Public Pensions and Retirement: Evidence from the Railroad Retirement Act

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Abstract

This paper develops early estimates of how public pensions affect retirement timing by examining the Railroad Retirement Act of 1937, which replaced private railroad pensions with a national program similar to Social Security. Leveraging various samples of linked Decennial Census records between 1910-1940, the analysis compares male labor force nonparticipation by previous industry, year, and age. Higher benefits led to earlier retirement, largely driven by exit at age 65. Exploiting newly progressive benefits, the elasticity of nonparticipation at ages 65-69 is 0.55, which is large relative to findings in modern settings but consistent with contemporary elderly transfer programs.

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Old Age and Survivors Insurance (OASI, henceforth Social Security) provides roughly 30 percent of elderly income in the United States (SSA, 2022a) and costs the federal government over $1.1 trillion in 2022 (SSA, 2022b), but population aging and low elderly labor force participation (LFP) threaten the future of the largest U.S. Social Insurance program, with the trust fund forecasted to be depleted by 2035 (SSA, 2022b). Proposed solutions often feature reductions in benefits, but the degree to which this affects future insolvency depends on how labor supply and claiming, or taxes collected and benefits paid, responds.[1] Yet, the relationship between Social Security benefit changes and retirement has proven difficult to uncover because of the strong link between benefits and wages (Moffitt, 1987; Coile, 2015), little within-cohort variation in the rules (Krueger and Meyer, 2002), and reforms based typically on marginal changes to the rules that are long-anticipated by retirement age.

This paper estimates the effect of changes to public pensions on retirement by investigating the Railroad Retirement Act (RRA) of 1937, which established a national pension comparable to Social Security for an industry comprising roughly 7 percent of 1920 male non-agricultural employment (Ruggles et al., 2021).[2] The national program subsumed private railroad pensions, crediting up to thirty years of prior work *ex-post* under a more generous formula. For many nearing retirement age, the RRA unexpectedly increased monthly benefits on the order of 30-50 percent, a magnitude far exceeding any modern U.S. public pension reform.[3] In urging President Franklin Roosevelt to sign the legislation, Senator Robert Wagner – member of Roosevelt's Committee on Economic Security and contributor to much New Deal legislation – argued it would provide a testing ground for Social Security (Graebner, 1980, p. 153), ultimately passed in 1935.

The current state of Social Security is in part the result of a century-long trend towards less participation of elderly men in the workforce (Figure 1 panel (a)). The empirical analysis begins by estimating the industry-wide affect of being a railroad worker on LFP after the

[1] A list of proposed changes to benefits is provided here: https://www.ssa.gov/oact/solvency/provisions/benefitlevel.html#B5

[2] The first RRA was passed in 1934 and subsequently struck down by the Supreme Court in *Railroad Retirement Board V. Alton Railroad Company* (1935). This paper focuses on the revised legislation of 1937. The RRA covered nearly all workers of railroad firms engaged in interstate commerce (see Appendix B).

[3] From 1931-1940, monthly benefits among railroad pensioners increased by roughly 30 percent. This partly reflects changes in who was eligible; I estimate the increase for a worker who would have qualified before the RRA was larger, around 50 percent. The nominal 1940 average monthly benefit ($65.60) was over three times the amount given for elderly public assistance (≈ $20), Social Security for a worker only (≈ $22), and median rent (≈ $18), and nearly double that of Social Security for a worker and spouse (≈ $36) (RRB, 1940; Carter et al., 2006 Series Bf649-662; Series Bf461-475; Ruggles et al., 2021). Average railroad wages were $159.42 (ICC, 1942 p. 59).

RRA, and uses these estimates to quantify how much of the aggregate, eleven percentage point decline in the 1930s is explained by nationalizing railroad pensions. I then estimate elasticities of nonparticipation, or how responsive retirement timing is to changes in benefits. Both analyses use research designs that account for the endogenous relationship between wages, benefits, and retirement by leveraging various comparisons across industry, age, and year. The elasticity estimates also use the switch from flat to progressive benefits, which weakly increased benefits for nearly all workers but varied in the percent increase. The paper proceeds by using the elasticity estimates to discuss the likely impact of Social Security benefit expansions in explaining LFP declines in the 1950s.

I summarize the development and structure of private railroad pensions, the RRA, and changes to pension incentives in Section I. I then develop testable predictions from a forward looking model of pensions and retirement applied to the structure of these benefits (Stock and Wise, 1990). The RRA is expected to induce many to retire earlier, most retirement should occur at age 65, and newly progressive benefits should induce lower wage workers to retire earlier. The model also highlights why cross-wage comparisons of retirement will likely be too large. An attractive feature of early railroad retirement benefits is the simplicity of the benefit structure, which contains little intertemporal substitution incentives and implies estimates largely represents income effects.[4]

Section II describes the analysis sample, which is based on recently publicly available full count Decennial Censuses (Ruggles et al., 2021). One limitation preventing earlier research on the RRA is that an individual's pre-retirement primary industry is generally unobservable. To address this challenge, I leverage recent advances in historical Decennial Census record linkage (Helgertz et al., 2020) to measure LFP a decade after industry for the population of railroad workers (who are linked), appending 1920-1930 and 1930-1940 linked samples to study nonparticipation before and after the RRA. Various sensitivity checks establish the link between nonparticipation and pension receipt, show results are not an artifact of linkage error, and are robust to other linkage algorithms (Abramitzky et al., 2020). Importantly, the cohorts studied were either too young or old to have reached eligibility *during* the Great Depression, but old enough to benefit from strong seniority rights to employment during the economic downturn (Ekern, 1934).

[4]Little dependence of benefit amount on retirement date (after 64) greatly simplifies the relationship between wages, age, and optimal retirement timing. Growth in monthly benefits is roughly equivalent to growth in expected pension wealth, so that estimates approximate wealth elasticities (Moffitt, 1987), and many of the distinctions between current and future benefits shown to be important for studying more complex benefits (Samwick, 1998; Friedberg and Webb, 2005; Coile and Gruber, 2007) are largely irrelevant in this setting.

I describe the age-specific difference-in-differences research design for the industry-wide analysis in Section III. The semi-parametric specification compares LFP of railroad employees in 1940 relative to 1930 (first difference), relative to other industrial pension-covered "control" workers (second difference), and for each age 50-74 relative to 64.[5] The results (Section IV) show that the RRA led to large LFP declines only at ages 65 and older – *directly explaining* at least 12 percent of the previously unexplained 1930-1940 population-level decline ages 65-74 (Fetter and Lockwood, 2018) – while null results at ineligible ages supports internal validity. Further analyses suggest most labor force *exit* occurred at age 65, consistent with expected patterns. Existing differences in 1930 only exist starting at age 70, which is the common age of compulsory retirement on pre-RRA private railroad pensions.

The previous results represent a combination of new eligibility for some and varied benefit increases for others. In Section V, I estimate elasticities relating retirement to benefit changes for a restricted sample of likely pre-RRA pension-eligible workers using a cross-sectional design.[6] The independent variable, benefit growth, is estimated using 1939 wages (in the 1940 Census) and the pre and post-RRA benefit formulae, interacted with railroad status. The specification includes granular wage-bin fixed effects, limiting coefficients to be based on comparisons between railroad and control workers with similar earnings. I measure LFP in 1940 for those who worked in 1939 (i.e., 1-year retirement hazard), with various checks confirming this restriction proxies well for the *age of exit*. The semi-elasticity at age 65 is roughly .18, which is large, accounting for roughly 67 percent of the observed hazard at 65, while elasticities at higher ages are generally small and insignificant. In other words, low wage workers retired earlier and systematically at age 65. Further, the elasticity is almost 50 percent larger when excluding control workers and wage dummies, consistent with the expected bias. Under weak assumptions I relate these estimates to the elasticity of labor force nonparticipation for workers ages 65-69, which is also large, around .55.

The unexpected nature of the RRA resembles many of the largest expansions to Social Security that occurred in the 1950s (Moffitt, 1987). Real benefit levels increased by almost

[5]The research design can be thought of as a generalized triple differences comparison, where the third difference (pension-eligible ages) is measured continuously. Section II details the procedure to select control workers from non-railroad industries that had broad pension coverage in the late 1920s. The result is a group of workers in public utilities and certain manufacturing industries. None of the results are sensitive to the particular choice of comparison industries.

[6]The most important factor determining pre-RRA eligibility was length of service. The defining aspect of this sample is the use of an additional set of linkages back to 1910 to define a set of individuals who had likely worked in the railroad industry for 30 years or more in 1940, well above the requirements under most pensions (see Section I).

100 percent over this decade while male LFP 65 and older declined substantially (Figure 1 panel (a)). Consistent with shifting retirement earlier, Figure 1 panel (b) plots the retirement hazard (difference in LFP by age) and shows that much of this decline is attributable to a more pronounced spike in retirement at age 65 in 1960 relative to 1950. In Section V.D, I use the elasticities to estimate that between 65-77 percent of the increased claiming among eligible men 65-69 in the 1950s is attributable to benefit increases.

This paper provides a bridge between elasticity estimates from transfer programs structured quite distinctly from Social Security in the first half of the 20th century (Costa, 1995; Friedberg, 1999) with those based on changes to Social Security benefits in later periods (Krueger and Pischke, 1992; Gelber et al., 2016). While the first set of estimates suggest a substantial role for pensions in driving nonparticipation, the second often suggest little. The results indicate that declining responsiveness survives comparisons of elasticities across only similarly incentivized programs, while the estimates are some of the first for a Social Security-like program based on designs that compare across industry, leverage benefit increases of a much larger magnitude, and are during a time in which other incentives to retire at age 65, such as Medicare, do not exist.

The analysis of *retirement timing* in 1940 improves our understanding of when retirement began to spike at various ages (Figure 1 panel (b)) and how timing interacts with previous labor market attachment and earnings levels during a period in which information on retirement date and prior earnings is scarce. The results show that railroad retirement benefits led many to retire at 65 when they had a strong employment relation and also that the modern, positive relationship between earnings and retirement timing (Li et al., 2008) existed in 1940, at least in this industry.[7] While the literature has focused the rise of retirement at 65 after the Social Security Act, compulsory retirement provisions under the RRA (also at age 70) provides one explanation for the large spike in the 1940 retirement hazard at age 70 (Figure 1 panel (b)). I highlight that forward looking economic models of retirement typically applied only to modern settings do quite well at predicting retirement behavior in historical settings.

The presence of a large, existing retirement spike in 1930 railroad nonparticipation at

[7]Retirement in the Census first spiked at age 65 in 1940 due to public assistance (Fetter and Lockwood, 2018), and first spiked at age 62 in 1970 after early retirement was legislated for men (Burtless and Moffitt, 1986). More recent evidence suggests changes in retirement ages lead to both later benefit claiming (Song and Manchester, 2007) and retirement (Behaghel and Blau, 2012; Mastrobuoni, 2009). Earlier work found higher wages are associated with delayed retirement (see Mitchell and Fields, 1981 (1981, p. 47) for a review).

age 70 also expands our understanding of early 20th century U.S. industrial pensions. While we know much about the development of industrial pensions (e.g., Haber, 1978; Williamson, 1997) and evidence from surveys in the 1930s suggests the importance of pension income (Moen and Gratton, 1999), little evidence of how early pensions affected retirement exists (see Alter and Williamson (2018) for an exception). As Figure 1 panel (b) shows, the 1930 *aggregate* retirement hazard is highest *at age 70* (as is the case in 1920). This paper is perhaps the first to provide an (incomplete) explanation of 70 as a focal retirement age in the pre-New Deal period.

I Railroad Pensions: Background and Expected Retirement Response

I.A Private Railroad Pensions

U.S. private pensions originated in the railroad industry in the 1870s (RRB, 2018).[8] In 1900, the Pennsylvania Railroad (PRR) set a precedent for the structure of ensuing industrial pension plans by including universal coverage of employees, a compulsory retirement age, and no individual contributions (Williamson, 1997).[9] By 1931, 84 plans covered over 90 percent of railroad workers.

The rise of railroad pensions coincided with an expanding industry characterized by a rapidly increasing and aging workforce. Employment rose from roughly 620 thousand in 1900 to 1.6 million in 1920, when industry revenue peaked (excluding WWII) (Ruggles et al., 2021; Carter et al., 2006; Series Df927-955).[10] Technological advancements in the 1920s (Graebner, 1980, p. 154) coincided with the rise of automobiles and secular decline in railroad passenger traffic (Thompson, 1993, pp. 63-64), resulting in layoffs. Union negotiated seniority rights

[8]Consistent with economic models of pension provision (Lazear, 1979), plans arose in part due to continued LFP of the elderly, perceptions of lower productivity relative to younger workers, and the strong association between age, service length, and earnings (Graebner, 1980). J. A. Gordon, a former president of the Detroit, Toledo, and Ironton, articulated the issue, "I should hate to see railroads adopt the Ford policy of discarding without pension employees who have grown old... because it was possible to obtain younger and huskier men do a bigger day's work. This may be efficiency, but Lord save the industrial world from such efficiency!" quoted in Railway Age (1921, p. 407) from the Wall Street Journal.

[9]Inclusion of compulsory retirement provisions was commonplace until being outlawed under amendments to the Age Discrimination in Employment Act in 1978 (Dorsey et al., 1998 p. 33).

[10]My definition of railroad workers is predominantly comprised of workers who were assigned the 1950 industry code 506 but slightly adjusted to include some occupations likely covered under the RRA in other industries (see Appendix B for details).

ensured workers with longer service maintained their rights to employment (Ekern, 1934; Harbison, 1940). Due to the common practice of age limits in hiring (Haber, 1978), those with longer service were also older, so layoffs fell largely on younger workers. To illustrate the scope of aging in this industry, I predict the number of individuals 65-80 who had worked for railroads using the 1900, 1910, and 1920 complete count Decennial Censuses.[11] Figure A.1 shows that, between 1920 and 1940, the stock of individuals 65-80 currently or formerly working for railroads more than tripled. Population growth only explains roughly 50 percent of the increase.

