADEA) and state age discrimination laws have increased employment of protected workers (Neumark and Stock, 1999; Adams, 2004). This motivates the key question this paper addresses — whether there are policy complementarities between supply-side efforts to increase labor supply and demand-side efforts to deter age discrimination. Specifically, we study whether stronger age discrimination protections at the *state* level enhanced the impact – in terms of delaying claiming Social Security benefits and encouraging continued employment – of the 1983 Social Security reforms that took effect in the last decade, increasing the FRA and reducing benefits when they are claimed before the FRA. State-level variation in age discrimination laws allows us to compare responses to these reforms in states with different age discrimination protections.

It might be natural to expect this kind of positive complementarity, but the reality is more complex. There is evidence suggesting that age discrimination remains pervasive, especially with regard to hiring older workers (e.g., Adams, 2002, 2004; Bendick et al., 1996, 1999;

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Hirsch et al., 2000; Hutchens, 1988; Johnson and Neumark, 1997; Kite et al., 2005; Lahey, 2008a).[1] Because in hiring cases it is difficult to identify a class of affected workers, and economic damages are smaller than in termination cases, age discrimination laws may not be effective in combating discrimination in hiring. And if age discrimination laws mainly raise the costs of terminating older workers, they could end up deterring hiring (Bloch, 1994; Lahey, 2008b; Posner, 1995).[2]

In this scenario, given that a good share of increased employment among seniors might be expected to come 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), age discrimination laws might not enhance the effects of the Social Security reforms.

2. The potential effects of the 1983 Social Security reforms on benefit claiming and employment

The basic empirical strategy is to ask whether the Social Security reforms that lowered benefits at the early retirement age of 62 and raised the full retirement age (FRA) at which full benefits are available had stronger effects on claiming or employment where state age discrimination laws provide greater protections to older workers. The strategy therefore rests on the effects of the Social Security reforms on 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 1983 Social Security reforms implemented increases in the FRA starting with people born in 1938 or later (Svahn and Ross, 1983). Beginning with this cohort, the benefits available at the early retirement age of 62 were reduced, and the FRA – when full benefits were payable – was slated to gradually increase by two months per birth year until it reaches 67. The sample period we study covers most of the first round of phased increases in the FRA to 66.

The implications for benefits of this first round of changes are shown in Table 1, beginning with the cohort born in 1931 (the oldest in our sample) and ending with the 1943 cohort (the youngest in our sample). Column (2) shows the FRA for each cohort, and column (3) converts this into months after age 62. Column (4) shows the reduction in benefits when they are claimed before the FRA. Through the 1937 birth cohort, the reduction is 0.556% of the Primary Insurance Amount (PIA) for each month, implying a 20 percent reduction for claiming at age 62 versus the FRA. For subsequent cohorts, there is a reduction of 0.556% of the PIA for each month prior to the FRA up to 36 months, and then an additional reduction of 0.417% per month for each month earlier than the FRA minus 36 months. Thus, for example, for the 1938 birth cohort the reduction for claiming at age 62 is 20.83%, climbing to 25% for the 1943 birth cohort. Note that this implies a slight convex kink in the budget constraint at 36 months prior to the FRA for the affected cohorts. Column (5) then shows the increase in benefits for claiming benefits after the FRA (the delayed retirement credit, or DRC). This increases over the cohorts considered. Through the 1938 cohort the DRC creates a concave kink at the FRA (very slight by the time we get to the later cohorts); beginning with the 1939 cohort the kink at the FRA becomes convex.

These changes in Social Security benefit computations and the FRA can influence decisions about when to claim benefits and when to stop working.[3] The most clear-cut effect of the changes in Social Security benefits from the point of view of the standard theory of labor supply is the reduction in the expected discounted value of Social Security benefits, which should exert a negative income effect, assuming that leisure is a normal good. This will lead to later retirement, and presumably also later claiming. Given the widely-documented spike in benefit claiming and labor force exit at age 62 – usually attributed to liquidity constraints – the impact of the cut in benefits might be most apparent for those aged 62.

In contrast, the changes in the FRA and in the benefit computation around the FRA do not create any economic reason for sharp changes in behavior around the FRA, based on standard labor supply considerations. As Table 1 shows, roughly coincident with the increase in the FRA, the concave kink at the FRA was eliminated, which could reduce clustering of claiming and retirement at age 65. But there is no simple economic reason for those delaying claiming or retirement to cluster at the new FRA. Then again, as many researchers have pointed out – perhaps most recently, Behaghel and Blau (2012) – it has always been difficult to explain the spike in claiming and retirement at age 65 other than through appealing to the FRA as a norm that many people follow, for behavioral economics reasons such as a social norm or a reference point for agents with loss aversion and reference dependence.[4] Behaghel and Blau also develop a stylized labor supply model that can be interpreted as a model of lifetime labor supply that shows explicitly that reference dependence and loss aversion with a norm of retirement at the FRA generates a spike at the FRA, and predicts that an increase in the FRA will raise the average age of retirement.[5] Indeed, because there are some economic reasons why cohorts unaffected by the increase in the FRA may have clustered at age 65 – including the kink in the budget constraint, defined benefit pension rules, and Medicare – but no reason to expect affected cohorts to cluster at the new FRA, Behaghel and Blau's study tests for such clustering as demonstrating that behavioral factors are an important reason why people claim and retire at the FRA.[6] Their key empirical result is that for cohorts affected by the increase in the FRA, the spike in claiming (and the smaller spike in employment) shifted from age 65 to the new FRA; the claiming results are echoed in Song and Manchester (2007).

Based on theoretical considerations – including behavioral ones – and these results for cohorts affected by the Social Security reforms, in exploring how stronger state age discrimination protections influenced responses to the Social Security reforms, we focus on how these protections influenced changes in claiming and employment behavior at or near age 62, age 65 and the FRA. Given that the empirical strategy rests on the effects of increases in the FRA on Social Security benefit

[1] The evidence is not cut and dried, however. See Neumark (2008) for a thorough review.

[2] This argument about discrimination laws deterring hiring has been made generally with respect to anti-discrimination laws, and it has perhaps appeared natural to assume that it applies to older workers as well (e.g., Lahey, 2008b). The argument may, however, have less force for older workers. Even if age discrimination laws increase termination costs, such costs may not weigh heavily in employers' decisions because many older workers may not plan on remaining at the employer for very long.

[3] This discussion closely follows Behaghel and Blau (2012). They also depict graphically some of the same budget constraint features documented in Table 1.

[4] Indeed Behaghel and Blau (2012) discuss ways in which the Social Security Administration's framing of the FRA as well as the advice other groups (like AARP) provide can reinforce the FRA as the norm. As an example, even after the FRA increased above age 65 the FRA was described this way in personalized Social Security statements; for the 1939 birth cohort, for example, the statements say "The earliest age at which you can receive an unreduced retirement benefit is 65 and 4 months" (quoted in Behaghel and Blau, footnote 10).

[5] In contrast to the predicted changes in behavior from liquidity constraints and norm or reference effects, in a model with perfect foresight (so that the reforms are taken into account in choosing a utility-maximizing life-cycle profile of labor supply and retirement) and no liquidity constraints (so that all that matters is the present discounted value of Social Security benefits) – such as in Laitner and Silverman (2012) – there is no reason to expect spikes in retirement at age 62 or the FRA, or, consequently, changes in the behavior at age 62 or the old or new FRA in response to the reforms. Rather, there are just income effects and substitution effects from changes in the present value of benefits and the marginal taxation of earnings. (See the discussion of retirement at age 62 in. e.g., Kahn (1988), and the discussion of retirement at the FRA in Behaghel and Blau (2012).)

[6] There were changes in the earnings test in 2000, after which it only applied to those between age 62 and their FRA (Pingle, 2006). This can generate incentives to delay claiming benefits to the FRA for those who would be subject to the test (and view this as a tax, not realizing that benefits are increased later to make up for the tax). However, because this change affected some cohorts for which the FRA remained 65, it is possible to test separately for the effects of the elimination of the earnings test at the FRA and increases in the FRA, and Behaghel and Blau (2012, Table 1) show that the change in the earnings test does not account for the spikes in claiming and in exiting employment at the FRA.

Table 1
Effects of 1983 Social Security reforms on benefits and the full retirement age (FRA).
Sources: U.S. Social Security Administration (www.ssa.gov/oact/progdata/nra.html, viewed March 11, 2011); http://www.socialsecurity.gov/policy/docs/statcomps/supplement/2012/apnd.pdf (viewed May 29, 2013); and Title 42, Section 402, U.S. Code.