Figure 2 shows that trends railroad pension in recipiency (panel (a)) and expenditure (panel (b)) tracked the rapidly aging stock of current and former railroad workers. Figure A.3 plots the recipiency rate, or share of the predicted elderly stock receiving pensions. While many workers never qualified for pensions, the time series suggests the recipiency rate had been fairly constant for at least 15 years leading up to the Great Depression.[12] The success railroads had found funding pensions out of general revenues proved insufficient during the Depression, leading to benefit cuts and further retention of older workers at the cost of their younger counterparts.[13] By the early 1930s, supporters of national railroad retirement

[11] I include only male workers to keep the prediction consistent with the empirical analysis, which requires restricting focus to men (this is not quantitatively important as there were few women in the industry, particularly at older ages). In each Census I take the population of railroad workers 49-64 and predict populations in ensuing years using age-specific 1-year mortality probabilities for 1900, 1910, and 1920 (Bell and Miller, 2005) and linear interpolations (by age) for interceding years. Each series thus begins 16 years after population measurement. Ideally, the prediction would target the population 65 and older, but predicting higher ages requires a longer horizon between observation and prediction. The count will be somewhat overestimated due to transitions out of the railroad industry. Figure A.1 shows the predicted values line up quite well across overlapping years, suggesting the magnitude of these issues are (jointly) small.

[12] There are two caveats. First, the most comprehensive source for total private railroad pension recipiency is Latimer (1932), which shows recipiency for a non-exhaustive set of railroads (firm names are redacted). The focus of the study was on "formal" pensions – essentially those with rules stipulating the same benefits for individuals of equivalent age-service – which were often the largest firms. Indirect evidence on how well this sample captures all pension recipiency may be obtained from comparing the Interstate Commerce Commission expenditure series to Latimer (1932) in Figure 2, the former which is based on all railroads engaged in interstate commerce. They are close to identical in overlapping years. Second, as described in the previous footnote, the denominator is missing the count of individuals over 80, which is not likely to be too important for the overall trends.

[13] With the exception of 6 plans, none had a designated trust fund (Railway Age, 1934 pp. 144-146). Missing information for the late 1920s and much of the 1930s leaves unclear the precise effect of the Great Depression on pension roles and benefit levels (Figure 2, panel (a)). One account is that the number of pensioners began to decrease after 1932 (Silverman and Senturia, 1939) although Sass (1997) finds that suspensions or terminations were "surprisingly limited". Indeed, panel (b) indicates expenditure declined only *after* the first RRA was passed, well after the most extreme period of economic downturn. The case of the

legislation emphasized three benefits: unemployment reduction, a testing ground for Social Security, and overwhelming public interest (Graebner, 1980, p. 153).

I.B The Railroad Retirement Acts

The Railroad Retirement Act (RRA) of 1934 set up the first federally administered and financed defined benefit public pension program for nongovernmental employees in the U.S. The Act was declared unconstitutional in *Railroad Retirement Board V. Alton Railroad Company* (1935), and the failure gave "the ideology of Social Security formal sanction... proponents of retirement legislation talked less about efficiency, economy and unemployment relief than about Social Security and the needs of older workers" (Graebner, 1980 p. 163). In 1937 a revised act established the program. All rules described below are according to that legislation.[14]

The RRA covered most employees of railroads engaged in interstate commerce (see Appendix B for details). The newly formed Railroad Retirement Board (RRB) assumed the claims of almost all existing "Pensioners" and paid benefits to retirees who had worked at least some time in RRB covered employment beginning January 1st, 1937 (termed "Annuitants").[15] I refer to these cohorts as pre and post-RRA claimants so as not to obfuscate the fact that both groups received annuities. Future benefits were financed by equivalent payroll taxes on employers and employees of 2.75 percent up to a maximum of $300 per month, but the Treasury funded the immediate transfer of existing pensions and retirement of the stock of elderly workers (the system is therefore "pay as you go").[16]

In stark contrast to the Social Security program established in 1935, the RRA credited up to 30 years of service before 1937, permitting "the immediate retirement, on relatively high annuities, of large numbers of aged workers still employed" (Silverman and Senturia, 1939 p. 3). By July, 1937, nearly 50,000 pre-RRA recipients had been taken over by the

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Pennsylvania Railroad, the largest railroad employer, is illustrative; they did not cut pension benefits until April 1, 1932 (The Pennsylvania Railroad Company, 1933, p.10).

[14]See Schreiber (1978) for a detailed description of the rules and legislative histories for each act. The RRA followed in an already lengthy tradition of federal railroad labor legislation (e.g., the Adamson Act of 1916 and Railway Labor Act of 1926).

[15]Railroads maintained some pension expenditure on their balance sheets – generally for auxiliary programs for executives – but it was quite small, constituting roughly 3 percent of RRB expenditure in 1940 (RRB, 1940; ICC, 1940). There were also survivor and death benefits, together constituting less than 2 percent of recipiency and roughly 1 percent of expenditure in May, 1939 (Silverman and Senturia, 1939 p. 10).

[16]This is similar to early Social Security beneficiaries, for whom the present value of pension wealth was much higher than lifetime contributions (Moffitt, 1984).

board and over 140,000 individuals received annuities under the act in 1940 (Figure 2, Panel (a)). Figure A.3 shows that, between 1931 and 1940, the estimated share of elderly railroad workers receiving benefits rose from roughly 45 percent to 83 percent.

I.C Changes to Pension Parameters

Table A.1 provides a digitized version of a Bureau of Railway Economics study detailing the parameters of all railroad pensions in 1932 (reproduced in Railway Age (1934, pp. 144-146)).[17] The first column shows 1928 employment (ICC, 1928) matched to plans (see Appendix B for details). Because the empirical analysis to follow focuses on the entire railroad industry, the table is ordered according to employment and descriptive statements below are phrased in terms of the affected railroad employment share.

As with benefits under the RRA and Social Security, industrial pensions were defined benefit functions of average monthly wages ($\overline{w}_i$) and service years (S_i) for workers age (a_i) above the eligibility age ($\underline{a}$) who had acquired sufficient S_i to meet the service requirement ($\underline{S}$). The monthly benefit is the product of a benefit factor $k(\overline{w}_i)$ and S_i:

$$B(\overline{w}_i, S_i, a_i) = (S_i \times k(\overline{w}_i)) \times \mathbf{1}\{S_i > \underline{S}\} \times \mathbf{1}\{a_i > \underline{a}\} \tag{1}$$

At least three important features are worth noting. First, benefits were not progressive (i.e., featured a linear replacement rate of $\overline{w}_i$ in S_i) most commonly with $k(\overline{w}_i) = 0.01 \times \overline{w}_i$, and the work period for which $\overline{w}_i$ is computed (not shown) is the 10 years preceding retirement date in all but two plans.[18] Second, compulsory retirement provisions affected roughly 70-72 percent of workers, 99 percent for whom the age was 70. Third, $\underline{a}$ (and associated $\underline{S}$) are listed for two "types" of pensions – age and disability.[19] Both types were often indistinguishable in their purported goals and largely facilitated "retirement" in the modern sense of a "permanent withdrawal from work based on an expectation that financial resources would meet future needs" (Moen and Gratton, 1999 p. S28).[20] Roughly 40 percent of workers

[17]This section describes the most important changes to pension incentives from the private plans to the RRA; see Appendix C.I for further details.

[18]Some plans had maximum and minimum monthly amounts, but these were usually not binding and I abstract away from their consideration.

[19]Of the roughly 50,000 pensions who would be taken over by the Railroad Retirement Board (RRB) –the new federal agency tasked with administering railroad retirement benefits – 56.4 percent were reported as retired under disability, 41.9 percent under age provisions, and 1.7 percent under service provisions; a third, rarer type of pension (RRB, 1938 p. 97).

[20]According to the RRB, "In some plans disability pensions were used to effect age retirements at earlier ages

faced $\underline{a} = 65$ with another 12 percent facing $\underline{a} < 65$ (often 60 or 61). Plans often contain $\underline{S}$ but no associated $\underline{a}$, indicating many of the remaining 48 percent had access to some type of early pension, but the particular age was somewhat more discretionary. These features underpin the predictions later in this section and inform sample restrictions intended to define a pre-RRA pension-eligible set of workers in Section V.

The RRA set $\underline{a}$ at 65 while imposing compulsory retirement at age 70 and removing minimum service requirements ($\underline{S} = 0$). Figure 3 shows the (typical) old and new pension benefit formulae (for eligible individuals). The new adjustment factor $k(\overline{w}_i)$ was progressive in $\overline{w}_i$, given by 2 percent of the first \$50, 1.5 percent of the next \$100, and 1 percent of the balance. S_i was capped at 30, with a maximum monthly benefit of \$120. Figure 2, Panel (b) shows that railroad pension expenditure increased dramatically beginning in 1937, so that from 1931-1940 real average monthly payments per recipient increased by roughly 30 percent even as recipiency almost tripled (panel (a)). In Section V I estimate the increase in average benefits for individuals who would have been eligible under private plans (generally corresponding to higher S_i and w_i) was higher, around 50 percent.

I.D Expected Effects on Retirement Timing

Almost all railroad workers on the margin of retirement ages experienced sudden and unexpected increases in expected pension wealth (discounted lifetime stream of benefits). Because early beneficiaries paid little in contributions, the RRA should induce earlier retirement (Coile, 2015; Blinder et al., 1978). Further, because the RRA was unexpected "well into the early 1930s" (Graebner, 1980, p. 156) and occurred close to retirement ages for many, life cycle models predict LFP responses to be large (Moffitt, 1987; Krueger and Meyer, 2002).[21]

than the age retirement provisions of the plans allowed. Other plans provided for disability retirements only, and all retirements under such plans were necessarily reported as due to disability" (RRB, 1938 p. 97). The idea that disability benefits facilitated retirement is also supported by Alter and Williamson (2018), who study the Pennsylvania Railroad pension and find that most retirements under disability at age 65 were due to employee request, indicating a choice over work. Further, the requirement of $\underline{S}$ indicates a reward for service more akin to voluntary retirement, while $\underline{a}$ is most commonly 65, which already had a long history denoting "retirement" and "the old" (Costa, 1998a, p. 11). This is not to say that health was not important in the decision whether to work, only that access to these benefits appears to have been based on similar criteria to age pensions. At the same time, it should be recognized that some plans likely had further requirements to prove disability. Railroad retirement only offered quite restrictive disability benefits until 1946, and Social Security did not include disability insurance until 1956.

[21] Evidence suggests employees have imperfect knowledge of their pensions (Gustman and Steinmeier, 2004; Chan and Stevens, 2008), there are several reasons to expect railroad workers near retirement ages possessed better knowledge: First, as argued in other contexts, simpler pension rules are easier to understand (Asch

The great majority of individuals who will be studied in 1930 had claimed their pensions too early to have been able to take into account benefit cuts of the early 1930s into their retirement decision, while those under 68 in 1940 were too young to have been age-eligible before the RRA.

To fix ideas, a typical railroad worker affected by the RRA and for whom retirement behavior will be examined is one in their late 50s and early 60s during the Great Depression who had worked for decades expecting to receive a pension according to relatively constant rules. While direct evidence on consistency of rules is difficult, if not impossible, to produce, the relatively flat recipiency rate (Figure A.3) provides evidence of minimal changes. They have seniority rights to employment allowing them to keep working during the economic collapse and earn sometime between 1935-1937 of a federal entitlement to much higher benefits when they turn 65.

Many other individuals with insufficient service to qualify for private pensions now expected benefits based on work retroactively. Because wages were positively correlated with service (RRB, 1938 p. 102), any cross wage comparisons across all railroad workers of retirement ages will conflate changes to eligibility and generosity. Given the near ubiquitous adjustment factor across firm plans of $k(\overline{w}_i) = 0.01 \times \overline{w}_i$, a more generalizable parameter is how retirement responds to changes in benefits for those *who would have been eligible* under private plans. As such, the ensuing theoretical predictions are focused on those previously eligible at age 65, while Section V discusses sample restrictions intended to define this set.

Let the pre and post-RRA benefits (for previously eligible retirees; $a_i \geq \underline{a}; S_i \geq \underline{S}$) depicted in Figure 3 be given by $B_{priv}(\overline{w}_i, S_i) = k_{Priv}(\overline{w}_i) \times S_i$ and $B_{RRA}(\overline{w}_i, S_i) = k_{RRA}(\overline{w}_i) \times S_i$, where a_i is now omitted an argument. I conceptualize the shock as the percent change to expected monthly benefits:

$$\%\Delta B(\overline{w}_i, S_i) \equiv \frac{B_{RRA}(\overline{w}_i, S_i)}{B_{Priv}(\overline{w}_i, S_i)} - 1 = \frac{k_{RRA}(\overline{w}_i)}{k_{Priv}(\overline{w}_i)} - 1 \tag{2}$$

An attractive feature of (2) is that the linearity of both $B_{RRA}(\overline{w}_i, S_i)$ and $B_{Priv}(\overline{w}_i, S_i)$ imply the percent change in benefits is independent of S_i and can be written as $\%\Delta B(\overline{w}_i)$. Further, as argued in Appendix C.II, $\%\Delta B(\overline{w}_i)$ is roughly equivalent to the percent change in pension wealth under reasonable assumptions regarding wage growth after age 65.

In Appendix C.II I develop a set of predictions from forward looking measures of pen-

et al., 2005). Second, most workers were working when their pension had begun. Third, the nationalization of railroad pensions was discussed in trade newspapers (Railway Age, 1934 pp. 144-146). Finally, older workers are more likely to know about their benefits (Gustman and Steinmeier, 2004).

sion incentives and retirement advanced in the literature applied to the structure of pre and post-RRA railroad pension benefits. These are intended solely as a guide to inform the reduced form analysis. I consider two measures advanced in the literature; the "Peak Value" (Coile and Gruber, 2007; Friedberg and Webb, 2005; Asch et al., 2005) and the "Option Value" (Stock and Wise, 1990). The former compares the difference between pension wealth at its maximum date to that of today, while the latter is a structurally derived measure contrasting the gains from retiring at any given future date from continuing to work. The disutility of work highlights the difficulty in estimating how retirement responds to benefit increases when the magnitude of the increase depends on wages, as nearly all reforms to defined benefit pension benefits do. Little dependence of benefit amount on retirement date (once aged 65) greatly diminishes any intertemporal substitution incentives (Coile, 2015) because pension wealth after the RRA is maximized for nearly all workers at 65 (benefits are "marginally unfair"; Burtless and Moffitt (1986)). This has two implications. First, the structure yields much simpler predictions over how the timing of retirement should relate to age. Second, because working longer is unlikely to change the expected monthly benefit much in this context, estimated coefficients should predominantly represent income effects.[22]

- *Predictions*

The option value framework appears to be more realistic in this setting, so I focus on the following three predictions that it yields: First, the RRA should result in a greater density of retirement at age 65 while also still lead to clustering at either end of the eligibility range (65 and 70). Second, the spike at 65 should be driven by lower wage workers who experienced a higher relative increase in their benefits. Third, any systematic relationship between the disutility of work and wages will bias comparisons of retirement across workers of differing wages (benefit). Wages and disutility are expected to be negatively correlated, indicating estimates of retirement responsiveness will be too large.[23] Another conclusion to come from the model, untestable with the data at hand, are that replacement rates were too low to rationalize retirement before age 70 at reasonable levels of disutility before the RRA. Note that these predictions are also borne out for ineligible workers, who are included in the first analysis linking railroad status to patterns of nonparticipation. The following section

[22]This point is strengthened by recent work showing that substitution effects are small and insignificant (Gelber et al., 2016), even in contexts in which changes to substitution incentives are quite large.

[23]Higher wages may be the outcome of unobserved preference for work. In the context of retirement, the disutility of work also likely includes health-related considerations; labor-intensive jobs may be more difficult to continue to perform at older ages or lower wages may be the result of poorer health.

provides some preliminary evidence on these predictions.

I.E Preliminary Evidence from Administrative Aggregates

Figure 4 panel (a) plots the age distribution of first benefits for cohorts of pre and post-RRA claimants.[24] The strict earnings test implies a close link between claiming and retirement – there is little scope for claiming while working and little incentive to not work and not claim – so it is reasonable to view claiming as a close representation of retirement timing (a point I provide direct evidence on in Section V). Pre-RRA retirement spikes first at age 65, declines somewhat between ages 65 and 70, and then exhibits a much larger spike at age 70 (roughly 30 percent). While some of the spike is driven by workers who were not eligible to retire prior, the simulation results also indicate that benefits were likely insufficient for eligible workers to choose to retire before they were forced to.

For cohorts claiming in the 1940 fiscal year under the RRA, the magnitudes of the spikes reverse, with roughly 40 percent now claiming at age 65. The simulation predicts that much of the new spike at age 65 should also be due to benefit increases among existing eligibles. To provide initial evidence on the relationship between benefit changes and retirement timing, I digitize two tabulations produced by the RRB on recipients of annuities in June, 1938, separated by cohorts who claimed before or after the RRA (RRB, 1938 pp. 90, 104). These series' contain average credited earnings, average monthly benefits, and average claiming ages, as well as the number of annuitants who comprise each cell. I first demean the (weighted) average for each claiming cohort and then difference the data.[25]

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[24]Early RRB publications provide the most comprehensive information on claiming age for pre-RRA pensioners. 90 percent of these pensioners had retired between 1924-1935. This figure combines age and disability benefits (see discussion in Section I.A). Figure A.2 panel (a) reproduces this pdf along with each for age and disability, and shows a larger spike in age benefits at 70 and disability benefits at 65 (with still around 65 percent of the latter claiming at ages 65 or older).

[25]For pre-RRA claimants, the aggregates are at the level of 21 Federal Coordinator of Transportation (FCT) occupation codes. For post-RRA they are at the level of 102 Interstate Commerce Commission (ICC) occupation codes, which aggregate to FCT codes (RRB, 1938 pp. 156). I collapse post-RRA average benefits and claiming ages to the FCT level, using claimant counts as weights, yielding a panel at the FCT code-level drawn from roughly 45,000 and 50,000 records of pre and post-RRA, respectively. As noted in Appendix C.I, benefits taken over by the RRB were adjusted to account for Depression-era benefit reductions. I use the adjusted amounts because interest lies in comparing expected benefits *at retirement*. I de-mean each cohort because the protracted litigation led many workers already aged 65 plus to remain working and delay claiming, either due to pension cuts or to holdout for more generous benefits. Indeed, the average age of claimants in the 1936-1937 fiscal year was 70.3, while it was 67.5 for the 1940-1941 year (RRB, 1941 p. 212). Removing the average age is a rough attempt to isolate variation in claiming age across benefit amount. Ideally the figure would use information on average wages, benefits, and retirement

Figure 4 panel (b) shows the expected negative relationship between percent change in benefit levels within FCT occupation and the change in retirement age (in years), where marker size denotes the total annuitant count (pre and post-RRA) for each occupation. Note that this is the percent change within occupation, not individual, and is in part comprised of new eligibility. Superimposed is the result from a weighted least squares regression of the form: $\Delta(\text{Retirement Age})_o = \alpha + \beta \times (\text{Benefit Percent Change})_o + \varepsilon_o$, where o indexes occupation group. The estimated effect of -1.86 years implies a 1 standard deviation percent increase to benefits (18.9 percent) led to earlier claiming by around four months ($-1.86 \times .189 \times 12 \approx 4.2$). The small sample size and shifting sample composition should caution interpretation of the precise magnitude. Nevertheless, this exercise documents the expected negative relationship between changes in expected pension wealth and retirement timing.

II Data

Researchers studying retirement during this period often turn to the Decennial Census of Population, the most comprehensive source for demographic and labor market information (e.g., Ransom and Sutch, 1986; Moen, 1987; Costa, 1995; Friedberg, 1999; Fetter and Lockwood, 2018). In principle, the availability of complete count Censuses allows an examination of retirement for the full population of railroad workers. However, a key limitation in the context of the RRA is that it is not generally possible to identify a worker's previous primary industry of employment when they are observed out of the labor force.[26] A second limitation is that no question asks whether an individual is covered by a pension or is receiving benefits.[27] This section describes the analysis sample for the first set of estimates engaging with nonparticpation in the railroad industry as a whole, which serves as the basis for a further-restricted sample used to estimate responsiveness to benefit changes, described in detail in Section V.

ages for those who retired in 1940 but I have not found this information in later publications of the RRB Annual Report.

[26] Among the approximately 2.8 million men ages 50-74 in 1940 who reported not being in the labor force, roughly 83 percent have either no industry reported, a non-industrial response, or a nonclassifiable industry.

[27] The 1940 Census asked about whether an individual received non-wage income in excess of \$50 in 1939, as well as their "usual industry" over the previous decade (irrespective of LFP). I use these questions to validate the link between exit and pension receipt and to rule out error due to linkage or temporary employment in Section IV.A. The 1940 Census also asked about deductions for Social Security or Railroad Retirement benefits, but this applies only for those who earned wages in 1939 and is thus not relevant for the cohorts I study.

II.A Linked Decennial Census Data

I address unobserved previous industry by using recent developments in Decennial Census record linkage and availability of complete count Decennial Census data for 1920, 1930, and 1940 (Ruggles et al., 2021), which facilitate comparisons of labor force outcomes ten years after observing a worker's industry. To develop my primary sample, I keep men in the labor force ages 37-67 in the base year (1920 or 1930), link to the following census year (1930 or 1940) using the publicly available linking algorithm provided by Helgertz et al. (2020), and measure LFP outcomes for individuals ages 50-74 in the later year.[28] I then stack the 1920-1930 and 1930-1940 linked samples generated from these links.

- *Treatment Group: Railroad Retirement Eligiblity*

I address the second issue – unobservable railroad pension coverage – by using the RRA legislation in conjunction with Railroad Retirement Board information on the number of workers with credited earnings in 1940 (RRB, 1941 p. 162). I compare the administrative counts of covered employment – workers who contributed payroll taxes – to employment totals in the 1940 complete count Census for reasonable choices of railroad industries and occupations. The totals align well, with 1940 Census employment among the selected industries and occupations comprising roughly 80 percent of the RRA credited employment count. I use these same codes to classify railroad workers in 1920 and 1930. Most workers are drawn from 1950 Industry code 506; see Appendix B for further details.

- *Control Group: Workers Covered by Other Industrial Pensions*

In 1930 the share of employment on railroads began declining at age 65 and declined markedly at 70 (see Figure A.5). Extensive pension coverage and compulsory retirement provisions described in Section I explain why pre-RRA retirement behavior of railroad workers was different than the average worker. They also suggest a natural control group – other industrial workers covered by private pensions – since provisions under those plans were similar

[28]The algorithm matches individuals by birth year ± 3 years to account for error in reported age over time. Since the analysis is focused on cohort comparisons of LFP, I consider the age in the later year of each link as the "true" age in the analysis. My preference is to use Helgertz et al. (2020) because it produces a higher linkage rate while performing similarly on various accuracy measures relative to other popular methods. I show in Appendix A.III, however, that results are not sensitive to using any of the algorithms provided by Abramitzky et al. (2019). See Helgertz et al. (2022) for a full description of the linkage methodology, as well as Bailey et al. (2020) and Abramitzky et al. (2019) for discussions of the tradeoffs and frontiers in historical Census Record linkage.

to those on pre-RRA private railroad plans, particularly regarding eligibility age.[29]

Appendix B details my procedure for classifying individuals as likely covered by other industrial pensions. The use of complete count Censuses indicates statistical power is not relevant for choosing these industries. I therefore focus on minimizing the probability of classifying workers falsely as covered by pensions rather than including as many covered workers as possible. I calculate total employment for industries that had pensions in 1930 and compare counts to estimated total pension covered employment by industry in 1929 provided by Latimer (1932), the most extensive survey of U.S. private pensions at that point. If Census employment is much larger (>3 times) I omit that industry. Utilities and a subset of manufacturing industries comprise the final comparison group, representing over 80 percent of total non-railroad industrial pension coverage in 1929 (Latimer, 1932, p. 47). In practice, the specific choice of these industries is inconsequential.[30] As with the railroad classification, I apply the same codes to classify these workers in both 1920 and 1930. Henceforth I refer to these industries as "control industries".

II.B Representativeness and Balance

I keep those individuals in the appended 1920-1930 and 1930-1940 linked samples who were in the labor force (employed or unemployed) in railroad or control industries in the base year, resulting in 956,391 men aged 50-74 when LFP outcomes are measured (177,031 of whom are 65-74). Following the literature standards (Abramitzky et al., 2020; Bailey et al., 2020), I estimate weights to make the sample representative of the population at risk of being linked.[31]

[29]Of 15 utilities pension plans that featured compulsory retirement, 11 were at the age of 70. Retirement among manufacturing industries was more often voluntary, but most plans still featured eligibility ages of 65 or 70 (Latimer, 1932, pp. 77-78). Public utility plans were typically more generous than railroads, with lower retirement ages, service requirements, and more generous formulae relating average earnings to benefits.

[30]Covered employment in railroads, utilities, and manufacturing industries combined represented over 95 percent of covered employment in 1929 (Latimer, 1932, p. 215). In Appendix A.IV I show the results are not sensitive to omitting any industry. I also show that patterns are generally consistent (and magnitudes are much larger) when simply comparing to all other non-agricultural male workers.

[31]I develop inverse probability weights based on covariates in the first linked year (i.e. 1920 for 1920-1930 links; 1930 for 1930-1940 links). I estimate 2 probit models on the population at risk of being linked – men ages 37-67 in the base year in railroad or control industries – where the dichotomous outcome variable y_i indicates a match. Covariates used include indicators for employment in railroads and utilities (manufacturing omitted) an indicator for employment (and another for unemployment in 1930, which is not available in 1920), dummies for 5-year age bins, and dummies for number of children, marital status, race (white versus nonwhite), 10-unit occupational income score bins, and urban status. I then obtain the

- Linked Sample

Table A.2 shows balance tests for a host of characteristics between the linked sample and population at risk of being linked in 1920, 1930, and overall. Column (1) gives the sample mean, column (2) the difference in the population, and column (3) the p-value for a test of equality. Given the large sample, it is unsurprising differences are statistically significant, but a few notable differences in the unweighted comparisons warrant further discussion. Individuals in the sample are more likely to be married, more likely to have kids, and have more kids conditional on having children. They are slightly positively selected on socioeconomic characteristics, with higher occupation scores and a higher probability of home ownership.[32] Columns (4)-(6) give the same for re-weighted tests using the weights described above. While many differences stay statistically significant across the samples, the size of the differences becomes small and economically insignificant (The largest difference is home ownership, wherein unlinked individuals are just 3.7 percent less likely to own a home). These tests validate the re-weighting procedure, and I use these weights in all analyses using linked data moving forward (in practice, results are not sensitive to weighting; see Appendix A.II).

- Covariate Balance Across Industry, Age, and Year

The literature suggests the importance of assessing whether measures of income, wealth, and family structure may potentially confound interpretations of the RRA as driving changes in retirement timing.[33] As discussed further in Section III below, the empirical analysis compares LFP across industry, over time, and by pension eligibility ages (65+) relative to those below. Covariates must therefore vary systematically along the intersection of these margins to confound the ensuing interpretation of causal effects. Table 1 presents (weighted) covariate means in column (1) for the full analysis sample. Column (2) shows the results from a series of descriptive triple-differences specifications that test for relative covariate differences

predicted probabilities $\widehat{p}_i$. The resulting weights are given by: $w_i = \left(\frac{1-\widehat{p}_i}{\widehat{p}_i}\right) \times \left(\frac{\bar{y}}{1-\bar{y}}\right)$.

[32] Occupational income scores give the median income by occupation according to 1950 levels. Wages were first recorded in the 1940 Census. Supplementary analyses in Appendix A.V show no evidence of cross-Census cohort changes in the probability of linkage by age between railroad and control workers.

[33] Many elderly individuals in the early 20th century could expect to live with (and depend on) their children for support after exiting the labor force (Williamson, 1997; Costa, 1998a; Gratton, 1996). The presence of children or a spouse may also represent different consumption needs or bequest motives, each which can affect the retirement decision. The level of non-pension wealth may also impact decisions over retirement (Imbens et al., 2001; Brown et al., 2010)

(measured in the base year) accross industry, period, and age (in the later year).[34] Only two are significant (dummy for white and occupation score) and the estimates are negligible relative to the means.[35] These tests also highlight the value of the research design – which uses cross time and cohort margins of comparison in addition to cross-sectional differences in industry – to embed falsifications tests for differential exit among groups where the RRA should have no effect.