(1)	(2)	(3)	(4)	(5)	(6)
Year of birth	FRA	FRA in months after age 62	Reduction in PIA for claiming before FRA or at FRA	Increase in PIA for claiming after FRA	Kink at FRA
1931	65	36	(36 − AR) × .00556	(AR − 36) × .00417	Concave
1932	65	36	(36 − AR) × .00556	(AR − 36) × .00417	Concave
1933	65	36	(36 − AR) × .00556	(AR − 36) × .00458	Concave
1934	65	36	(36 − AR) × .00556	(AR − 36) × .00458	Concave
1935	65	36	(36 − AR) × .00556	(AR − 36) × .005	Concave
1936	65	36	(36 − AR) × .00556	(AR − 36) × .005	Concave
1937	65	36	(36 − AR) × .00556	(AR − 36) × .00542	Concave
1938	65 + 2 months	38	Claim ≤ 62 and 2 months: (36 − AR) × .00556 + (2 − AR) × .00417 Claim > 62 and 2 months: (38 − AR) × .00556	(AR − 38) × .00542	Concave
1939	65 + 4 months	40	Claim ≤ 62 and 4 months: (36 − AR) × .00556 + (4 − AR) × .00417 Claim > 62 and 4 months: (40 − AR) × .00556	(AR − 40) × .00583	Convex
1940	65 + 6 months	42	Claim ≤ 62 and 6 months: (36 − AR) × .00556 + (6 − AR) × .00417 Claim > 62 and 6 months: (42 − AR) × .00556	(AR − 42) × .00583	Convex
1941	65 + 8 months	44	Claim ≤ 62 and 8 months: (36 − AR) × .00556 + (8 − AR) × .00417 Claim > 62 and 8 months: (44 − AR) × .00556	(AR − 44) × .00625	Convex
1942	65 + 10 months	46	Claim ≤ 62 and 10 months: (36 − AR) × .00556 + (10 − AR) × .00417 Claim > 62 and 10 months: (46 − AR) × .00556	(AR − 46) × .00625	Convex
1943	66	48	Claim ≤ 62 and 12 months: (36 − AR) × .00556 + (12 − AR) × .00417 Claim > 62 and 12 months: (48 − AR) × .00556	(AR − 48) × .00667	Convex

Note: PIA = Primary Insurance Amount (benefits at FRA). AR = number of months after age 62 in which benefits claimed. Sample includes cohorts from birth year 1931 through 1943. The 1942 birth cohort is the youngest to reach the FRA in our sample period.

claiming and employment, we begin with a more limited analysis that estimates these effects, after describing the HRS data that we use.

3. 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 through 2008, which extends through the first phase of increases in the FRA.[7] The initial HRS cohorts were born from 1931 to 1941, but other 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.[8] In addition, although the sampling frame for the HRS depends on birth year, spouses of the respondents are also included, with birth years that range from 1890 to 1983. Because the respondents targeted in the original HRS cohort were aged 62–72 in 2003, the HRS data cover exactly the right ages to study the effect of first phase of increases in the FRA. We restricted our data (for almost all of our analyses) 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; in addition, we get information on earlier changes in behavior for this cohort. 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 because of the much lower labor force participation of women in the cohorts we study. Everyone born in 1929 or later needs 40 covered quarters to be eligible.[9] In 1950, the labor force participation rate of men aged 16 years and older was 86.4%, versus 33.9% for women, and by 1960 the difference had narrowed only slightly, to 83.3% 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.[10] 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.

The dependent variables we study are Social Security claiming, full-time employment, and any employment. In the HRS, we know the month in which a person started to collect Social Security benefits. We report results for full-time employment (35 h or more per week), which is, in a sense, most "opposed" to Social Security claiming, and

[7] The 2010 restricted HRS data (including the state identifiers we need to merge in state age discrimination laws, described below) were not yet available.

[8] 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.

[9] See http://www.socialsecurity.gov/retire2/credits2.htm (viewed March 17, 2011).

[10] 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.)

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). Table 2 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. For example, 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* affected by the increase in the FRA, while those observed after the FRA increased to 65 years and 4 months are affected by the increase (and they face lower benefits for early retirement). Table 3 shows that we have many observations in the HRS, subsequent to 2003 when implementation of the reforms began, on individuals over age 65 who are subject to a higher FRA. We have many more observations, of course, on those aged 62–65 who face lower benefits as a result of the reforms.

Table 2
HRS summary statistics.

	Collecting SS benefits regression sample		Employment (full-time) and any employment regression sample	
	Mean	St. dev.	Mean	St. dev.
Dependent variables:				
Claimed SS benefits	0.403	0.490	…	…
Claimed SS benefits, ages 60–61	0.153	0.360	…	…
Claimed SS benefits, age ≥ 62	0.775	0.417	…	…
Claimed SS benefits, age ≥ 62 and affected by SS reforms	0.736	0.441	…	…
Claimed SS benefits, age ≥ 65 and affected by SS reforms	0.860	0.347	…	…
Claimed SS benefits, age ≥ FRA and affected by SS reforms	0.896	0.306	…	…
Employment (full-time)	…	…	0.479	0.500
Employment (full-time), ages 60–61	…	…	0.545	0.498
Employment (full-time), age ≥ 62	…	…	0.267	0.442
Employment (full-time), age ≥ 62 and affected by SS reforms	…	…	0.287	0.452
Employment (full-time), age ≥ 65 and affected by SS reforms	…	…	0.226	0.418
Employment (full-time), age ≥ FRA and affected by SS reforms	…	…	0.215	0.411
Any employment	…	…	0.620	0.485
Any employment, ages 60–61	…	…	0.669	0.471
Any employment, age ≥ 62	…	…	0.453	0.498
Any employment, age ≥ 62 and affected by SS reforms	…	…	0.472	0.499
Any employment, age ≥ 65 and affected by SS reforms	…	…	0.419	0.494
Any employment, age ≥ FRA and affected by SS reforms	…	…	0.409	0.492
Independent variables:				
Age ≥ 62 and affected by SS reforms	0.174	0.379	0.176	0.381
Age ≥ 65 and affected by SS reforms	0.089	0.285	0.090	0.286
Age ≥ FRA and affected by SS reforms	0.074	0.262	0.075	0.263
High school	0.347	0.476	0.346	0.476
Some college	0.195	0.396	0.195	0.396
College and above	0.224	0.417	0.223	0.416
Very good health condition	0.294	0.455	0.292	0.455
Good health condition	0.306	0.461	0.306	0.461
Fair health condition	0.160	0.367	0.162	0.368
Poor health condition	0.076	0.264	0.077	0.267
Partnered	0.034	0.180	0.034	0.181
Separated/divorced/widowed	0.131	0.337	0.132	0.338
Single	0.030	0.172	0.031	0.174
Black	0.139	0.346	0.142	0.349
Other race	0.036	0.186	0.037	0.189
Suburban	0.276	0.447	0.276	0.447
Ex-urban	0.306	0.461	0.305	0.462

Notes: For the categorical demographic and other variables, all categories but one are shown. The notes to Table 4 list the full set of categories.

4. The effects of increases in the Full Retirement Age on Social Security claiming and employment

4.1. Empirical approach

As a preliminary to our main analysis, we estimate the effects of increases in the FRA and associated benefit reductions on Social Security claiming and employment behavior, without regard to whether the effects vary across states depending on their age discrimination laws — which is our main question of interest. We focus on changes at three points: age 62, age 65 (the FRA before the reforms), and the new FRA that applies to individuals depending on their birth cohort. In line with the previous discussion, we might expect changes in behavior at age 62 to the extent that there are liquidity constraints and later cohorts face lower benefits upon reaching age 62. And we might expect shifts from age 65 to the new FRA because of the reference or norm effect of the FRA.

Behaghel and Blau (2012) show that there have been shifts in claiming and in exiting employment from age 65 to the new FRA, although the latter shift is modest compared to the shift in claiming.[11] Mastrobuoni (2009) focuses on 62–65 year-olds, and finds that those who faced reduced benefits at the early retirement age retired later.[12] These papers focus only on the aggregate variation over time induced by the increase in the FRA, rather than any variation across states based on their laws. We are simply replicating the kinds of results established in the existing literature as a prelude to studying how these effects vary with the strength of age discrimination protections.[13]

We estimate linear probability models for benefit claiming and employment, with a rich set of age dummy variables, and variables capturing whether one was affected by the Social Security reforms, and, for those who were affected, allowing shifts in behavior at age 62, age 65, and the new FRA depending on one's birth cohort. The regression model is

$$R_{ist} = \alpha + \beta A62_{ist} \cdot ISSR_t + \gamma A65_{ist} \cdot ISSR_t + \delta AFRA_{ist} \cdot ISSR_t + \lambda ISSR_t + \sum_k A^k{}_{ist}\psi_k + X_{ist}\theta + \varepsilon_{ist}. \quad (1)$$

In Eq. (1), i, s, and t denote individual, year, and state. A is a detailed vector of age dummy variables in two-month cells, and X is a vector of individual-level demographic and other controls.[14] *ISSR* is a dummy variable equal to 1 for cohorts that faced an FRA higher than age 65 and lower benefits for early retirement (cohorts born 1938 and later). The control variables in X include state dummy variables, and individual level dummy-variable controls for urban or rural residence, race, marital status, education level, and self-reported health. Urban–rural status includes urban, suburban, or ex-urban residence; race includes white, black, and other; marital status includes married and married with

[11] We would expect the norm or reference effect of the FRA to be most salient with respect to claiming, while employment behavior would be driven to some extent by claiming.

[12] Pingle (2006) finds that the reforms increased labor supply among those aged 60–64, but not 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.

[13] Mastrobuoni (2009) uses CPS data rather than HRS data (which we and Behaghel and Blau use), 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.

[14] Standard errors for this specification reported in the paper are clustered at the individual level, since we are not yet using any state-level policy variation. For the main specifications introducing variation in state age discrimination laws, standard errors are clustered at the individual and state level, using non-nested clustering (Cameron et al., 2011).

Table 3
Number of individuals observed in age ranges covered by increases in full retirement age (FRA), 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	
	Affected	Not affected	Affected	Not affected	Affected	Not affected	Affected	Not affected	Affected	Not affected	Affected	Not affected
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: "Affected" indicates that respondent was observed at age 65 or above (and below age 66) and subject to an FRA beyond their 65th birthday. 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. "Not affected" denotes people who were observed in this age range but when the FRA was 65. 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 classified as "Affected" because his age at interview is 65 years and 3 months but he has not reached his FRA yet. But person B born in December 1938 (whose FRA is 65 years and 2 months) and interviewed in March 2004 is classified as "Not affected" because his age at interview is 65 years and 3 months and he has reached his FRA. The sample used for this table comes from a total of 29,330 observations, which corresponds to the sample for our employment regressions in Table 4 and subsequent tables. Note that some interviews are in odd-numbered years that do not correspond exactly to the even-numbered-year HRS waves.