- *Geographic Distribution*

Railroads had reached peak mileage by the 1930s (Carter et al., 2006, Series Df927-955). Figure A.5 panels (a) and (b) shows that the county share of railroad employment does not follow any distinct geographic pattern in either 1920 or 1930. Panels (c) and (d) show the same is true for control industries. The Adjusted R^2 from a regression of being a railroad worker on state fixed effects is 0.05 (on county fixed effects it is 0.18). Taken together, these patterns suggest comparisons are not likely to come from specific geographic areas that either had more generous elderly public assistance (Fetter, 2017) or were differentially affected by the Depression (Rosenbloom and Sundstrom, 1999), issues I discuss further in Section IV.

III Research Design: Comparing LFP by Railroad Status, Decade, and Age-Eligibility

Figure 5 plots LFP by age in 1930 and 1940 for railroad and control workers as measured a decade earlier.[36] Four patterns stand out: first, the 1930 gap in LFP between railroad and control workers only begins at the common pre-RRA compulsory retirement age of

[34]Specifically, for each covariate $x_{i,t}$, $t \in \{1920, 1930\}$, I estimate the following specification via weighted least squares (see FN 31 for weight calculations):
$x_{i,t} = \beta_0 + \beta_1 \times \text{RR}_{i,t} + \beta_2 \times \mathbf{1}\{t = 1930\} + \beta_3 \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_4 \text{RR}_{i,t} \times \mathbf{1}\{t = 1930\} + \beta_5 \text{RR}_{i,t} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_6 \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_7 \text{RR}_{i,t} \times \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \varepsilon_{i,t}$

[35]Table A.3 shows traditional mean comparisons between railroad and control workers, broken down by each Census year and each age group. To summarize whether there is evidence that these covariates jointly predict railroad status along the potentially confounding margin, I also regress railroad status on indicators for age 65 and older (in 1930 or 1940), for the later cohorts, their interaction, each covariate, and each covariate interacted with these dummies. The F-statistic for the set of coefficients interacting each covariate with post and age 65 plus is 0.21 (p-value=0.97), indicating no evidence that potential confounders vary with railroad status at margins not differenced out in the analysis below.

[36]See Appendix A.I for a discussion of changed to LFP measurement between 1930 and 1940. Gaps are slightly smaller when using a consistently defined (but poorer) measure of LFP and the patterns are quite similar (Figure A.13).

70.[37] Second, the gap in 1940 began at age 65, the initial age of eligibility under Railroad Retirement benefits. Third, control workers also exited differentially more at age 65 in 1940 relative to 1930. Finally, the patterns at pension-ineligible ages < 65 are quite similar in both years. The correspondence between pension rules as described in Section I and patterns in Figure 5 provide evidence of the link between pensions and retirement behavior as measured in the Census. The decline at 65 among control workers shows that other factors influencing retirement over the 1930s should be accounted for, while similar patterns at younger ages lend support to my choice of control industries.

The empirical specification estimates the magnitude of these gaps – exploiting changes to pension incentives under the RRA for railroad employees in the 1930s relative to those in the 1920s (first difference) and relative to control workers (second difference) at *every* age $a(i) \in [50, 74]$, relative to 64:

$$\begin{aligned}(\text{Not in LF})_{i,t} = \boldsymbol{\delta}_{RR(i)a(i)c(i),t-10} + \mathbf{X}_{i,t-10}\boldsymbol{\beta} + \sum_{a(i)<64} \pi_{a(i)} \times \text{RR}_{i,t-10} + \sum_{a(i)>64} \rho_{a(i)} \times \text{RR}_{i,t-10} \\ + \text{Post}_{1940} \times \left(\text{RR}_{i,t-10} + \sum_{a(i)<64} \mu_{a(i)} \times \text{RR}_{i,t-10} + \sum_{a(i)>64} \gamma_{a(i)} \times \text{RR}_{i,t-10} \right) + \varepsilon_{i,t-10}\end{aligned} \tag{3}$$

Where $(\text{Not in LF})_{i,t}$ indicates whether individual i was not in the labor force in 1930 or 1940, $\text{RR}_{i,t-10}$ indicates whether i was working for railroads a decade earlier, and Post_{1940} is a dummy indicating $t = 1940$. The coefficients of interest $\widehat{\gamma}_{a(i)}$ trace out the differential effect of being a railroad worker on labor force nonparticipation in 1940 relative to 1930, relative to workers in control industries, and at pension-eligible ages. Crucially, the $\widehat{\mu}_{a(i)}$ represent falsification tests for differential exit of railroad workers at post-RRA pension-ineligible ages, admitting graphical evidence analogous to tests for differential pre-trends from event study specifications in canonical difference in differences research designs. The $\widehat{\pi}_{a(i)}$ and $\widehat{\rho}_{a(i)}$ indicate whether there were any preexisting differences in exit at pension ineligible and eligible ages between railroad and control workers in 1930, respectively. In additional specification checks, covariates $\mathbf{X}_{i,t-10}$ include race, presence of children, marital status, and fixed effects for occupations or occupational income scores (all in year $t-10$), while $\boldsymbol{\delta}_{RR(i)a(i)c(i),t-10}$ include various levels of fixed effects for interactions between county,

[37]Because of greater coverage in the railroad industry and broad availability of benefits at age 65, we might expect an existing gap at ages 65 in 1930. However, as illustrated by discussion in the previous section and in Appendix C.II, replacement rates were likely too low to induce many to retire earlier than 70. Figure A.16 shows that, when compared to all U.S. non-agricultural workers, the differences in nonparticipation at ages 65-69 are positive and statistically significant (and the differences at ages 70 and above are larger).

age, railroad status, and period. Given stark county-level variation in Depression severity (Rosenbloom and Sundstrom, 1999) and elderly public assistance generosity (Fetter, 2017), I cluster at the county level (in $t-10$), but also present results clustered at the state level in Appendix A.II.

The empirical approach can be thought of as a generalized version of a triple differences design, with the $\widehat{\gamma}_{a(i)}$ and $\widehat{\mu}_{a(i)}$ representing the difference between the difference-in-differences estimates at $a(i)$ and at age 64. Identification requires that the only factor differentially affecting LFP of railroad workers relative to other pension-covered workers, relative to the same age groups in 1930, and relative to the same comparisons at pension-ineligible ages was differential pension incentives under the RRA. Tests for zero relative differences at ineligible ages ($\widehat{\mu}_{a(i)}=0$) go a long way towards supporting the identifying assumption. Nevertheless, Section IV and Appendix A present evidence to rule out a host of alternative explanations.

IV Results: The Effect of the RRA on Labor Force Participation and Retirement Timing

Figure 6 plots the coefficient estimates from (3) for 1930 baseline differences in nonparticipation ($\widehat{\pi}_{a(i)}$ under age 65; $\widehat{\rho}_{a(i)}$ ages 65+) and 1940 relative differences ($\widehat{\mu}_{a(i)}$ under age 65; $\widehat{\gamma}_{a(i)}$ ages 65+) between railroad and control workers. Reassuringly, the patterns match the gaps shown in Figure 5. The gray, solid line shows relative differences occur only at newly eligible ages (65-69) and decline precisely at age 70 while remaining above 1930 levels. These patterns are consistent with the predictions in Section I.D of sizable *behavioral effects* at voluntary ages, while many workers still found it optimal to delay retirement until mandatory exit at 70. Preexisting differences (black, solid line) were flat until age 70, when private pension compulsory provisions already bound many workers. Both sets of estimated coefficients are only statistically significant at these ages.[38] The magnitude of $\widehat{\gamma}_{a(i)}$ rise sharply after age 64, from 7.6 p.p. (s.e.=2.2) at age 65 to 13.0 p.p. (s.e.=2.4) at age 66 and peaking at 18.9 p.p. (s.e.=3.0) by age 69. Because LFP is measured for the last week in March, 1940, many workers turned 65 recently, or even since the week measured (interviews continued through mid-April). I therefore view coefficients as partially including the age prior, which may explain why the coefficient at age 65 is roughly half the size as that of 66. At the same

[38] The dashed lines are pointwise confidence bands. In Figure A.6 I show that conclusions regarding statistical significance do not change when using Uniform $Sup-t$ confidence bands (Montiel Olea and Plagborg-Møller, 2019) with consistent asymptotic coverage instead. I estimate the bands using using the STATA user written command available here.

time, Figure 4 panel (a) suggests much of the claiming is indeed also at 66.

Interpreting these coefficients relative to 1930 nonparticipation among railroad workers yields similar patterns by age, with the largest changes occurring between ages 65-69 (ranging between 47.9 and 73.0 percent). Because nonparticipation was already quite high for workers 70 and above and the coefficients at these ages are smaller, the aggregate relative increase for ages 65-74 of 33.3 percent (60.6 percent for ages 65-69, 17.9 percent for ages 70-74). Comparing the estimates instead to the *observed change* in nonparticipation indicates roughly two out of every five workers ages 65-74 would have worked in the absence of the RRA, with more at ages 65-69 and less at ages 70-74, which reaffirms that the RRA had the biggest impact on retirement at ages 65-69.[39]

- *Retirement Timing*

How well do these estimates reflect the prediction in Section I.D that retirement timing should be largely clustered at either end of voluntary eligibility ages; 65 and 70? In Figure A.7 I plot F-statistics from tests of consecutive equality (by age) of the coefficients in Figure 6.[40] I plot these for the relative effects ($\widehat{\gamma}_{a(i)} = \widehat{\gamma}_{a-1(i)}$) as well as the total effects ($\widehat{\gamma}_{a(i)} + \widehat{\rho}_{a(i)} = \widehat{\gamma}_{a-1(i)} + \widehat{\rho}_{a-1(i)}$). The only ages between 65-74 for which coefficients are statistically different from the previous age are 65, 66, and 70, indicating that workers responded by exiting predominately at either end of the voluntary age range. These are precisely the ages of highest density in Figure 4 panel (a).

I also examine the effect of the RRA on the 1-year retirement hazard – or probability of exiting in year t conditional on working in $t-1$ – which is a commonly used measure in the literature (e.g., Hausman and Wise, 1985; Coile, 2015). Note that this type of hazard analysis is central to the estimation strategy in Section V. The 1930 Census does not allow determination of work in the previous year, so the analysis sample is restricted to the 1930-

[39] To show the role of control workers in forming these magnitudes, I plot estimates from a specification similar to (3) that omits control workers – comparing railroad workers before and after the RRA – in Figure A.9. Consistent with the trends in Figure 5, point estimates are sizeably larger. The use of control workers as a counterfactual for LFP is thus important in this context. It is not surprising the main estimates imply the RRA does not fully explain the LFP decline since counterfactual LFP accounts for access to public assistance for workers who would not have been eligible under private plans. Further, to the extent that declines in revenue on railroads were made up by 1940 and home values recovered, some individuals may have exited the labor force regardless of changing pension incentives.

[40] Retirement is an "absorbing state", so these differences will provide a good indication of exit *at that age* if these probabilities of retirement by age are the same across the post-RRA years. These differences in LFP can be thought of as aggregate hazards as in Figure 1 panel (b) and are often used to understand retirement timing from aggregate data (Costa, 1998c; Coile, 2015).

1940 links. I further restrict it to include only those workers with positive weeks employed in 1939 and estimate the following, cross-sectional version of (3):

$$(\text{Not in LF})_{i,1940} = \sum_{a(i)<64} \mu_{a(i)} \times \text{RR}_{i,1930} + \sum_{a(i)>64} \gamma_{a(i)} \times \text{RR}_{i,1930} + \varepsilon_{i,1930} \tag{4}$$

The results, displayed in Figure A.8, show that the hazard was the same among railroad and control industries through age 64 and spikes discontinuously at age 65, remains at a similarly higher value than control workers of roughly 2.7-4.3 p.p. through age 69, and spikes again at 70.[41] In sum, the preponderance of evidence suggests that retirement timing was clustered at either end of voluntary pensioned-retirement window, consistent with the expected patterns of retirement.

IV.A Additional Evidence

- *Relating Nonparticipation to Pension Receipt*

To provide evidence relating labor force nonparticipation to pensioned retirement, I test the effect on the probability of non-wage income (NWI) $\geq$\$50 per year (nominal) in 1939 as a proxy for pension receipt.[42] This question was only asked in 1940. I estimate (4) and compare the coefficients for nonparticipation and NWI receipt in Figure A.10, which shows the patterns track one another quite closely. Because the nonparticipation estimates approximate the sum of effects represented in Figure 6 (and NWI estimates do for effects that could be estimated if that question were asked in 1930), the effects continue to rise past age 70. As Figure 6 shows, however, there *were no* 1930 differences in nonparticipation through age 69 ($\widehat{\rho}_{a(i)} = 0$). It is therefore reasonable to view the effects for NWI through age 69 as resulting *from* the RRA, and provides compelling evidence of the link between nonparticipation and pension receipt.

- *Linked Data are Not Driving Results: Using "Usual Industry" in 1940*

Railroad Retirement benefits were more generous than other industrial pensions or public el-

[41] The patterns indicate significantly higher hazards at ages above 70. Because 70 marked compulsory retirement, the sample size declines considerably at these ages. The coefficients are thus driven by a selected group of workers either no-longer in the railroad industry or with special permission to continue work and I do not place much stock in the hazard at those ages.

[42] Fetter (2017) shows this measure proxies well for public assistance recipiency, benefits for which were much smaller. 0.3 percent of post-RRA claimants received less than \$10 per month (RRB, 1940 p. 150), so this measure is likely to provide a good proxy for pension receipt. One month of average benefits in 1939 would be enough to answer yes to the NWI question, so I assume the age of receipt is their 1940 age (and the results in Figure A.10 bare this out).

derly transfers (see FN 3). Therefore, to the extent that proxying for a worker's "permanent" industry with their industry in a single year is measured with error – due to temporary work or imperfect linkage – lower pension incentives should attenuate results.[43] To provide evidence the main estimates constitute a plausible lower bound, I use the 1940 Census question asking about "usual industry".[44]

I estimate (4) on the set of individuals in 1940 aged 50-74 who reported railroad or control usual industries (time subscripts are now all 1940), matching railroad and control IPUMS usual industry to 1950 occupation codes. Figure A.11 plots these estimates both for exit and non-wage income receipt. The smaller sample size leads to less precision, but the pattern confirms flat differences in LFP at pension-ineligible ages while showing a somewhat *larger* increases in retirement after age 64 than those indicated from the cross-sectional results (Figure A.10). Similar patterns and null results at ineligible ages indicates that neither proxying for permanent industry with observed industry nor linkage error is driving the effects, while the larger magnitudes suggest the main estimates may instead constitute a plausible lower bound.