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, and poor.

The key variables are the following: *A62* is a dummy variable for those aged 62 and over, *A65* is a dummy variable for those aged 65 and over, and *AFRA* is a dummy variable for those whose age is equal to or greater than their FRA. Given these definitions, *A62·ISSR*, *A65·ISSR*, and *AFRA·ISSR* capture changes in behavior at age 62, at age 65, and at the FRA, for those affected by the Social Security reforms that increased the FRA and reduced benefits.

Given that the three variables *A62*, *A65*, and *AFRA* multiplying *ISSR* in Eq. (1) are defined to equal 1 for age greater than or equal to the reference age, their coefficients identify the *shift* in behavior at each age for the affected cohorts. Thus, for example, a negative estimate of β in the equation for benefit claiming would imply a decline in early benefit claiming (which could include both a decline in the spike at age 62, and more general delays above age 62). A negative estimate of γ would imply an additional decline in claiming right around age 65. As noted earlier, we would not expect this additional effect from changes in benefits for affected cohorts or from changes in the budget constraint, but it could occur because of reference or norm effects from the increase in the FRA. And finally a positive estimate of δ in the equation for benefit claiming would point to a shift in benefit claiming to the new FRA for affected cohorts.[15]

Eq. (1) can be interpreted as embedding three differences-in-differences, one for those aged 62 and older, one for those aged 65 and older, and one for those at the FRA (which can vary) and older. The corresponding parameters – β, γ, and δ – capture the shifts in the dependent variables at each of these ages for cohorts born after the reforms began to be implemented, relative to the differences in behavior by age for cohorts unaffected by the reforms. For claiming, we expect a decline in claiming at age 62 and 65, and an increase at the FRA. And for employment we expect the opposite. The results at age 65 and the new FRA may be particularly compelling because identification of the effects of increases in the FRA on behavior at the FRA (or what was the FRA) may be cleaner given that it comes from changes in behavior across very narrow age ranges (defined in months) in nearby years, making it easier to rule out coincident changes in Social Security claiming or employment behavior by age as an explanation of our findings.

5. Results

Table 4 reports the estimates of Eq. (1) in columns (1), (3), and (6); we return to the estimates in the other columns shortly. The equation is estimated for three outcomes — claiming benefits, full-time employment, and any employment. The equation includes a full set of age dummy variables, but the table reports only those around ages 62 and 65, when sharp changes occur.

Looking first at these age dummy variables, note that there is a distinct increase in the probability of Social Security claiming at and near age 62, when people are first eligible for Social Security benefits, and at and near age 65. The declines in employment at ages 62 and 65 are less pronounced, although the declines for full-time employment are sharper than for any employment.[16] We would not expect as distinct a change for employment, as one can make a transition to receiving Social Security benefits without a change in employment status (being either non-employed in the period before and after starting to receive benefits, or employed).

Of more direct interest are the estimates for age greater than or equal to 62, 65, and the FRA, interacted with the indicator for cohorts affected by the Social Security reforms (with the first corresponding to *A62·ISSR* in Eq. (1), etc.). For benefit claiming, the estimated coefficient of −0.052 for those aged 62 or older indicates that, relative to the age profile of benefit claiming for earlier cohorts not affected by the reforms, the probability that those aged 62 or older have claimed benefits is lower by 5.2 percentage points. The estimates at the bottom of the table suggest an increase in the probability of claiming of around 0.4 at or near age 62, so the −0.052 estimate implies the probability of claiming in this age range drops by about one-eighth.

The estimates point to a much larger drop at age 65, of 17.6 percentage points. Relative to the increase in claiming probability around age 65 for older cohorts (as reported in the bottom rows of the table), this

[15] Evidence on the sum of these effects at the new FRA, for example, would be informative about whether, overall, there is less benefit claiming by the new FRA for affected cohorts, which we would expect from the overall decline in benefits. Technically speaking, because the FRA varies by cohort, *AFRA* is not a simple dummy variable for an age range, but is instead defined to equal 1 when (i) a person is in a cohort affected by the increase in the FRA, and (ii) that person's age is equal to the FRA for his cohort, or older. As a result, the interaction with *ISSR* 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 Social Security reforms (born in 1938). Then we would not have the problem of a changing age range based on cohort, *AFRA* would be defined to simply equal 1 for age 65 and 2 months or greater, and the equation would be a standard difference-in-differences specification. Eq. (1) can be motivated by expanding it to allow separate estimates corresponding to δ 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.

[16] The unreported estimates for the other age dummy variables generally indicate slow increases in the probability of claiming with age, and slow declines in the probability of employment.

Table 4
Effects of Social Security reforms on claiming and employment between age 65 and the full retirement age and at ages 62–65.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Claiming SS benefits		Employment (full-time)			Any employment		
Cohorts affected by SS reforms ×								
Age ≥ 62	−0.052	…	0.003	0.018	…	0.009	0.021	…
	(0.015)		(0.014)	(0.014)		(0.014)	(0.014)	
× 1938–39 birth cohorts	…	0.020	…	…	0.021	…	…	−0.001
		(0.018)			(0.018)			(0.018)
× 1940–41 birth cohorts	…	−0.058	…	…	0.033	…	…	0.035
		(0.019)			(0.018)			(0.018)
× 1942–43 birth cohorts	…	−0.174	…	…	0.001	…	…	0.041
		(0.027)			(0.024)			(0.023)
Age ≥ 65	−0.176	−0.144	0.048	0.014	0.004	0.024	0.005	−0.007
	(0.025)	(0.025)	(0.026)	(0.028)	(0.028)	(0.028)	(0.030)	(0.030)
Age ≥ FRA	0.155	0.095	−0.027	−0.026	−0.020	−0.016	−0.019	−0.001
	(0.024)	(0.023)	(0.025)	(0.026)	(0.026)	(0.028)	(0.028)	(0.028)
Earnings test change:								
Age ≥ 65 × year ≥ 2000	…	…	…	0.049	0.057	…	0.023	0.024
				(0.018)	(0.018)		(0.021)	(0.020)
Year ≥ 2000	…	…	…	−0.044	−0.052	…	−0.036	−0.037
				(0.010)	(0.010)		(0.009)	(0.009)
Selected age dummies:								
61 and 10 or 11 months	0.066	0.065	−0.178	−0.159	−0.156	−0.165	−0.149	−0.148
	(0.019)	(0.019)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
62 and 0 or 1 month	0.218	0.217	−0.220	−0.208	−0.206	−0.184	−0.175	−0.174
	(0.024)	(0.025)	(0.026)	(0.027)	(0.027)	(0.025)	(0.025)	(0.025)
62 and 2 or 3 months	0.489	0.492	−0.289	−0.275	−0.273	−0.214	−0.203	−0.204
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)
64 and 10 or 11 months	0.658	0.655	−0.348	−0.322	−0.317	−0.278	−0.258	−0.256
	(0.024)	(0.024)	(0.026)	(0.027)	(0.027)	(0.027)	(0.028)	(0.028)
65 and 0 or 1 month	0.758	0.743	−0.456	−0.452	−0.450	−0.348	−0.337	−0.332
	(0.023)	(0.023)	(0.025)	(0.026)	(0.026)	(0.028)	(0.030)	(0.030)
65 and 2 or 3 months	0.791	0.780	−0.429	−0.426	−0.422	−0.318	−0.306	−0.301
	(0.022)	(0.022)	(0.026)	(0.027)	(0.027)	(0.028)	(0.030)	(0.030)
65 and 4 or 5 months	0.864	0.861	−0.442	−0.439	−0.436	−0.318	−0.305	−0.303
	(0.019)	(0.018)	(0.025)	(0.026)	(0.026)	(0.028)	(0.031)	(0.031)
R^2	0.557	0.560	0.244	0.244	0.245	0.230	0.231	0.231
Sample size	28,546	28,546	29,330	29,330	29,330	29,330	29,330	29,330

Note: The claiming variable is equal to 1 if individuals are collecting Social Security benefits and 0 otherwise. The employment variable is equal to 1 if individuals have a full-time job (second column) or any job (third column) and 0 otherwise. The cohorts affected by SS reforms are the ones who are born in 1938 of after. Age ≥ 62, ≥65, and ≥FRA variables are equal to 1 if individuals' ages are equal to or older than 62, 65, or their own FRA, respectively. The earnings test control variable after 2000 is equal to 1 if the respondent is observed in year 2000 or later. 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, and poor. OLS estimates of linear probability models are reported with standard errors, reported in parentheses, 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 who are younger than 69.

indicates that essentially the entire spike at age 65 disappears for cohorts with an FRA greater than 65. However, the third estimate in column (1), for age greater than or equal to the FRA, is more or less the same size, indicating a 15.5 percentage point higher likelihood of claiming, so that the spike in claiming shifts from age 65 to the FRA. The estimated changes in claiming attributable to the Social Security reforms are statistically significant for all three ages.[17]

Columns (3) and (6) report results for employment. The sign pattern is the same in each case but most of the estimated effects are not statistically significant. Even the point estimates give no indication of a higher employment probability for those aged 62 and older and affected by the reforms. That is not inconsistent with a claiming effect, of course, because the change in claiming could occur largely among people who continue to work or have already stopped working. The estimated changes at age 65 are both positive, with the estimate for full-time employment statistically significant at the 10-percent level, indicating a smaller reduction in employment at age 65 than for cohorts for which the FRA was 65. And offsetting this is a negative (but insignificant) estimate for those with age greater than or equal to the FRA. Together, the latter two estimates point to a shift in the decline in employment from age 65 to the FRA. The stronger effects for claiming than for employment at age 65 and the FRA are consistent with the results in Behaghel and Blau (2012), and we would anticipate that the effects would be stronger for Social Security claiming than for employment.[18]

In the remaining columns in Table 4 we look at two other issues. First, in columns (4) and (7), for employment, we account for the elimination of the earnings test in 2000 for those who have reached the FRA, which can affect those aged 65 and over differentially before and after 2000.[19] We add an interaction between a dummy variable for year 2000 or later and age greater than or equal to 65 (as well as the main year effect). As expected, the estimated effect of this interaction is positive (and statistically significant for full-time employment, for which the earnings test should have been more binding). With this addition to the specification, we no longer find evidence of a relative increase in employment for those aged 65 and over for whom the FRA increased.

Second, we examine whether the changes at age 62 were greater for the younger cohorts among the cohorts affected by the Social Security

[17] The shift in benefit claiming to the new FRA is consistent with evidence in Benitez-Silva and Yin (2009), Song and Manchester (2007), and Behaghel and Blau (2012).

[18] Note, though, that the smaller average changes in employment for those affected by the Social Security reforms do not imply that we cannot find strong interactions of age discrimination protections and being affected by these reforms for employment.

[19] We do not estimate this specifications for benefit claiming because there is no direct implication of the removal of the earnings test for claiming at age 65.

reforms, for whom the benefit reductions were larger (Table 1). In columns (2), (5), and (8) we interact *A62·ISSR* with three separate dummy variables indicating to which of the affected cohorts a respondent belongs. We would expect the decline in claiming after age 62 to be largest for the cohorts that faced the biggest reduction in benefits (the latest birth cohorts), and that is indeed what the estimates

Table 5
Age discrimination laws 1992 and 2008.

	Firm size (number of employees)		Compensatory/punitive damages		Statute of limitations (days)	
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	
	1992	2008	1992	2008	1992	2008
Alabama	No law	20	No law	No	No law	180
Alaska	1	1	No	Yes	Unknown	Not specified
Arizona	15	15	Yes	Yes	180	180
Arkansas	No law	No law	No law	No law	No law	No law
California	5	5	Yes	Yes	365	365
Colorado	1	1	No	No	180	180
Connecticut	3	3	No	No	180	180
Delaware	4	4	Unknown	Yes	120	120
District of Columbia	Unknown	1	Unknown	Yes	Unknown	365
Florida	15	15	Yes	Yes	365	365
Georgia	1	1	Unknown	No	180	180
Hawaii	1	1	Yes	Yes	180	180
Idaho	5	5	Yes	Yes	365	365
Illinois	15	15	Unknown	Yes	180	180
Indiana	1	1	No	No	120	120
Iowa	4	4	Yes	Yes	180	300
Kansas	4	4	Yes	Yes	180	180
Kentucky	8	8	Yes	Yes	180	180
Louisiana	8	20	Yes	Yes	180	365
Maine	1	1	Yes	Yes	180	180
Maryland	Unknown	15	Unknown	Yes	Unknown	180
Massachusetts	6	6	No	No	180	300
Michigan	1	1	Yes	Yes	180	180
Minnesota	1	1	Yes	Yes	365	365
Mississippi	No law	No law	No law	No law	No law	No law
Missouri	6	6	Yes	Yes	180	180
Montana	1	1	Unknown	Yes	180	180
Nebraska	25	20	No	No	300	300
Nevada	15	15	No	No	180	300
New Hampshire	6	6	Yes	Yes	180	180
New Jersey	1	1	Yes	Yes	180	180
New Mexico	4	4	Unknown	Yes	180	300
New York	4	4	Yes	Yes	365	365
North Carolina	15	15	No	No	Not specified	Not specified
North Dakota	1	1	No	No	300	300
Ohio	4	4	Yes	Yes	180	180
Oklahoma	15	15	No	No	180	180
Oregon	1	1	Unknown	Yes	365	365
Pennsylvania	4	4	No	No	180	180
Rhode Island	4	4	Yes	Yes	Unknown	365
South Carolina	15	15	No	No	180	180
South Dakota	No law	No law	No law	No law	No law	No law
Tennessee	8	8	Yes	Yes	180	180
Texas	15	15	No	Yes	180	180
Utah	15	15	No	No	180	180
Vermont	1	1	No	Yes	Unknown	365
Virginia	1	5	No	No	180	180
Washington	8	8	Yes	Yes	180	180
West Virginia	12	12	No	No	180	365
Wisconsin	1	1	No	No	300	300
Wyoming	2	2	No	No	90	180

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 Maine as of 1992 civil penal damages from $10,000 to $50,000 could be imposed. In 1997 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 these cases as having the stronger remedies otherwise implied by compensatory/punitive damages.

show. Although the link between claiming and employment is not necessarily that sharp, to some extent the employment effects reveal the same pattern. With the exception of the estimated interaction in column (5) for the 1942–43 birth cohorts, the estimates are monotonically increasing in the reduction in benefits (and corresponding increase in the FRA).

Thus, these preliminary estimates by and large match other related findings in the literature, as well as expectations, with the reductions in benefits and increases in the FRA delaying claiming and to a lesser extent increasing employment between age 62 and the new FRA, and shifting benefit claiming, in particular, to the new FRA. This sets the stage for the contribution of this paper — asking whether these kinds of shifts were larger in states with stronger age discrimination protections.

6. Data on age discrimination laws

To test whether stronger state age discrimination protections boost the effectiveness of supply-side Social Security reforms, we require comprehensive data on 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 LexisNexis, Westlaw, and HeinOnline, 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.[20]

Because it is complicated to read and interpret the law correctly based solely on statutes, we cross-checked our understanding of the statute with other legal references or treatises and additional sources of information on state laws.[21] 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.[22]

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 in footnote 22), 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.[23] We focus on three aspects of age discrimination laws that, based on 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.[24] 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.[25]

Second, we use information on remedies allowed under state law. We focus on whether compensatory or punitive damages are allowed, which they are not under federal law.[26] 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.[27]

Table 6 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

[20] 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).

[21] These included Fitzpatrick (2005, 2006, 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).

[22] 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 Rights 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 in the specific rules the Civil Rights Commission filed.

[23] 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 laws by state and year.

[24] Table 5 reveals that the distribution of stronger protections across states does not reflect the usual pattern related to generosity of social programs, minimum wages, etc. For example, some southern states have among the strongest age discrimination protections.

[25] 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.

[26] See U.S. 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).

[27] We also considered looking at recoverability of attorneys' fees. 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, including relying on court decisions and the language used in those decisions because recoverability is not always specified in the state statute. Although many states (41, including the District of Columbia) allow recoverability of attorneys' fees, the ADEA also does, stating that, if an ADEA plaintiff is successful, the "court in such action shall […] allow a reasonable attorneys' fee to be paid by the defendant, and costs of the action." (This language in stated under Title 29, Section 216 (b), which is incorporated in the ADEA by Title 29, Chapter 14, Section 621 (b).) It is possible that a different kind of stronger state protection is made more effective when there is recoverability under state law, but with only a handful of states (9) not allowing recoverability of attorneys' fees, it becomes impossible to reliably identify the effects of interactions between this recoverability and other state age discrimination protections. (And this could be exacerbated by the classification based on court decisions, which may not be as definitive).

Table 6
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

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.

not covered by the ADEA *are* covered by the state age discrimination law. As shown in Table 5, we could have simply used whether 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).[28] However, as Table 5 shows, only a few states do not have age discrimination laws. And there is very limited variation in state age discrimination laws — variation that is *not* needed for our identification strategy discussed below.[29]

7. State age discrimination laws and the increase in the full retirement age

7.1. Empirical approach

We now turn to our main analysis that asks whether responses to the reduction in Social Security early retirement benefits and the higher FRA differed in states with stronger age discrimination laws. This analysis can be thought of as expanding the difference-in-differences estimators in Eq. (1) to difference-in-difference-in-differences (DDD) estimators, allowing the estimated effects of being affected by the Social Security reforms – captured in the equation by the interactions *A62·ISSR*, *A65·ISSR*, and *AFRA·ISSR* – to vary with state age discrimination laws. Expanding on Eq. (1), then, the estimating equation is

$$R_{ist} = \alpha + \beta_1 A62_{ist}\cdot ISSR_t\cdot PA_s + \gamma_1 A65_{ist}\cdot ISSR_t\cdot PA_s + \delta_1 AFRA_{ist}\cdot ISSR_t\cdot PA_s + \beta_0 A62_{ist}\cdot ISSR_t + \gamma_0 A65_{ist}\cdot ISSR_t + \delta_0 AFRA_{ist}\cdot ISSR_t + \kappa ISSR_t\cdot PA_s + \lambda ISSR_t + \sum_k A^k{}_{ist}\cdot PA_s\psi_{1k} + \sum_k A^k{}_{ist}\psi_{0k} + X_{ist}\theta + \varepsilon_{ist}. \quad (2)$$

We use the same Greek letters as before, but now those with a '1' subscript capture whether the shifts discussed in the context of Eq. (1) are larger when state age discrimination protections are stronger. In Eq. (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. (We also discuss evidence from specifications with multiple features of state age discrimination laws considered simultaneously.) The controls in *X* are the same as in Eq. (1), including state dummy variables.[30]

Eq. (2) embeds three DDD estimators. One (β_1) is the difference in the dependent variable for those aged 62 or greater in cohorts affected by the Social Security reforms 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 unaffected cohorts. The second DDD estimator (γ_1) is the parallel estimator, but for those aged 65 and over. And the third (δ_1) is for those at the FRA for their cohort or older.[31] Extending the example from the discussion of Eq. (1), then, a negative estimate of γ_1 and a positive estimate of δ_1 in the equation for benefit claiming would point to a stronger age discrimination protection enhancing the shift in the spike in benefit claiming from age 65 to the new FRA for affected cohorts.