IV.B Results in Context: Aggregate LFP Trends

Figure 1 panel (a) shows the elderly male LFP declined by roughly 11 percentage points between 1930-1940. I develop counterfactual age-specific LFP rates in 1940 absent the RRA using my preferred estimates from Section IV. In Table 2, I tabulate the estimates of $\widehat{\gamma}_{a(i)}$ for ages 65-74 (scaled $\times 100$) in column (2), followed in column (3) by the number of railroad workers in 1930 according to their age in 1940. I calculate observed LFP in 1930 and 1940, then calculate counterfactual 1940 LFP using each estimate multiplied by the count of

[43] Nonportability of pensions across firms is a main reason why older employees would prefer to remain at the same company. Pensions were often used to "instill self-reliance in [employee's] corporate families" (Huibregtse, 2010, p. 95). In response to a planned strike in 1921, railroads asked employees to "consider carefully any decision to leave the service... they will lose not only their jobs but their seniority rights and pension privileges." (Railway Age, 1921 p. 835). Among those railroad workers in 1930 aged 64 in 1940, reporting employment in 1940, and who had worked positive weeks in 1939 (n=7,204), roughly 71 percent were still working for railroads.

[44] I prefer using the linked sample for the main analysis because usual industry does not permit evidence on employment differences in 1930 and is only asked for roughly 1 in 20 individuals (Census "sample line"). Balance tests in the 1930-1940 linked sample between those that did and did not report usual industry in 1940 (Table A.4) indicate a relatively small difference in occupational income scores, constituting roughly 5 percent of the mean occupation score. They are balanced on home ownership, children, and marriage, implying usual industry is a reasonable additional test on the validity of Census linkage.

workers (columns (2) and (3)).[45] This exercise indicates the RRA explains between 2.5 and 6.4 percent of the LFP decline, depending on the age considered, with more of the decline explained at ages 65-69. The last row shows the aggregate change for ages 65-74 was almost 5 percent. Fetter and Lockwood (2018) show that around 60 percent of the aggregate decline ages 65-74 can be attributed to public assistance for the poor; these results indicate around 12 percent of the remainder can be directly attributed to the RRA.

This paper is one of the first to note a focal retirement age before the New Deal at age 70 (Figure 1 panel (b)), while the large, existing spike in nonparticipation beginning at age 70 in 1930 provides one proximate cause. At the same time, the industry-wide results show increased eligibility and compulsory retirement provisions under the RRA provides one explanation for the continuance of a large spike in the 1940 retirement hazard at age 70. Railroad pension recipiency continued to grow after 1940 (Figure 2). While speculative, generous benefits and later expansions for disability, spousal, and early retirement benefits likely continued to depress elderly employment in the railroad industry in ensuing decades. On the other hand, the share of the elderly population who had worked for railroads declined substantially in ensuing decades, which may be one reason why the age-70 spike is much lower by 1960.

IV.C Robustness and Ruling out Alternative Explanations

This section summarizes a host of additional results in Appendix A. The definition of LFP changed markedly in 1940, from the concept of "gainful employment" to asking about work at during a specific reference period (see Moen (1988) for a useful discussion). In Appendix A.I I show that using gainful employment to consistently define LFP does not meaningfully change results. In Appendix A.II I next show the patterns in Figure 6 are maintained across 9 specification checks that include various sets of county-level fixed effects, covariates, limit comparisons to be within occupational income score or occupation, or for which standard errors are clustered at different geographic levels. The appendix proceeds by documenting that results are not sensitive to using any of the four linkage algorithms provided by Abramitzky et al. (2020) or their intersection (in Appendix A.III) or the particular choice of railroad or control industry workers included (in Appendix A.IV).

Appendix A also presents evidence ruling out alternative explanations. I first show that that differential linkage probabilities proxies reasonably well for differential mortality and

[45] Following Fetter and Lockwood (2018) I use conversion factors provided in Durand (1948) to adjust down age-specific LFP in 1930.

next show there is no evidence of differential selection into linkage over the age profile in Appendix A.V. I next show in Appendix A.VI that results are not driven by either of the other New Deal elderly social insurance programs, Old Age Assistance and Social Security. Finally, I show in Appendix A.VII that results are stable across quartiles of the 1930 unemployment rate, indicating results are not an artifact of railroad workers locating in geographic areas differentially impacted by the Depression.

V Elasticities of Elderly Labor Force Nonparticipation

For voluntary retirement ages 65-69, the previous estimates represent a combination of incentives stemming from new eligibility for some and increased monthly annuities of varying amounts for others. They are therefore of limited use for understanding how retirement behavior responds to changes in public pensions in other settings. This section develops an empirical approach to estimate elasticities with respect to pension wealth *changes* (the intensive margin) that leverages the switch to a newly progressive benefit formula under the RRA. The section concludes by comparing these estimates to those in the literature.

V.A Analysis Sample

- *Restricting to 1940*

Recall from Section I.D that, for workers who would have qualified for pensions under private plans, equation (2) shows the percent change to benefits ($\%\Delta B(\overline{w}_i)$) is a function only of average wages ($\overline{w}_i$). The best available signals for $\overline{w}_i$ are wages in 1939 (w_i) recorded in the 1940 Census (the first to ask about wage income). I restrict attention to cross-sectional comparisons of retirement in 1940 for individuals working in 1939, leading to a natural interpretation of regression coefficients as changes in the 1-year retirement hazard (as in **??**). Reassuringly, Figure A.12 shows that the density of 1940 claiming (Figure 4 panel (a)) is close to that of 1940 nonparticipation in the 1930-1940 linked sample at ages 65 and older when further conditioned on working in 1939.[46]

[46] As discussed in Section IV, the smaller spike at 65 is likely due to retiring (claiming) after measured in the Census while still 65. This is consistent with the higher density at age 66 in the Census relative to administrative files. Figure A.12 shows the density formed instead by taking a "moving average" of the density at age a and at age $a + 1$ (except at age 65, which is the density plus one half at age 66) matches the aggregate claiming density quite closely. This highlights why, in the results to follow, there is a large (but insignificant) estimate at age 66.

The predictions from Section I.D indicate that, among pre-RRA eligibles, the observed spike in retirement at age 65 should be primarily driven by workers who had higher $\%\Delta B(\overline{w}_i)$ (lower $\overline{w}_i$). This suggests the importance of focusing on behavior *at age 65* in 1940 and to test how the change affected retirement conditional on remaining in the labor force past 65. Because of compulsory retirement, I limit the sample to cohorts aged 65-69 in 1940, acknowledging that those 68 and above were above 65 when benefits became broadly available in 1937 and faced a constrained choice-set over retirement age. I also include workers as young as 50 to show $\%\Delta B(\overline{w}_i)$ does not predict retirement at ineligible ages. Because $\overline{w}_i$ (and thus $\%\Delta B(\overline{w}_i)$) is likely endogenous to LFP, the empirical analysis includes control workers and dummies for granular bins of $\overline{w}_i$, limiting comparisons between railroad and control workers of similar wages.

- *Eligibility Restrictions*

Length of service is perhaps the most important criterion determining whether an individual would have been eligible for pensions prior to the RRA (Table A.1). Tenure on the same firm was also required. I proceed by making sample restrictions that are intended to proxy for fulfilling these requirements. I consistently attempt to make conservative choices rather than include all potentially pre-RRA eligible workers.

Few plans required more than 30 years of service.[47] I link the 1930-1940 component of the main analysis sample back to 1910, keeping those railroad workers in 1930 also working for railroads in 1910 to proxy for having worked for railroads for *at least 30 years*. For consistency, I also include only control workers who were linked to 1910.[48] 43.2 percent of the railroad workers linked to 1910 were working for railroads in 1910, which is around the share estimated to have been receiving pensions in the decades leading up to the RRA (Figure A.3). Table A.7 compares those who were working on railroads in 1910 versus those

[47] Of workers on firms with plans that reported a minimum service for age-based retirement, 90 percent faced service requirements less than 30 years. None of the disability pensions specified service of over 30 years.

[48] For these links, I use the "exact-conservative" method from Abramitzky et al. (2019), since the algorithm provided by Helgertz et al. (2020) is only publicly available for consecutive decennial censuses. Similar results using either methods for the previous set of estimates (Figure A.15) suggests results would be similar using the same algorithm throughout. 52.7 percent of railroad workers are linked to 1910, and 52.2 percent of control workers. I generate weights to make the sample representative in a similar fashion as the previous sample (see FN 31). Because this sample is linked twice, I adopt the following procedure: I estimate a probit to create the 1910-1930 weights (using the same covariates as before), but I estimate the model using the original 1930-1940 weights. Table A.8 shows the elasticity results presented in this section are relatively stable across the various Abramitzky et al. (2019) algorithms (referred to as "ABE") as well as unweighted specifications.

who were not among those linked. Because service length was one key determinant of wage levels (RRB, 1938 p. 102), wages should be higher among those on railroads in 1910. Indeed, wages are roughly 40 percent higher for this group.

Some of the above workers may have worked for separate firms, in which case they would generally have not been eligible or faced reduced annuities (see FN 43). I advance the notion that an individual observed living in the same county has a higher likelihood of working for the same company relative to someone who had moved. I restrict the sample further to the roughly 72.3 percent of railroad workers who had also resided in the same county in 1910 and 1930. In practice, the results are not sensitive to this restriction (Table A.8).[49]

- *Calculating the Benefit Percent Change* ($\%\Delta B(\overline{w}_i)$)

I estimate $\overline{w}_i$ using 1939 wage income (w_i), measured for railroad and control workers in 1940. This section provides a brief summary, while Appendix C.III provides a detailed description. I include both full time workers and those who worked part of the year (by linearly interpolating their wages using weeks worked). I choose to include "part-time" workers because those who worked the entire year could only have retired in the first quarter of 1940. I then leverage the aggregate age-wage profile of railroad workers – derived from the complete count 1940 Census – to "backcast" individual earnings by age to 1924, the first year in which wages entered into annuity computations. I also use annual average wages ICC (various years) to adjust previous wages.[50] Finally, given the complexity of how benefits changed at both tails of the distribution (see Appendix C.I), I omit workers with $\overline{w}_i \notin (\$125, \$300)$.[51]

[49] Over 50 percent of pension-covered workers were covered by plans that explicitly had eligibility ages at 65 or less, while many more were likely eligible at these ages (see discussion in Section I). Further, Figure 4 panel (a) shows 62 percent of workers claimed at ages less than 70, a lower bound for eligibility at those ages, and no plans gave a retirement age between 65 and 70. This is likely a conservative lower bound, given the discussion of low replacement rates in Section I.D. There is limited scope for further restrictions to proxy for this margin of eligibility, but if age-65 eligibility is conditionally random after the above service and tenure restrictions, measurement error should indicate that the below estimate represent a plausible lower bound.

[50] This approach is similar to other examples of constructing pension incentives from imputing work histories or potential wages (e.g., Burtless and Moffitt, 1986; Gruber, 2000; Coile and Gruber, 2007). Table A.8 shows that using observed (partially imputed) wages instead of this procedure leads to quite similar results

[51] One additional reason for the lower restriction is that Figure C.4 shows the computation of average wages (in the elasticity sample but not under the service and tenure restrictions) does well at matching the administrative distribution of average wages above roughly \$125 but overestimates the density at lower wages. Another reason for the upper-wage restriction is that the compulsory retirement age of 70 was less applicable to executives, whom are likely to represent many of the very high wage earners observed. Table A.8 shows results are quite similar for all wage workers or various alternative restrictions.

I then estimate $\%\Delta B(\overline{w}_i)$ via equation (2).

The final sample consists of 1,398 men ages 65-69 who worked positive weeks in 1939, were linked back to 1910, live in the same county, satisfy the above restrictions on average wages, and, for railroad workers in 1930, also worked for railroads in 1910.

V.B Empirical Strategy

I proceed by estimating semi-elasticities separately for each age 65-69, and also present a falsification test for ages 50-59 (grouped).[52] I estimate the following specification:

$$(\text{Not in LF})_{i,1940} = \mathbf{X}_{i,1930}\boldsymbol{\beta} + \overline{w}(i) + o(i) + \epsilon \times \text{RR}_{i,1930} \times \%\Delta B(\overline{w}_i) + u_{i,1930} \tag{5}$$

Where the coefficient of interest (ϵ) captures the effect of $\%\Delta B(\overline{w}_i)$ on the retirement hazard. In some specifications, $\overline{w}(i)$ represents average wages, but the preferred specification includes as $\overline{w}(i)$ fixed effects for \$100 (annual) wage bins, which limits variation contributing to ϵ to come only from comparisons of how retirement differentially depends on $\%\Delta B(\overline{w}_i)$ between workers in railroad and control industries of similar earnings. In other words, ϵ represents how the differences in retirement between railroad and control workers with similar wages varies by $\%\Delta B(\overline{w}_i)$.[53] The preferred specification includes occupation fixed effects ($o(i)$) to account for occupation-specific factors influencing the retirement decision (Ransom and Sutch, 1986; Hayward et al., 1989), as well as covariates $\mathbf{X}_{i,1930}$ (race, presence of children, and marital status). Standard errors are clustered at the wage-bin level in all specifications.

Under the assumption that workers in railroad industries would have had similar incentives to retire as those in other industries with broad pension coverge absent the RRA, $\widehat{\epsilon}_1$ identifies the semi-elasticity of the 1-year retirement hazard with respect to the percent change in annual benefits. Zero existing differences in nonparticipation in 1930 between railroads and control workers at ages less than 70 (Figure 6) provides evidence that cross-sectional comparisons should only represent changing incentives in the 1930s. Because of the rough equivalence between $\%\Delta B(\overline{w}_i)$ and the percent change to pension wealth (see Appendix C.II), $\widehat{\epsilon}_1$ will also approximate elasticities with respect to pension wealth (Moffitt, 1987). Further, because workers in control industries faced a greater substitution incentive

[52] I opt to not include individuals who are 60-64 – since they were able to obtain early retirement benefits at a reduced rate – although it does not appear many took up these benefits (see Appendix C.I).