Eq. (2) can be made a bit more flexible, and we do this in an extension of the empirical analysis. First, we can include birth-cohort dummy variables by year rather than simply *ISSR*, to allow more heterogeneity in the intercepts across birth cohorts. And second, we can introduce interactions between these birth-cohort dummy variables and *PA*, to allow cohort profiles to differ across states with and without stronger age discrimination protections. Identification of how the effect of increases in the FRA 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 in different states, and in that sense has the same flavor as a regression discontinuity design.

7.2. Main results

Estimates of Eq. (2), using the more parsimonious specification described above, are reported in Table 7.[32] The table reports the main interactions of interest, along with the main effects of being affected by the Social Security reforms and the combined effects. Columns (1)–(3) report the results for Social Security claiming. We consider the different features of age discrimination laws one at a time. The estimates in the first three rows, for changes in claiming at age 62 depending on state age discrimination protections, are all negative. The sign is consistent with stronger age discrimination protections being associated with larger declines in benefit claiming for those aged 62 and older, although none of the estimates are statistically significant.

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

[29] Tables in the on-line appendix report the means for the policy variables by year, and document the handful of policy changes that occurred.

[30] 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. As a short-hand, though, *PA* in Eq. (2) only has an *s* subscript. During the period when the Social Security reforms took effect, there were no changes in federal age discrimination law that could be used as identifying variation; regardless, federal changes would not be useful in identifying the effect of interest because these laws would affect all workers in a given birth cohort simultaneously.

[31] Paralleling the discussion of Eq. (1), for the older group this setting is more complex than a conventional DDD estimator because the FRA varies by birth cohort for those born in 1938 or later; the earlier comment about the redundancy of the *ISSR* 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 *AFRA·ISSR·PA* and *AFRA·ISSR* variables for each affected birth cohort, with the age range 65 and 2 months or older for the 1938 birth cohort, 65 and 4 months or older for the 1939 birth cohort, etc. With six such cohorts in our sample, we would then effectively have *eight* DDD estimators, one for each of these cohorts and the ones defined at age 62 and age 65. We could then constrain the effects (corresponding to δ_1 and δ_0 in Eq. (2)) to be the same for each of the groups with age greater than or equal to the FRA. This is equivalent to redefining those with age equal to or older than their FRA and affected by the reforms (in this case, 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 having age equal to or older than their FRA if age is 65 and 2 months or older and birth year equals 1938, *or* if age is 65 and 4 months or older and birth year equals 1939. Collapsing the larger model in this way yields Eq. (2).

[32] The "Cohorts affected by increase in FRA × age range × age discrimination law feature" variables reported in the table correspond to *A62·ISSR·PA*, *A65·ISSR·PA*, and *AFRA·ISSR·PA* in Eq. (2). The "Cohorts affected by increase in FRA × age range" main effects correspond to *A62·ISSR*, *A65·ISSR*, and *AFRA·ISSR*.

Table 7
Effects of state age discrimination laws on impact of Social Security reforms 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)
	Claiming SS benefits			Employment (full-time)			Any employment		
Cohorts affected by SS reforms × age ≥ 62 ×									
Lower firm size (<10)	−0.015	…	…	−0.004	…	…	−0.028	…	…
	(0.025)			(0.032)			(0.020)		
Stronger remedies	…	−0.035	…	…	0.078	…	…	0.018	…
		(0.027)			(0.025)			(0.025)	
Longer statute of limitations	…	…	−0.005	…	…	0.022	…	…	−0.015
			(0.030)			(0.033)			(0.022)
Cohorts affected by SS reforms × age ≥ 65 ×									
Lower firm size (<10)	−0.024	…	…	0.140	…	…	0.145	…	…
	(0.037)			(0.054)			(0.043)		
Stronger remedies	…	−0.071	…	…	0.047	…	…	0.107	…
		(0.038)			(0.062)			(0.054)	
Longer statute of limitations	…	…	−0.021	…	…	−0.031	…	…	0.044
			(0.031)			(0.054)			(0.055)
Cohorts affected by SS reforms × age ≥ FRA ×									
Lower firm size (<10)	0.045	…	…	−0.132	…	…	−0.132	…	…
	(0.038)			(0.052)			(0.044)		
Stronger remedies	…	0.088	…	…	−0.081	…	…	−0.101	…
		(0.038)			(0.057)			(0.055)	
Longer statute of limitations	…	…	0.028	…	…	−0.006	…	…	−0.040
			(0.033)			(0.055)			(0.055)
Main effects, cohorts affected by SS reforms ×:									
Age ≥ 62	−0.042	−0.030	−0.050	0.021	−0.030	0.013	0.038	0.010	0.026
	(0.018)	(0.024)	(0.015)	(0.024)	(0.016)	(0.021)	(0.014)	(0.021)	(0.017)
Age ≥ 65	−0.165	−0.131	−0.168	−0.065	−0.021	0.024	−0.078	−0.066	−0.008
	(0.021)	(0.032)	(0.028)	(0.038)	(0.053)	(0.040)	(0.036)	(0.047)	(0.030)
Age ≥ FRA	0.132	0.100	0.143	0.048	0.028	−0.024	0.057	0.047	−0.007
	(0.027)	(0.033)	(0.026)	(0.038)	(0.052)	(0.035)	(0.038)	(0.048)	(0.027)
Combined effects, interaction plus main effect:									
Age ≥ 62	−0.057	−0.065	−0.055	0.017	0.048	0.035	0.010	0.029	0.011
	(0.018)	(0.014)	(0.026)	(0.021)	(0.020)	(0.027)	(0.016)	(0.015)	(0.016)
Age ≥ 65	−0.190	−0.201	−0.189	0.075	0.026	−0.007	0.067	0.041	0.036
	(0.030)	(0.020)	(0.015)	(0.039)	(0.034)	(0.041)	(0.029)	(0.031)	(0.050)
Age ≥ FRA	0.177	0.188	0.171	−0.084	−0.052	−0.030	−0.075	−0.054	−0.047
	(0.027)	(0.019)	(0.021)	(0.036)	(0.029)	(0.045)	(0.027)	(0.026)	(0.049)
R^2	0.558	0.558	0.559	0.247	0.247	0.247	0.233	0.233	0.234

Note: All specifications include dummy variables for age in months (by two-month increments), interactions between these age dummy variables and the age discrimination law feature included in the column, state dummy variables, and the individual-level controls for urban–rural, race, marital status, education level, and self-reported health status described in the notes to Table 4. Employment and any employment specifications include Social Security earnings test control variables described in Table 4. See Tables 5 and 6 (and Tables A1 and A2 in the online appendix) for information on state age discrimination laws. OLS estimates of linear probability models are reported with standard errors, reported in parentheses, calculated using non-nested clustering at the state and individual level. HRS restricted data with state identifiers are used. Sample sizes are as in Table 4.

The next three rows report on how the changes in behavior at 65 – the original FRA – vary with state age discrimination protections. Again, all three estimates are negative, consistent with cohorts with an FRA above age 65 (and lower benefits for early retirement) delaying claiming past age 65 more in states with stronger age discrimination protections. In states with stronger remedies the effect of the reforms in reducing benefit claiming is particularly strong, with the estimated coefficient implying a 7.1 percentage point lower probability of having claimed by age 65, which is statistically significant at the 10-percent level.

Finally, the next three rows report on changes at the FRA. Here, all the estimates are positive, consistent with stronger age discrimination protections boosting the delay in claiming benefits to the new FRA. Again, the estimates for a lower firm-size cutoff and a longer of statute of limitations are relatively small and not significant, but the estimate for stronger remedies is much larger, and statistically significant at the 5-percent level.

Note that the estimated main effects of the reforms are negative at age 62 and age 65 (strongly so), and strongly positive at the FRA. These main effects hold in the states without the state age discrimination protection referenced in the column. The interactions in the first nine rows indicate how much stronger these shifts in behavior are in states with the stronger age discrimination protection. For benefit claiming, as a comparison of the main and combined effects indicates clearly, stronger age discrimination protections are always estimated to enhance these shifts in behavior. As noted above this "enhancement" is statistically significant for the shifts at age 65 and the FRA, for stronger remedies. In other words, the data support the conclusion that stronger remedies amplify the shift in the spike in benefit claiming from age 65 to the FRA.

Columns (4)–(9) instead look at employment — full-time employment and any employment. In the first three rows, for changes in employment at age 62, the signs are mixed. There is, however, a statistically significant positive effect of stronger remedies on the increase in full-time employment at age 62.[33] The positive sign is consistent with the idea that stronger age discrimination protections enhance the employment response to the decline in benefits for the affected cohorts.

In the next three rows, which turn to changes in behavior at 65, the estimated coefficients for a lower firm-size cutoff and stronger remedies are both positive in every case – for both full-time employment and any employment – again consistent with stronger age discrimination protections enabling a stronger positive employment response. For both employment measures the estimate is larger for the lower firm-size cutoff, and statistically significant in both cases, with the point estimate for full-time employment implying a 14.0 percentage point higher probability of being employed at age 65 and after. For any employment, in

[33] Here and henceforth, unless otherwise specified, statements that estimates are statistically significant refer to the 5-percent level.

column (8), there is also a statistically significant positive effect of stronger remedies on the probability of being employed at (and after) age 65.

Finally, in the next three rows, for changes at the FRA, all the estimates are negative, consistent with stronger age discrimination protections boosting the delay in exiting employment to the FRA for cohorts affected by the Social Security reforms. In this case the estimates for both a lower firm-size cutoff and stronger remedies are sizable for both employment measures; and they are statistically significant (in one case only at the 10-percent level), with the opposite sign, for the corresponding cases for which we found a positive and significant effect on employment at age 65.

For employment the main effects for states without stronger age discrimination protections are not always in the direction of higher employment at or after age 62 and age 65, and lower employment at the FRA. Given the evidence in Table 4 of rather weak overall effects of the reforms on employment at these ages, this finding is plausible. But the combined effects in the last three rows of the table indicate that in 17 out of 18 cases the data fit this pattern for states with stronger age discrimination protections; the one exception is for a longer statute of limitations, for which we never find statistically significant effects.

Overall, then, the employment results indicate quite strongly that stronger age discrimination protections in the form of a lower firm-size cutoff or stronger remedies boost the labor supply response to the decline in benefits and increases in the FRA, leading to higher employment at or after age 62 or 65, and shifting the decline in employment to the FRA. The evidence for benefit claiming is in the same direction, although only for stronger remedies. It is not surprising to find stronger effects on employment, as we would expect the age discrimination that stronger protections might reduce to have direct effects on employment, but only to affect claiming to the extent that claiming is dependent on employment.

It is useful to consider the implications and plausibility of the magnitudes of the estimates. For example, the second estimate in column (2) of Table 7 implies that stronger remedies under state age discrimination laws enhance the reduction in benefit claiming for those over age 65, from the increase in the FRA (presumably), by 7.1 percentage points, and the main effect in that same column is a decline of 13.1 percentage points, suggesting that greater age discrimination protection in the form of stronger remedies increases the effect of the reforms by about 54%. The effect is large, but it may be plausible in that it is a good deal smaller than the baseline change that appears to be induced by the increase in the FRA (a decline of 17.6 percentage points, from Table 4).

The magnitudes of the estimated employment effects are perhaps harder to rationalize. Estimates of the complementary effects of lower firm-size cutoffs and stronger remedies under state age discrimination laws, for those aged 65 and over, are sometimes in the 10 percentage point range or higher. These implied effects are large relative to the overall change in employment for those aged 65 or over who are affected by the Social Security reforms (Table 4), although the overall effects, as reported in the last three rows in Table 7 (e.g., 7.5 percentage points in column (4)), are considerably smaller. At the same time, the large employment effects may not be as implausible as might appear at face value. Because our specification simply shifts when the changes in behavior – such as the decline in the employment probability with age – occur over a narrow age range (e.g., age 65 to the FRA), these estimates do not imply large aggregate effects. Moreover, earlier evidence on the effects of state-level variation in age discrimination laws (Adams, 2004) suggested increases in employment probabilities of 3.6 to 4.1 percentage points among those aged 60 and older or 65 and older. Since those estimates reflect an average effect in a population where many are not working, whereas the estimates in this paper are likely to capture more of a marginal effect on staying employed, we might expect larger effects in the present context. On the other hand, our estimates should be smaller because they pertain to strengthening of existing (i.e., federal) age discrimination laws, whereas the Adams estimates pertain to the original implementation of age discrimination laws.

Finally, the estimated effects of the lower firm-size cutoffs would be unexpected if relatively few workers are employed at small firms. However, the HRS data show that as individuals age they are more likely to be employed at smaller firms. Using the firm-size categories available in the HRS, and excluding the self-employed, at ages younger than 62, 23.4% of workers in our sample were employed at firms with 14 or fewer employees. This percentage rises to 27.7 for 62–65 year-olds, and to 35.7% for those age 65 and over.[34] Given that this percentage rises sharply with age and ends up quite high, strengthening age discrimination protections at smaller firms could have a substantial impact on older workers.

Thus, to summarize, there is evidence that stronger remedies in state age discrimination laws enhanced the effects of Social Security reforms that lowered early retirement benefits on the claiming behavior and employment of those aged 62 and over — lowering claiming and increasing full-time employment. There is also evidence that stronger remedies resulted in more shifting of claiming to the FRA, from age 65, with offsetting movements in employment (more employment at age 65, and a larger decline at the FRA); presumably this was in direct response to the increases in the FRA. There is also evidence that a lower firm-size cutoff enhanced the effects of increases in the FRA on employment, although it does not appear to affect claiming. In this analysis the stronger and more consistent evidence arises for stronger remedies, which perhaps is not surprising given that stronger remedies apply across the board to all workers potentially affected by an age discrimination claim, and directly affect the financial incentives to pursue a claim.[35]

7.3. Additional analyses and extensions

We extended the analysis in a few ways to ensure that the findings are robust; these results are reported in the on-line appendix. First, we estimated the models for claiming and employment when all three age discrimination laws are considered simultaneously. The estimates were very similar to those in Table 7. Moreover, the standard errors did not increase much, indicating that we can identify the effects of the different types of state age discrimination protections.[36]

Second, we explored the results of estimating a more-saturated model that includes birth-cohort dummies and their interactions with state age discrimination law features. The estimates were very similar, and in some cases the evidence was a bit stronger.

Third, given that the choice of what firm-size cutoff to use is somewhat arbitrary, we instead used a cutoff of 15 employees. We only estimated these models for employment, for which we found an effect of this kind of age discrimination protection. In every case, when we use the higher cutoffs we find smaller effects of this state age discrimination protection on how the Social Security reforms influenced behavior at age 65 and the FRA. That is, as we include states where cutoffs for age discrimination protection are more similar to the ADEA, and hence the

[34] 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. Our calculation is based on males born between 1931 and 1943 who are younger than 69.

[35] 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).

[36] There was one apparently anomalous result with the estimated effect of the lower-firm-size cutoff on any employment negative and significant at the 10-percent level. We already noted that we generally find more consistent evidence for stronger remedies.

Table 8
Effects of state age discrimination laws on impact of Social Security reforms on claiming and employment between age 65 and the full retirement age and at ages 62–65, falsification tests.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Self-employed only		Apply reforms in 1997–2002; omit 2003 and after, and 1938 and later birth cohorts					
	Claiming SS benefits	Claiming SS benefits	Claiming SS benefits	Claiming SS benefits	Employment (full-time)	Employment (full-time)	Any employment	Any employment
Cohorts affected by SS reforms × age ≥ 62								
Lower firm size (<10)	−0.111	…	−0.011	…	−0.080	…	−0.072	…
	(0.081)		(0.046)		(0.063)		(0.073)	
Stronger remedies	…	−0.080	…	0.032	…	−0.132	…	−0.081
		(0.075)		(0.042)		(0.061)		(0.068)
Cohorts affected by SS reforms × age ≥ 65								
Lower firm size (<10)	0.099	…	−0.018	…	−0.004	…	−0.005	…
	(0.142)		(0.032)		(0.059)		(0.058)	
Stronger remedies	…	0.009	…	−0.020	…	0.003	…	−0.074
		(0.151)		(0.045)		(0.062)		(0.059)
Cohorts affected by SS reforms × age ≥ FRA								
Lower firm size (<10)	0.100	…	−0.000	…	0.018	…	0.054	…
	(0.131)		(0.036)		(0.065)		(0.049)	
Stronger remedies	…	−0.035	…	0.014	…	0.016	…	0.071
		(0.154)		(0.038)		(0.062)		(0.049)
R^2	0.621	0.618	0.547	0.547	0.231	0.231	0.211	0.211
Sample size	4635	4635	15,484	15,484	15,773	15,773	15,773	15,773

Note: Notes from Table 7 apply, except for sample restrictions noted, and the assignment of reforms to (incorrect) years in columns (3)–(8).

protections are weaker, we find less evidence of this policy complementarity. This evidence supports the conclusion that lower firm-size cutoffs for the applicability of state age discrimination protections enhanced the effects of the increases in the FRA that were part of these reforms. Nonetheless, using the 15 cutoff the estimated firm-size interactions remain fairly large and statistically significant at the 5-percent or 10-percent level, so that, overall, conclusions about the effects of firm-size cutoffs are not fragile.[37]

7.4. Falsification tests

Next, we present results from two falsification tests, in Table 8. First, we estimate specifications for the self-employed. Age discrimination should not directly affect employment or retirement decisions of the self-employed, and hence stronger age discrimination protections should not be associated with changes in their behavior in response to Social Security reforms (although these reforms can affect their behavior). Because self-employment status is only defined for those who are employed, we only estimate specifications for this subsample for claiming behavior. As shown in the first two columns of Table 8, there is no evidence from this falsification test suggesting that state-level variation in responses to the Social Security reforms varied in such a way as to create spurious evidence, for the full sample, of complementary effects of state age discrimination protections. None of the estimates in columns (1) and (2) are statistically significant, and although the estimates are considerably less precise than the full-sample results in Table 7, the sign pattern does not fit the Table 7 estimates, as half of the estimated coefficients have different signs.[38]

In our second falsification test we pretend that the reforms took effect in earlier years (1997–2002 instead of 2003–2008). We apply these reforms to the right ages in these years, so that, for example, the FRA for the cohort reaching 65 in 1997 (the 1932 birth cohort) would be 65 and 2 months. We drop from the sample the birth cohorts that were actually affected, and to be symmetric with respect to the ages covered by the "reforms," we use 2002 as the last year of data, rather than 2008. The only difference is that to get data on more birth cohorts prior to the placebo reforms, we add data back to the 1925 birth cohort (rather than starting with the 1931 birth cohort, as we do in our main analyses). Without this latter change, we would have only one unaffected cohort (those born in 1931). We verified that our results based on the actual reforms were similar when these earlier cohorts were added.