[53] \$100 is a relatively fine grouping, comprising around 4.7 percent of 1939 average annual railroad wages (similar results below controlling instead for a linear effect in $\overline{w}_i$ indicates the particular choice of the wage bin is largely inconsequential.

to retire earlier (due to declining wages at higher ages), these estimates provide a plausible lower bound on income effects.

V.C Hazard Elasticity Estimates

Table 3 shows the semi-elasticity estimates for each age group (in rows) and across specifications (columns). There is clear evidence of a large elasticity at age 65 across specifications. Column (1) presents results for railroad worker-only comparisons (without $w(i)$). Evaluated at the mean benefit percent change ($\overline{\%\Delta B(\overline{w}_i)} = 53.1\%$) implies a hazard of 14.0 percent, explaining 97 percent of the observed average hazard (equality cannot be rejected). Moving towards the other specifications – which include control workers and a linear control for wage (column (2)), wage bin fixed effects instead (column (3)), add occupation fixed effects (column (4)), and add controls (column (5)) – shows that the estimate is reduced by about half and quite stable. For the preferred specification in column (5), the results indicate the average benefit increase can account for 67.7 percent of the hazard at age 65. The smaller estimates using control workers and controlling for wages indicates that the bias is positive, consistent with the expected sign (see discussion in Section I.D). Various additional robustness tests presented in Table A.8 indicate a range explained of between roughly 50 percent and 72 percent.

The estimates also suggest larger benefit increases led to more retirement at age 66 conditional on remaining working at least some time when aged 65, although the estimates are imprecise. Nonparticipation measured at age 66 is driven in part by workers who retired when age 65, so I view this as broadly consistent with exit at age 65. Reassuringly, the results for younger ages show no effect of $\%\Delta B(\overline{w}_i)$ on labor force exit. As an additional check on the validity of $\%\Delta B(\overline{w}_i)$, I instead estimate the preferred version of (5), replacing $\%\Delta B(\overline{w}_i)$ with $\overline{w}_i$. If the nonlinearity of $\%\Delta B(\overline{w}_i)$ captures important incentives impacting retirement, then $\%\Delta B(\overline{w}_i)$ should be a better predictor of retirement than $\overline{w}_i$. Indeed, the estimated effect of $\overline{w}_i$ is wrong-signed (positive) and insignificant (p-value=.156).

Table 3 shows that, for those who remain working past 64 (and to some extent 65), there is little evidence of a retirement response, except perhaps at age 69 (although not statistically significant).[54] In sum, the evidence is consistent with the prediction in Section I.D that the

[54]Declining sample sizes should caution interpretation, particularly at older ages. In a regression pooling ages 67-69, effects are small and not statistically significant. Further, those aged 68 and above in 1940 did not have the opportunity to retire under the RRA at age 65, which complicates interpretation of retirement timing for these cohorts.

retirement spike at age 65 should be driven by lower wage workers, and that average earnings don't have much explanatory power in the decision to retire conditional on remaining working past 64 and, to some extent, 65.

V.D Magnitudes in Context: Elasticities of Nonparticipation

The previous results are not directly comparable to those presented in Section IV, as the latter focus on nonparticipation (a stock) while the former on labor force exit (a flow). Further, much of the literature on Social Security (.e.g, Coile and Gruber, 2007), and nearly all of the literature on elderly transfers and retirement before the 1970s, (e.g., Costa, 1995, 2010; Fetter and Lockwood, 2018) focus on nonparticipation for various age groups. There is no way to leverage wage information in 1939 and examine nonparticipation for an age group over 65 in 1940 directly, so I develop a measure of the implied cumulative effect of benefit increases on nonparticipation from the hazard elasticities. Counterfactual nonparticipation then forms the basis for estimates of elasticities of nonparticipation.

A simple Bayes Rule argument implies the unconditional LFP at age a can be estimated by the product of the within-cohort hazard $h(a)$ and LFP at age $a-1$.[55] Estimates of LFP are computed iteratively, using LFP at age 64 as the baseline: $\text{LFP}(a) = \text{LFP}(64) \times \left(\prod_{t=65}^{a}(1-h(t))\right)$. LFP for ages 65-69 is then determined by taking the average of $\text{LFP}(a)$, weighted by population shares: $\text{LFP}(65,69) = \sum_{a=65}^{69}\left(\text{Pop}(a) \Big/ \sum_{a=65}^{69}\text{Pop}(a)\right) \times \text{LFP}(a)$.

I next apply the counterfactual hazards at each age by first re-estimating (5) for ages 65-69 *jointly*, fully interacting all independent variables with dummies for each age.[56] Let the hazard estimates be termed $\hat{\epsilon}(a)$, I formulate counterfactual LFP by iterating on counterfactual hazards, evaluated at the mean benefit percent change $\overline{\%\Delta B(\overline{w}_i)}$: $\widehat{\text{CF\_LFP}}(a) = \text{LFP}(64) \times \left(\prod_{t=65}^{a}(1-(h(t)-\hat{\epsilon}(t)\times\overline{\%\Delta B(\overline{w}_i)})\right)$. Counterfactual LFP for ages 65-69 is then determined by taking the average of $\widehat{\text{CF\_LFP}}(a)$, weighted by population shares: $\widehat{\text{CF\_LFP}}(65,69) = \sum_{a=65}^{69}\left(\text{Pop}(a) \Big/ \sum_{a=65}^{69}\text{Pop}(a)\right) \times \widehat{\text{CF\_LFP}}(a)$. p-values for these estimates are calculated using the Delta Method.

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[55]This assumes the probability of exiting by age is stable across cohorts. This is not the case for the cohorts who had already exited (see discussion in FN 25) but is plausible for cohorts turning 65 after the RRA.

[56]I estimate the coefficients jointly to obtain the variance covariance matrix required for the delta method. I do not include covariates ($\mathbf{X}_{i,1930}$), since the estimates are quite similar (Table 3), so this specification leads to numerically equivalent estimates as in Table 3 column (4).

The estimated effect of benefit increases on nonparticipation ages 65-69 is $1-\text{LFP}(65,69)-(1-\widehat{\text{CF\_LFP}}(65,69)) = \widehat{\text{CF\_LFP}}(65,69)-\text{LFP}(65,69) = 10.1$ p.p. (p-value=.022). The elasticity of nonparticipation evaluated at the mean benefit percent change is given by:

$$\left(\frac{\widehat{\text{CF\_LFP}}(65,69) - \text{LFP}(65,69)}{1 - \widehat{\text{CF\_LFP}}(65,69)}\right) \times \left(\frac{1}{\overline{\overline{\%\Delta B(\overline{w_i})}}}\right) \tag{6}$$

Which is $\left(\frac{.1007}{.347}\right) \times \frac{1}{.531} = .55$ (p-value=.033).[57] As expected, the elasticity declines as ages higher than 69 are considered; the compulsory retirement provisions, not the benefit increases, cause exit at those ages. Following the same procedure for ages 65-74 (and assuming the "effect" of benefit increases on exit at ages 70 and above is 0) implies an elasticity of .45 (p-value=.006).

The elasticity of nonparticipation found is much larger than those found in modern quasi-experimental settings from Social Security (Krueger and Pischke, 1992), teachers pensions (Brown, 2013) and disability insurance (Bound, 1989). At the same time, the estimate are within the range of those found by Costa (1995) from Union Army Pensions (0.73 in 1900; 0.47 in 1910) and by Friedberg (1999) for elderly means-tested public assistance (0.25-0.42 in the 1940s, as calculated in Costa (1998b), Table 1). My estimates complement those for this period, since marked differences in how these programs were structured and who they targeted may limit their applicability to Social Security-type programs.[58]

The large effects found from the RRA are consistent with the unexpected nature of the reform and its occurrence late in life for the cohorts under study (Moffitt, 1987; Krueger and Meyer, 2002) and because retirement rates were low and benefits high. At the same

[57]LFP is overstated relative to that observed in the sample because the hazard is underestimated at each age (individuals may retire later while the same age). Counterfactual LFP is overstated in the same way. Note that, because these terms enter into both the numerator and denominator of (6), it is not directly evident whether this leads to an under or overstated estimate. Figure A.12 shows that a simple moving average of consecutive age retirement probabilities provides a better match of the retirement (claiming) density in the 1930-1940 linked sample (see discussion in FN 46). If I adjust $h(a)$ by the factor of the moving average to that of each age and recalculate (6), the estimate is .52 (p-value=.027). If I also adjust $\hat{\epsilon}(a)$ by the same factor, the estimate is .63 (p-value=.016), both within the confidence interval of the main estimate.

[58]Union Army Pensions were available at any age, did not require workers to quit their jobs, and were not based on wages. No link between receipt and LFP makes these estimates immediately applicable to estimating, for example, effects of increases in private wealth on LFP. They are arguably less applicable to workers facing a Social Security system with age-specific criteria, work history-based benefits, and recipiency linked with detachment. Estimates from means-tested elderly public assistance, which targeted the poor who were often unemployed or engaged in public works (Fetter and Lockwood, 2018), may also not be applicable to that for workers with relatively stable earnings prospects whose expected benefits were a direct function of labor market connectedness.

time, it appears that behavior around age 65 is predictable based on wages, through its determination of changes to benefits and resulting wage-replacement rate.

VI Discussion: Social Security Benefit Expansions and Retirement in the 1950s

Social Security grew markedly in the 1950s through a set of amendments that increased benefits and expanded eligibility. Between January, 1950 and January, 1960, the percentage of "fully insured" men (eligible for Social Security) 65 and older increased from 14.7 percent to 35.2 percent, while real benefit levels among recipients increased by roughly 94 percent (SSA, 1959 p. 18; Haines et al., 2010).[59] In turn, male LFP 65 and older declined by roughly 11 percentage points (Figure 1 panel (a)). As with railroad retirement benefits, Figure 1 panel (b) shows much of the decline is attributed to a much more pronounced spike in the nonparticipation hazard at age 65 in 1960 (15.9 percent), relative to 1950 (6.8 percent), with LFP among men 65-69 declining by roughly 15.5 percentage points, the largest historical decadal decline for this age group.

Unsurprisingly given their intertwined history, the benefits under both railroad retirement and Social Security were structured quite similarly during this period, with both providing progressive benefits and having the same retirement age of 65. The elasticity estimates in this paper speak best to the impact of benefit expansions in the 1950s in explaining increased (and earlier) claiming. Similar to the RRA, these changes were unexpected and, for many, occurred close to or at pension eligible ages (Moffitt, 1987). The change in the share *of those eligible* who claimed benefits ages 65-69 – from 44 percent in 1950 to 70 percent in 1960 – is suggestive that increased benefits led to earlier claiming and retirement.[60]

I focus on those individuals who would have been eligible for benefits *absent amendments in the 1950s* – so called "1939 eligibles" – which is one key distinction from the cross-sectional (Boskin, 1977) or time-series (Moffitt, 1987) estimates that combine changes to benefits and eligibility. I focus on claiming, rather than LFP, because I cannot observe LFP among only

[59] I focus here on male retirement to keep results consistent with earlier sections. Nominal benefit increases in the 1950s were as follows: 77 percent in 1950; 12.5 percent in 1952; 13 percent in 1954; and 7 percent in 1958 (Martin and Weaver, 2005). Real benefit changes are calculated relative to 1949 using the Consumer Price Index.

[60] Some evidence to support this interpretation may be found in surveys asking reason of retirement. In the 1940s, 5 percent of retirees had left the labor force by choice in "good health", in 1951 15 percent, and by 1963 28 percent (Quinn and Burkhauser, 1994). Further, the change in claiming among men 75 and older was less (17 percent points), indicating a shift to earlier retirement.

those who would have been eligible under the original amendments. Because all who are induced to leave the labor force due to benefit increases should claim, this should provide a lower bound on the effect on LFP during this period.[61] Appendix A.VIII provides details for this procedure, which uses both the hazard and nonparticipation elasticity estimates of the previous section in conjunction with the size of cross-cohort benefit increases. The exercise indicates benefit increases can explain between 65 and 77 percent of increased claiming over this decade for men 65-69 who were previously eligible. This is a large effect, but consistent with two recent attempts to re-engage with Social Security expansions and retirement during this period.[62] Although outside the scope of this paper, the extension of eligibility was a greater shock to pension wealth for previously ineligible men, so the effect on LFP for that group should be even larger.

VII Conclusion

This paper uses the introduction of Railroad Retirement benefits under the Railroad Retirement Act of 1937 to produce the earliest estimates of elasticities of labor force nonparticipation with respect to public pension benefits that depend on work history. Key elements facilitating the analysis are the recent availability of complete count Decennial Census data and developments in Census record linkage, which allow comparisons of LFP by prior industry. Models of forward looking pension incentives do well at predicting retirement timing and the RRA explains disproportionate share of the 1930-1940 elderly male LFP. I use the switch

[61] As described earlier, various checks between measures of non-income wage and retirement (Figure A.10) and claiming and retirement densities (Figure A.12) indicate a close link between retirement and pension recipiency. The earnings test for Social Security in 1950 was also quite stringent. Mild relaxation of the earnings test in the 1950s may have weakened the link by allowing some to claim Social Security while still working part-time. Still, by 1960, the earnings test was "all or none" for individuals younger than 72 (DeWitt, 1999), and the exempt amount of $1200 per year constituted less than one fourth of average wages (Ruggles et al., 2021). Substantial liberalizations of the earnings test in the 1960s and 1970s may be one reason why Gelber et al. (2016) find large earnings increases in response to benefit decreases in the 1970s, but somewhat more modest nonparticipation effects than I find.