The results are reported in columns (3)–(8) of Table 8, for each of our three outcomes, and for each of the two age discrimination protections for which we earlier found effects. If we estimated similar effects for these birth cohorts of the placebo reforms, we would have to conclude that our main results were driven not by actual differences across states, associated with state age discrimination protections, in responses to the Social Security reforms. Rather, they would instead be due to cross-state differences between younger and older cohorts in claiming and employment behavior in the 62–66 age range that are correlated with state age discrimination protections in such a way as to make it seem that, for example, stronger age discrimination protections led to reductions in claiming of benefits after age 62 and after age 65, and increases in claiming between age 65 and 66. However, as the table shows, we never find evidence in this falsification exercise of results paralleling those in Table 7. There are a couple of large coefficient estimates (one significant in column (6)); but these are always the opposite sign of the corresponding estimates in Table 7.

[37] Finally, we also estimated the specifications in Table 7 with individual fixed effects (see the on-line appendix). We do not emphasize these results because there is not a compelling reason to be concerned about bias from omitted individual heterogeneity, given that all of the variation in the variables of interest stems from federal policy variation over time or cross-state policy variation, coupled with small age differences across individuals. Moreover, such models do not make sense for claiming behavior, because one can begin claiming benefits only once, so there is no meaningful pre- and post-variation relative to passing the age thresholds used in the model. Nonetheless, for the employment models the results were qualitatively similar to those in Table 7. The estimates indicating effects of state age discrimination laws on responses to the Social Security reforms were sometimes a little smaller and/or less strong statistically, but this was also often true for estimates of models without individual fixed effects, using the subsample of respondents with at least two observations, who are the only respondents from whom we can identify the other parameters once we use individual fixed effects.

[38] We verified that results for the preceding analyses were similar if we excluded the self-employed (see the on-line appendix). The falsification test may not be viewed as absolutely definitive because self-employment can be a temporary state and may be tied to consulting or other temporary arrangements that older worker have with their previous employer. For the same reason, we chose to show results for all workers rather than excluding the self-employed in the earlier tables.

Table 9
Effect of state age discrimination laws on employment transitions for those affected by Social Security reforms.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Previous wave	Employed (wage/salary or self-employed) at t − 2	Employed (wage/salary) at t − 2	Employed (wage/salary) at t − 2	Not employed at t − 2	Not employed at t − 2	Employed (wage/salary) at t − 2	Not employed at t − 2
Current wave (outcome)	Any employment at t	Employed (wage/salary, same employer) at t	Employed (wage/salary, different employer) at t	Any employment at t	Wage/salary employment at t	Self-employed at t	Self-employed at t
Cohorts affected by SS reforms × age ≥ 62 × lower firm size	−0.057	−0.075	−0.011	−0.006	0.046	0.005	−0.040
	(0.038)	(0.051)	(0.029)	(0.042)	(0.034)	(0.015)	(0.022)
Cohorts affected by SS reforms × age ≥ 65 × lower firm size	0.162	0.165	0.028	0.052	0.002	0.002	0.050
	(0.074)	(0.105)	(0.069)	(0.061)	(0.048)	(0.031)	(0.036)
Cohorts affected by SS reforms × age ≥ FRA × lower firm size	−0.137	−0.094	−0.025	−0.066	−0.034	−0.005	−0.032
	(0.085)	(0.108)	(0.053)	(0.050)	(0.042)	(0.025)	(0.033)
Cohorts affected by SS reforms × age ≥ 62 × stronger remedies	0.058	0.046	0.025	−0.056	−0.056	−0.024	−0.001
	(0.040)	(0.057)	(0.027)	(0.037)	(0.035)	(0.018)	(0.021)
Cohorts affected by SS reforms × age ≥ 65 × stronger remedies	0.076	0.000	0.099	0.047	0.041	−0.002	0.006
	(0.078)	(0.111)	(0.062)	(0.056)	(0.050)	(0.034)	(0.038)
Cohorts affected by SS reforms × age ≥ FRA × stronger remedies	−0.101	−0.019	−0.117	−0.062	−0.052	0.024	−0.010
	(0.080)	(0.098)	(0.050)	(0.049)	(0.046)	(0.031)	(0.033)
Sample size	15,220	11,285	11,285	8006	8006	11,285	8006

Notes: OLS estimates of linear probability models are reported with standard errors, reported in parentheses, calculated using non-nested clustering at the state and individual level. For each subsample (e.g., employed for a wage or salary at wave t − 2, not employed at t − 2), we estimate linear probability models corresponding to the outcome at wave t in each column. Each panel reports estimates of separate specifications using the different specified state age discrimination protections; lower firm size refers to a size cutoff of 10 employees. All specifications include dummy variables for age in months (by two-month increments), state dummy variables, and dummy variables for urban or rural residence, race, marital status, education level, self-reported health, and earnings test control variables. See notes to Table 4. The specifications also include: a dummy variable for the age discrimination protection indicated; a dummy variable for the birth cohorts affected by the increase in the FRA as well as an interaction of this variable with the age discrimination protection feature; dummy variables for the three age groups (older than 62, 65, or their own FRA); and interactions of the age dummy variables and the specified age discrimination law protection. The specification corresponds to Eq. (2) in the text.

7.5. The dynamics of employment: hiring

The findings so far point to larger employment changes for cohorts affected by benefit cuts and increases in the FRA in states with stronger age discrimination protections. Next, we turn to the question of how the employment increases come about. Is it through continued employment at the same employer, hiring at new employers, or even re-entry into employment? We set the stage for this inquiry earlier. We noted that, on the one hand, age discrimination laws are likely to do more to enhance the effects of Social Security reforms if they increase hiring. On the other hand, though, there is more skepticism that these laws are effective at increasing hiring of older workers and could even deter it.

If hiring discrimination against older workers deters transitions to new jobs, then some workers may have to respond to supply-side Social Security reforms by remaining in their current jobs. This would likely limit overall responsiveness to such reforms, perhaps allowing only minor adjustments to increases in the FRA as workers remain at their same employer a little bit longer, but inhibiting partial retirement by taking up bridge jobs. Conversely, if stronger age discrimination protections enhance hiring of older workers, then these protections may ultimately lead to more substantial lengthening of work lives in response to supply-side incentives, in part, perhaps, by enabling older workers to move to jobs more suitable for them given possible physical constraints associated with aging.

The only empirical studies of how age discrimination laws affect hiring are by Lahey (2008b) and Adams (2004). Adams does not find any evidence that age discrimination laws increase hiring of older workers, and perhaps the opposite, especially for those aged 65 and over. However, the data he uses are not that well-suited to measuring hiring. Lahey concludes that stronger age discrimination laws deter hiring of older workers, but there are reasons to be skeptical of this conclusion.[39] Among other things, Lahey simply looks at whether there was a state age discrimination law, as this lengthens the statute of limitations. But this is the one feature of state age discrimination laws that – earlier in the paper – we found does not matter. In contrast, the age discrimination protections that appear to matter are whether state age discrimination laws cover small firms exempted from the ADEA, and whether stronger remedies in the form of compensatory and punitive damages are allowed. Thus, the question of how hiring and other transitions of older workers are affected by the features of state age discrimination laws that *do* matter is unexplored.

To study hiring, rather than employment, we exploit the longitudinal nature of the HRS data to measure employment transitions. For the most part, we do this based on employment status at each wave and information on whether a worker reported changing employers. Thus, for example, we measure whether a person was not employed at wave t − 2 but employed at wave t, or whether a worker changed employers between wave t and t − 2.[40] We also try to tease more information on hiring out of the HRS by using responses to interview questions that provide information on labor market transitions between the interviews, which we refer to as "inter-wave" information. The regression framework we use is the same as before, but now the outcomes are various transitions.[41]

Table 9 provides evidence on whether stronger age discrimination laws lead to differences in the labor market transitions of individuals

[39] Neumark (2008) provides a lengthier discussion.
[40] HRS waves are two years apart.
[41] These specifications include the same earnings test controls as the specifications in Table 7. Given that we are not in any way studying hiring conditional on applying, it seems appropriate to include these controls. However, the results were nearly identical when they were not included.

affected by the Social Security reforms. We report estimates for many possible labor market transitions, incorporating the information on state age discrimination protections (as in Eq. (2)). Specifically, we focus on those employed at all at wave t − 2, employed in wage or salary jobs, as well as those not employed at wave t − 2. For those with wage or salary employment at wave t − 2, we estimate linear probability models for the probability of wage or salary employment at wave t, and then the probability of employment with the same or a different employer. For those not employed at wave t − 2, we estimate linear probability models for the probability of any employment or of wage/salary employment at wave t. We estimate models first incorporating an indicator for a lower firm-size cutoff (10 or more workers) under state age discrimination law. We then instead use the indicator for stronger remedies under state law.[42]

The estimates in the first row of each panel, in columns (1)–(3), indicate that, for those aged 62 and over in affected cohorts who are initially employed, there are no statistically significant effects of a lower firm-size cutoff or of stronger remedies on the probability of remaining employed, or the probability of either remaining at the same employer or switching to a new employer. Consistent with the results in Table 7, the point estimates are negative for the firm-size cutoff, and positive for stronger remedies.

At age 65, a stronger state age discrimination law in the form of a lower firm-size cutoff is associated with a higher probability of remaining employed, by 0.162 (significant at the 5-percent level). We then focus on those initially in wage or salary employment, and look at whether they are subsequently employed at the same or a different employer. The evidence suggests that the positive effect of a lower firm-size cutoff on remaining employed comes about because of a higher probability of remaining at the same employer, rather than transitions to a different employer. This evidence does *not* point to a lower firm-size cutoff increasing hiring of those aged 65 and over, and already employed, of those affected by the Social Security reforms (in particular, the increase in the FRA). As shown in columns (4) and (5), for those not employed initially, a lower firm-size cutoff under state age discrimination law is not significantly associated with a higher probability of becoming employed.

Continuing to focus on changes at age 65, in the second panel, which looks at stronger remedies under state age discrimination law, there is no statistically significant evidence that this feature of state age discrimination laws affects the probability of remaining employed overall, although the point estimate is quite large (0.076). However, when we narrow attention to those with wage or salary employment and look at whether one stays at the same employer or makes a transition to a new employer, this effect arises solely in a boost in the probability of a transition to a new employer, by 0.099 (which is only marginally significant). And again, for this type of law there is no evidence of a hiring effect for those initially non-employed.

Finally, the third row of each panel focuses on transitions at the new FRA for the affected cohorts. Here the evidence is consistent. The same stronger age discrimination protections that boost continued employment at age 65 for these cohorts are associated with declines in the likelihood of continued employment at the FRA, consistent with shifts in employment behavior so that those in the affected cohorts are more likely to remain employed through age 65, and then to leave employment at the new FRA. For example, in the top panel we find large negative estimates for the probability that the employed remain employed (columns (1) and (2), with the first nearly statistically significant at the 10-percent level), and also negative estimates for the probability that the non-employed have become employed (columns (4) and (5), although not statistically significant). The point estimates in the bottom panel are similar.

Table 10
Effect of state age discrimination laws on hiring for individuals affected by Social Security reforms.

	(1)	(2)
	Based on employment status at waves t − 2 and t	Incorporating information inter-wave information
Outcome	Hired between waves t − 2 and t	Hired between waves t − 2 and t
Cohorts affected by SS reforms × age ≥ 62 × lower firm size	0.004	0.008
	(0.020)	(0.022)
Cohorts affected by SS reforms × age ≥ 65 × lower firm size	0.030	0.051
	(0.035)	(0.043)
Cohorts affected by SS reforms × age ≥ FRA × lower firm size	−0.028	−0.053
	(0.029)	(0.036)
Cohorts affected by SS reforms × age ≥ 62 × stronger remedies	0.008	0.014
	(0.019)	(0.021)
Cohorts affected by SS reforms × age ≥ 65 × stronger remedies	0.057	0.039
	(0.031)	(0.041)
Cohorts affected by SS reforms × age ≥ FRA × stronger remedies	−0.072	−0.067
	(0.027)	(0.036)
Sample size	23,226	22,806
Hiring rate		
Overall	0.089	0.141
Ages 60–61	0.092	0.141
Age ≥ 62	0.078	0.129
Age ≥ 65	0.073	0.117

Notes: Notes to Table 9 apply. Additional controls include a dummy variable equal to 1 if the respondent was self-employed at t − 2 and a dummy variable equal to 1 if the respondent was not working at t − 2. The dependent variable "hired" is equal to 1 if we observe any hire between waves; in the first column this is based only on employment status and respondent's answer to "are you still working for the same employer?" at the waves, and in the second column we incorporate additional information on job transitions between the waves. Specifically, employment transitions from self-employed or not working to employed are coded as hires, as are transitions from employed at wave t − 2 to working for a different employer at wave t. Respondents who make transitions from non-employment at wave t − 2 to self-employed or non-employment at wave t are coded as hires if they report working for a wage or salary between waves. Otherwise respondents are coded as non-hires. The sample size is smaller in the second column because data are sometimes missing to fill in inter-wave hires. In some cases – but not all – we made a determination as to whether there was an inter-wave hire. Specifically, the questions on work between waves were not asked for respondents who went from self-employed to not employed or self-employed, if they do not know when they stopped the initial self-employed job; we assumed these individuals were not hired between waves. Also, many observations are missing the inter-wave information and classified as "inapplicable or partial interview" in the codebook. For cases with missing data and transitions from wave t − 2 to t between disabled, retired, and not in the labor force (based on the RAND HRS labor force status code), we assumed no hire occurred.

The evidence for transitions at age 65, in particular, suggests – albeit weakly – that stronger age discrimination protections in the form of stronger remedies may have acted to boost hiring of older workers affected by the Social Security reforms, who had an incentive – even if driven only by norm or reference effects – to work longer. As a falsification test with regard to whether age discrimination protections boost hiring of older individuals in the affected cohorts, we estimated models for transitions to self-employment, which should not be directly affected by stronger age discrimination protections. As shown in the last two columns of the table, there is no evidence that these protections increase the probability of transitions to self-employment for these older individuals. The estimates are generally very small and never statistically significant.

This possibility of positive effects of age discrimination laws on hiring contrasts with the conjecture that age discrimination laws will

[42] We do not focus on transitions from employment to non-employment – i.e., separations – because we are interested in the decisions initiated by *workers* in response to Social Security reforms and how discrimination protections influence these responses. There is no obvious reason why employer-initiated separations would respond to these reforms, and age discrimination laws should not affect employee-initiated separations.

deter hiring of older workers, and is more positive with respect to thinking about how stronger age discrimination laws can help lengthen work lives. However, the evidence is not entirely consistent across different types of age discrimination protections, and is not strong statistically. Thus, we try to get a firmer understanding of effects on hiring by focusing on hiring specifically. We first estimate models for a single hiring variable (i.e., in Table 9, whether one switched employers, or went from self-employment or non-employment to wage or salary employment). We also use more-detailed data than what is simply available at each wave, exploiting information on job transitions between waves to try to capture additional hiring that could be missed because of employment transitions between interviews. With older individuals making transitions to partial retirement, changes over a shorter period than the two years between waves could be common. For example, a person employed at wave t − 2 but not employed at wave t could have been hired into a job after wave t − 2 that he left by wave t.

We use the HRS's Employment, Retirement, and Pension questions to track hires that occurred between interviews. The data have some limitations for this purpose because the questions on work between waves are not asked of every respondent, either because of incomplete interviews or because in a couple types of cases the questions that could detect a hire between waves were not asked (for those who go from self-employed to not self-employed). In many cases of missing data we were able to make an educated guess that no hire occurred, although it is possible that in some cases a hire did in fact occur.[43] Overall, we lose about 500 observations for whom we cannot determine (or make an educated guess at) whether a hire occurred between waves.

Table 10 reports the estimates. Column (1) just uses the information at the waves, while column (2) supplements this with information on hiring between waves. As the descriptive statistics at the bottom of the table show, the definition of hiring using the inter-wave information picks up substantially more hiring. These statistics also show that hiring rates decline with age, as would be expected.

The point estimates always indicate a positive effect of stronger age discrimination protections on hiring, for those aged 65 and over in the cohorts for which the FRA increased. This holds for either definition of hiring. However, only one of the four estimates – for stronger remedies in column (1) – is statistically significant (at the 10-percent level). Thus, this table provides an additional indication – although the evidence is not strong – that state age discrimination protections helped boost hiring of older individuals for whom the FRA increased. Finally, note also that the estimates are always negative, and generally significant at the 5- or 10-percent level, for the effect of stronger remedies on hiring at the new FRA. Again, then, the combined evidence indicates that stronger age discrimination protections coupled with the increases in the FRA that were part of the 1983 Social Security reforms acted to keep people employed through age 65 and up to the new FRA.

8. Discussion and conclusions

In states with stronger protections against age discrimination in the labor market, older individuals were more responsive to the 1983 Social Security reforms that lowered early retirement benefits and increased the full retirement age (FRA). Specifically, where the state law applies to small firms not covered by the ADEA, for cohorts affected by the reforms employment increased at age 65 and then subsequently declined at the new FRA, consistent with shifting the exit from employment from 65 to the FRA. Similar changes in employment at age 65 and the new FRA were evident where state laws provided stronger remedies (compensatory or punitive damages). In states with stronger remedies, full-time employment of those aged 62 and over also increased. And similarly, echoing the employment results, benefit claiming shifted more from age 65 to the new FRA in these states. This evidence is broadly consistent with the idea that stronger state age discrimination protections enhanced the effects of the Social Security reforms, complementing the supply-side incentives created by these reforms by reducing demand-side barriers.

Further evidence on the reduction of demand-side barriers comes from studying employment transitions and hiring. Specifically, for 65 year-olds for whom the FRA increased, stronger state age discrimination protections appear to enable labor market transitions that permit people to remain at work, through either moving to a new employer or re-entering employment. If, as some of the results suggest, stronger age discrimination laws increased hiring of those older workers who were trying to work longer in response to these Social Security reforms, then these stronger age discrimination laws may make extended work lives more viable by boosting transitions to bridge or partial retirement jobs, in contrast to older workers simply staying a bit longer in their career jobs. At the same time, we do not want to overstate the evidence. In our view the preponderance of the evidence points this way, but some of the results are less consistent with this interpretation. It may be hard to get firmer answers until we have more evidence on people for whom the FRA has increased.

The employment and hiring 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.[44] 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. And working longer should also have positive ramifications for old-age support from private savings, by increasing earnings on these savings and reducing the period in which retirees rely on these savings.

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 – such as the lower actuarially adjusted benefits if they claimed prior to the FRA – 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.

[43] This is explained in the notes to Table 10, discussed next.

[44] The adjustment may not be actuarially fair for sub-populations. Duggan and Soares (2002) argue that the gender-neutrality of the actuarial adjustment coupled with lower life expectancy of men implies that the downward adjustment in benefits for men who claim benefits early is too small, yielding “actuarial premia” for them.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2013.09.006.

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