[62] The current exercise provides a bridge between Fetter and Lockwood (2018), who apply estimates from public assistance to estimate between 50-90 percent of male LFP 65-74 declines through 1960 is due to Social Security, with Gelber et al. (2016), who apply estimates from the Social Security notch in the 1970s to the period 1950-1985, finding roughly 60 percent the elderly decline is explained. The primary distinction from Fetter and Lockwood (2018) is largely in that the approach exploits changes to benefits from a non-zero amount for a group of workers with good employment prospects. Relative to Gelber et al. (2016), this procedure essentially does the reverse, "forecasting" the effect from earlier estimates versus "backcasting" from later estimates. These effects need not be symmetric, since the base-level replacement rates are quite distinct. Further, the rules governing railroad retirement benefits in 1940 – especially the earnings test – are more similar to those of the Social Security program in the 1950s than was the program circa the mid-1970s.

to progressive benefits to develop 1-year nonparticpation (hazard) elasticity estimates and to develop an estimate of the elasticity of nonparticpation for various elderly age groups. The implied elasticity of nonparticpation for ages 65-69 is much larger than those found today, but consistent with those from distinct transfer programs of the period. Importantly, the ways in which Railroad Retirement incentives differed from the early programs are ways in which the program has always always resembled Social Security. Application of these estimates to historic Social Security benefit expansions in the 1950s suggests benefit increases explain a large share of elderly LFP declines in that decade, driven largely by earlier retirement among men ages 65-69.

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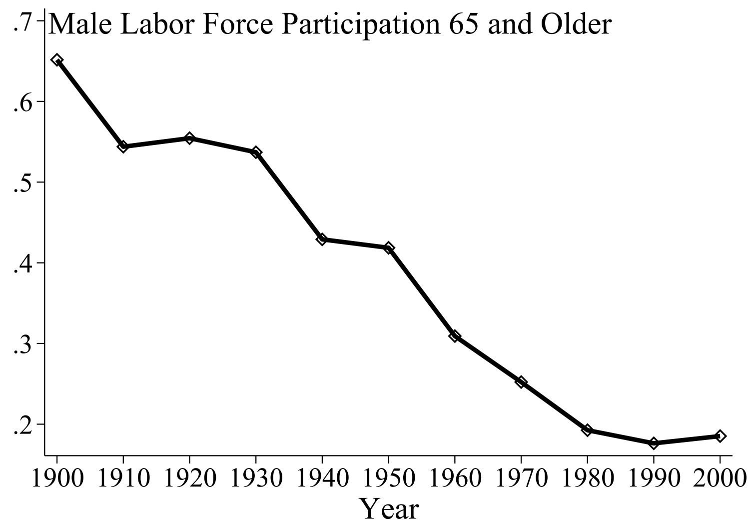
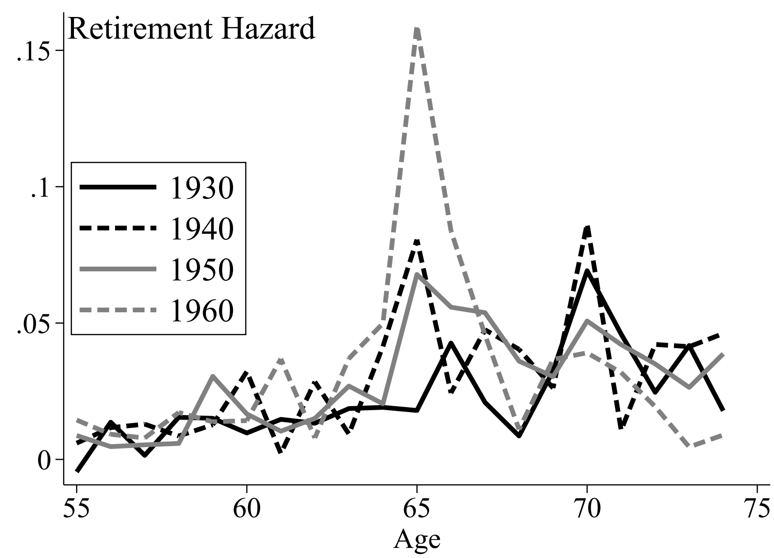
Figures and Tables

Figure 1

(a) 20th Century Male Labor Force Participation 65 and Older

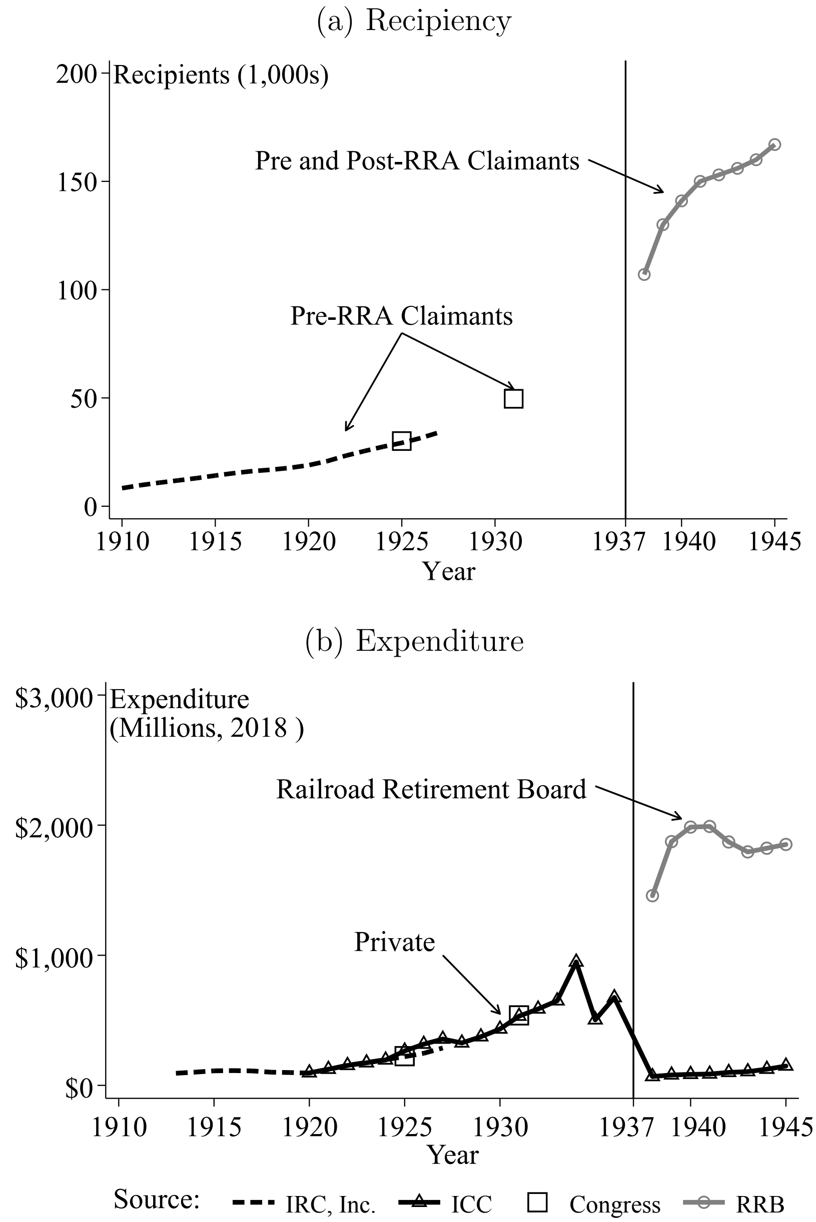


(b) Retirement Timing of Men: 1930-1960



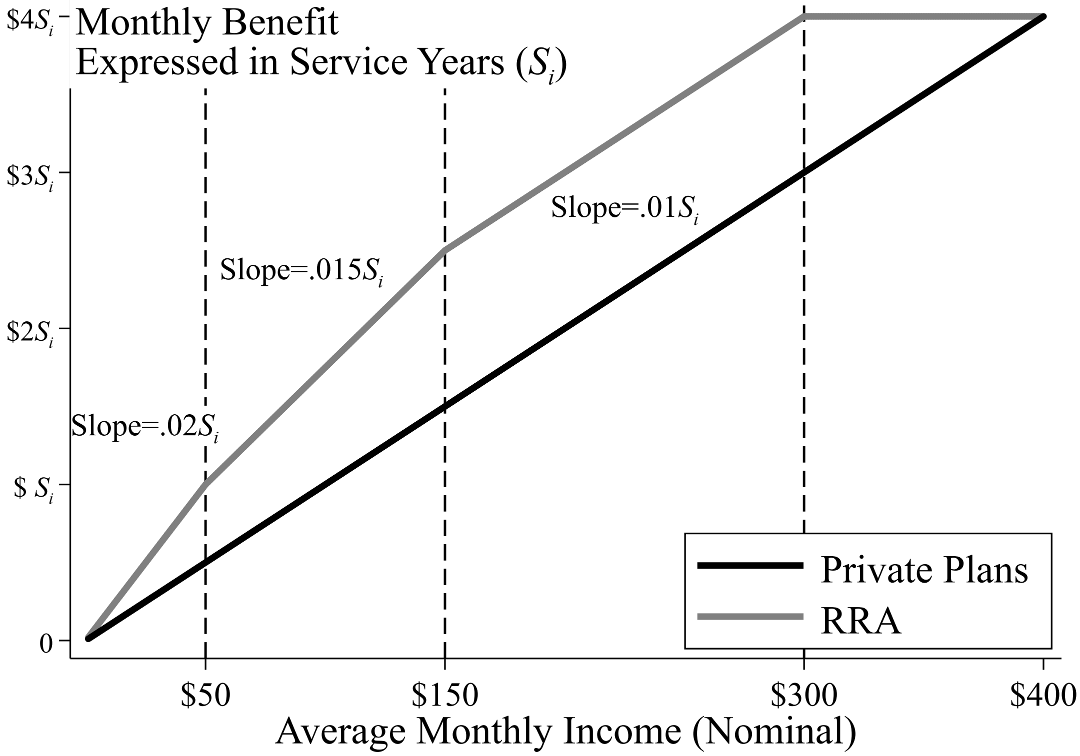
Notes: Panel (a) plots elderly male labor force participation (LFP) 65 plus (share of individuals 65 plus not in the labor force) as estimated from 1 percent Decennial Census samples (Ruggles et al., 2021). For 1940 and after, the series is formed from the unified IPUMS "employment status" question. Before 1940, LFP was defined by whether the individual had any "gainful employment". Appendix A.I describes how this measure overstates LFP relative to the new definition in 1940. I take adjustment factors provided by Durand (1948, p. 199) for the 1930 Census and apply these to all years prior to 1940, to make them comparable to later years. These adjustment factors are provided for ages 65-69, 70-74, and 75 plus. I assume the value for the mid-point (or 77 for 75 plus) and linearly interpolate for ages in between before aggregating to 65 and older. Panel (b) plots the aggregate 1-year retirement hazard, or difference in elderly male labor force nonparticipation between age a and $a-1$, as estimated from 1 percent Decennial Census samples (Ruggles et al., 2021) from 1930-1960. The series is formed from the unified IPUMS "employment status" question.

Figure 2: Railroad Pension Recipiency and Expenditure, 1910-1945



Notes: This figure shows the number of railroad pension recipients (Panel (a)) and real expenditure in 2018 dollars (Panel (b)) from 1910-1945, with various lines indicating separate sources. For panel (a) these are Latimer (1932) for years 1910-1927 (black, dashed line); U.S. Congress (1934) for 1925 and 1931 (black boxes), and Carter et al. (2006) Series Bf746-761 for 1938 and later (gray line with circles). Sources for expenditure are the same, with the addition of ICC (various years) (black, solid line) covering 1920-1945. Pre-RRA claimants refers to recipients who claimed prior to the RRA, whereas pre and post-rra refers to the sum of rolled over pensions (from private plans to the RRB) and annuitants, who had received credits through the RRB. Recipiency and expenditure data for 1910-1927 are from a non-exhaustive set of reporting railroads (see discussion in FN 12.)

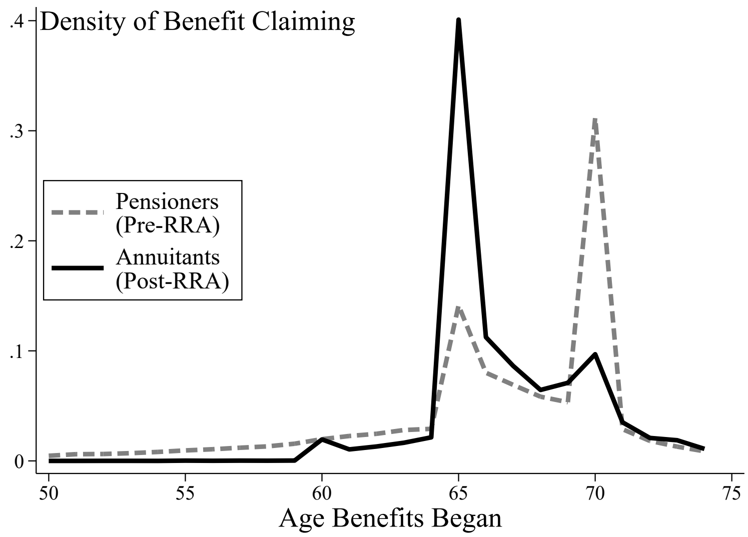
Figure 3: Newly Progressive Benefits Under the RRA



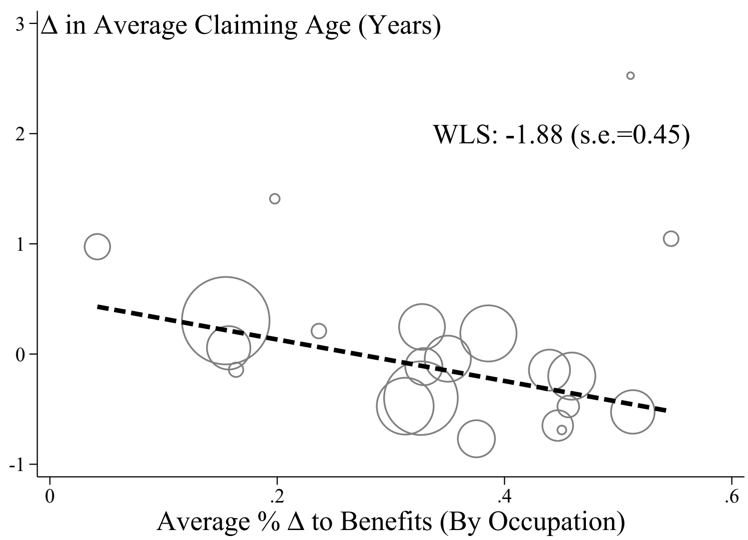
Notes: This figure plots a typical pre-RRA private railroad pension benefit formula (see Table A.1) in black, and the formula common to all railroad workers covered by the RRA in gray. The y-axis is in terms of service years S_i. The slope values represent the slope of the schedule in the relevant region (also in terms of S_i).

Figure 4: Claiming Ages and Benefit Changes

(a) Pre and Post-RRA Density of Claims, by Age

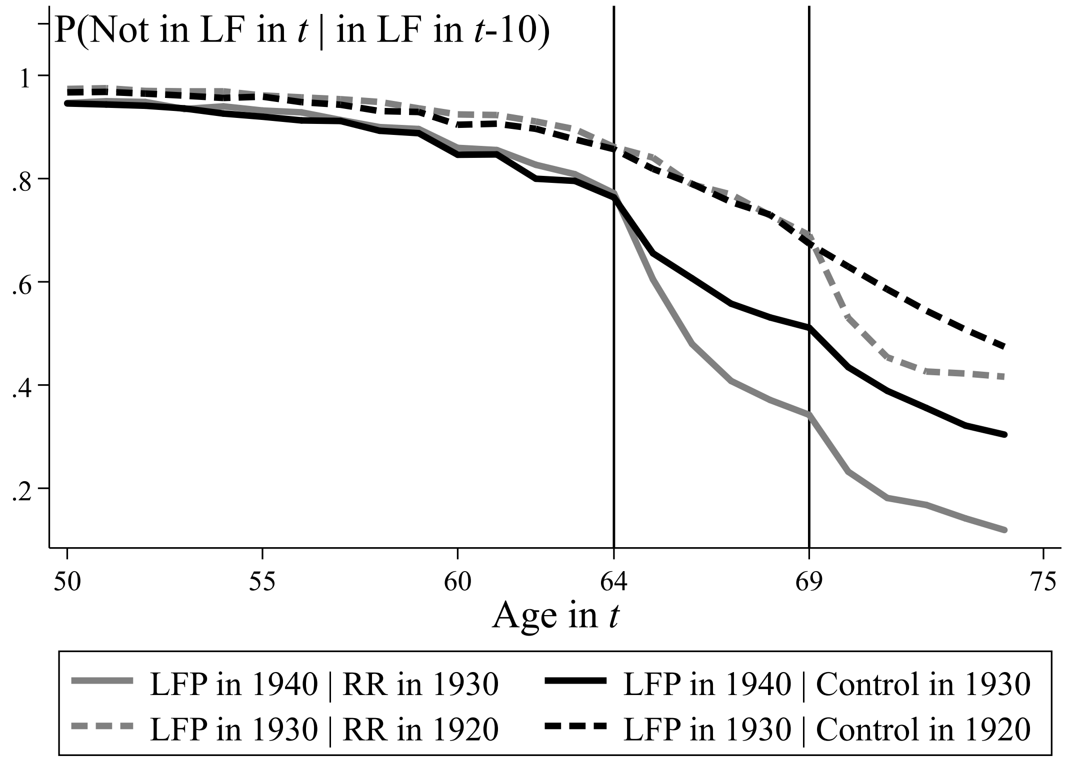


(b) Benefit Growth and Retirement Timing



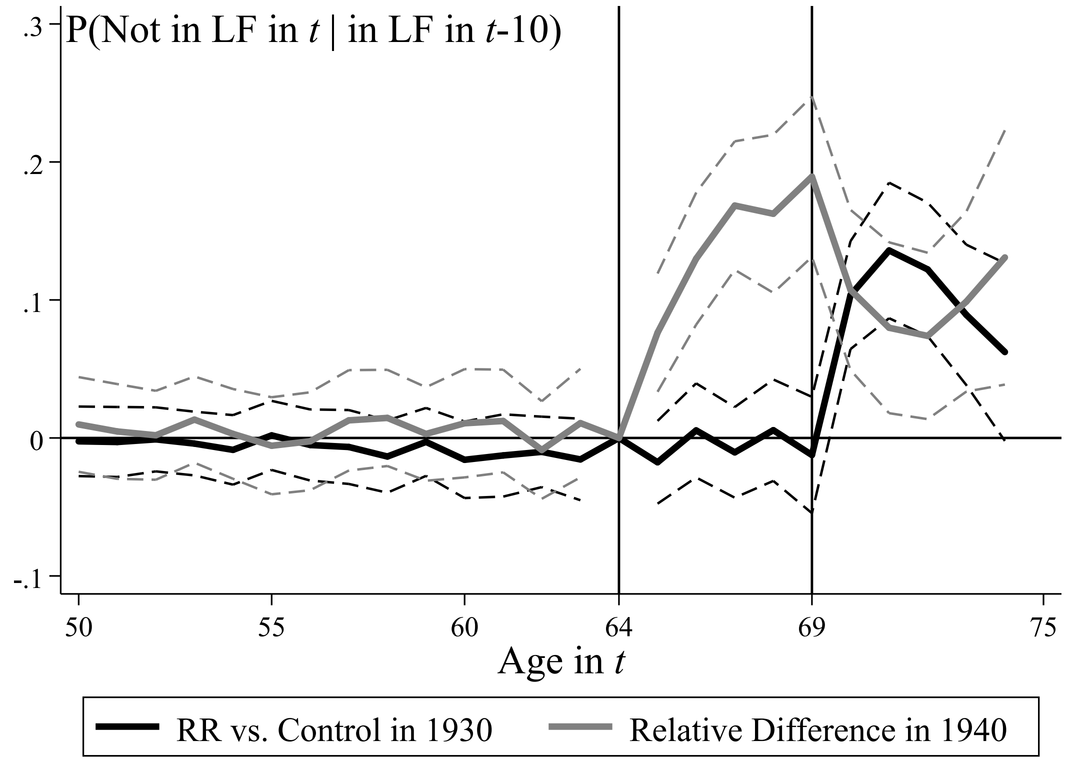
Notes: Data for panel (a) come from the RRB (1938 p. 99; 1941 pp. 211,212). The gray, dashed line is the density of benefit claiming age (in years) for pre-RRA private railroad pension plans, 90 percent of whom claimed between 1926-1935. I include both retirees under "age" and "disability" annuities (see Figure A.2 for each type separately). The black, solid line is the density of first benefit claiming age for post-RRA claimants who claimed in fiscal year 1940 (between July, 1939 and June, 1940). Roughly 2.3 percent of annuitants and 4.1 percent of pensioners claimed at ages outside of 50-74. See FN 24 for more details. Panel (b) Shows the difference in average claiming age (y-axis) against the percent change in average benefits (x-axis) at the occupation level between two cohorts – pre-RRA and post-RRA claimants – who were receiving annuities in 1938. Retirement ages are de-meaned at the claiming cohort level and markers represent 21 Federal Coordinator of Transportation (FCT) occupation codes, with the size of the markers representing the number of recipients in each occupation (summed across both cohorts). see main text and FN 25 for further details. Superimposed is the result from a weighted least squares regression of the form: $\Delta(\text{Retirement Age})_o = \alpha + \beta \times (\text{Benefit Percent Change})_o + \varepsilon_o$, where o indexes FCT occupation group.

Figure 5: Labor Force Participation of Workers in Railroad and Control Industries



Notes: Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Plots the age-specific LFP for railroad and control industries in 1930 and 1940 (according to industry as determined in 1920 and 1930), reweighted using weights generated to match the population at risk of being linked (see FN 31).

Figure 6: The Effect of the RRA on Labor Force Nonparticipation



Notes: N=956,391. Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Estimates are from (3) where the outcome indicates whether the individual was not in the labor force, reweighted using weights generated to match the population at risk of being linked (see FN 31). The black, solid line plots the differences in labor force nonparticipation between railroad and control workers in 1930 ($\widehat{\pi}_{a(i)}$ under age 65; $\widehat{\rho}_{a(i)}$ ages 65+) and the gray line plots the relative differences between railroad and control workers in 1940 ($\widehat{\mu}_{a(i)}$ under age 65; $\widehat{\gamma}_{a(i)}$ ages 65+). Black and gray dashed lines represent pointwise 95 percent confidence intervals. Standard errors are clustered at the county level (in $t-10$).

Table 1: Comparing Covariates Across Industry, Age, and Year

	(1) Sample Mean	(2) DDD ($\widehat{\beta}_7$)	(3) p-value
Marital Status	.75	.004	.16
		(.0029)	
White	.89	.0042	.037
		(.002)	
Have Children	.5	.00034	.94
		(.0044)	
# of Children | Have Children	2.6	-.014	.57
		(.025)	
Urban	.8	-.0058	.19
		(.0044)	
Occupation Score	28.3	.28	<.01
		(.09)	
Own House	.48	.0034	.51
		(.0052)	

Notes: Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Column (1) presents the sample mean. Column (2) shows the results from a series of descriptive triple-differences specifications that test for relative covariate ($x_{i,t}$) differences (measured in the base year) across industry, period, and age of the form: $x_{i,t} = \beta_0 + \beta_1 \times \mathrm{RR}_{i,t} + \beta_2 \times \mathbf{1}\{t = 1930\} + \beta_3 \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_4 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} + \beta_5 \mathrm{RR}_{i,t} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_6 \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_7 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \varepsilon_{i,t}$. All regressions are reweighted using weights generated to match the 1940 population at risk of being linked (see FN 31).

Table 2: Counterfactual LFP Absent the RRA

(1) Age	(2) NILF Estimate (×100)	(3) RR Emp. in 1930 $(a(i)-10)$	(4) 1930 LFP	(5) 1940 LFP	(6) C.F. 1940 LFP	(7) RRA Share
65	7.6 (2.2)	21,717	76.5	67.8	68.1	3.8
66	13 (2.4)	19,229	74	62.9	63.6	6.3
67	16.8 (2.4)	17,182	71.7	58.7	59.5	6
68	16.2 (2.9)	17,548	69.4	55.8	56.6	6
69	18.9 (3)	15,203	66.2	52.1	53	6.4
70	10.7 (3)	18,205	60.2	45.7	46.3	4
71	8 (3.2)	10,861	57.2	42.8	43.2	2.6
72	7.4 (3.1)	12,935	54.1	39.3	39.7	2.5
73	9.9 (3.3)	11,659	50.6	36.2	36.7	3.6
74	13.1 (4.7)	10,324	47.1	32.5	33.2	4.6
Aggregate (65–74)		154,863	64.8	52.2	52.8	4.8

Notes: Estimates of $\widehat{\gamma}_{a(i)}$ from (3) are presented in column (2) (see notes in Figure 6 for more details). Data in columns (3)-(5) are from complete count Decennial Censuses (Ruggles et al., 2021). Column (3) gives the number of railroad workers in 1930 according to their age in 1940 (corresponding to column (1)). Column (4) calculates 1930 LFP by age, adjusted down using conversion factors provided in Durand (1948). Column (5) calculates 1940 LFP by age. Column (6) calculates the counterfactual LFP assuming that column (2)/100×column (3) number of workers would have participated in the labor force. Column (7) is the ratio of difference between columns (6) and (5) and that of (4) and (5).

Table 3: Hazard Elasticity Estimates, by Age

Age	Mean RR Hazard		(1)	(2)	(3)	(4)	(5)
50–59 (Falsification Test)	.013	$RR_{i,1930} \times \%\Delta B(\overline{w}_i)$	-.019	.004	.003	.001	.001
			(.013)	(.004)	(.004)	(.005)	(.005)
		p-value	[.151]	[.394]	[.427]	[.832]	[.812]
		N	4,777	9,951	9,951	9,935	9,935
65	.144	$RR_{i,1930} \times \%\Delta B(\overline{w}_i)$	.261	.171	.163	.182	.181
			(.139)	(.036)	(.037)	(.051)	(.053)
		p-value	[.075]	[$< .01$]	[$< .01$]	[$< .01$]	[$< .01$]
		N	279	477	477	461	461
66	.093	$RR_{i,1930} \times \%\Delta B(\overline{w}_i)$	.305	.106	.095	.118	.121
			(.262)	(.096)	(.1)	(.091)	(.089)
		p-value	[.258]	[.279]	[.352]	[.213]	[.187]
		N	153	295	295	278	278
67	.087	$RR_{i,1930} \times \%\Delta B(\overline{w}_i)$	-.237	.026	.033	.011	.022
			(.155)	(.046)	(.044)	(.089)	(.088)
		p-value	[.142]	[.577]	[.467]	[.903]	[.81]
		N	144	247	247	232	232
68	.084	$RR_{i,1930} \times \%\Delta B(\overline{w}_i)$	0	.059	.069	.006	.001
			(.298)	(.07)	(.073)	(.108)	(.106)
		p-value	[.999]	[.407]	[.353]	[.956]	[.992]
		N	119	218	217	201	201
69	.072	$RR_{i,1930} \times \%\Delta B(\overline{w}_i)$	.043	.1	.103	.111	.114
			(.214)	(.065)	(.065)	(.086)	(.078)
		p-value	[.842]	[.143]	[.129]	[.208]	[.157]
		N	99	174	174	165	165
		Control Workers		Yes	Yes	Yes	Yes
		Wage Bin FEs			Yes	Yes	Yes
		Occupation FEs				Yes	Yes
		Controls					Yes

Notes: Sample is comprised of all male individuals in the relevant age group in the 1940 complete count Census (Ruggles et al., 2021) who were linked to the 1930 Census and who were working on railroads or industries I classified as covered by pensions (in 1930), using the linking algorithm provided by Helgertz et al. (2020) (See Section II and Appendix B for more details). The sample is further restricted to those workers who: worked in 1939 and had positive wages; are successfully linked to the 1910 Census using the "exact-conservative" method provided by Abramitzky et al. (2019); and, for railroad workers, worked for railroads in 1910 and lived in the same county as in 1930. The percent change to monthly benefits $\%\Delta B(\overline{w}_i)$ defined in equation (2) depends on $\overline{w}_i$, which is estimated using wage information in 1939 (Appendix C.III provides a detailed description of the procedure). Estimates are from (5) where the outcome indicates whether the individual was not in the labor force, reweighted using weights generated by the procedure described in FN 48). Standard errors are clustered at the level of wage bin.