Public Pensions and Retirement: Evidence from the Railroad Retirement Act

Matthew Pesner*

This Version: December, 2022
Click Here for the Current Version

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.

JEL Codes: H55, J21, J26, J32, N32, N42

---

*Pesner: University of Michigan, Population Studies Center, 426 Thompson St, Ann Arbor, MI 48106-1248 (email: mpesner@umich.edu). Much of this research was completed as part of my Ph.D. Dissertation at Vanderbilt University, for which I am grateful to my committee members Bill Collins (chair), Lesley Turner, Andrew Goodman-Bacon, and Kitt Carpenter. Also special thanks to George Alter, Brian Beach, Hoyt Bleakley, Daniel Fetter, Pat Flynn, Rick Hornbeck, Joanna Lahey, Nic Maeder, Dan Mangrum, Paul Mohnen, Sarah Quincy, Craig Sylvera, and Ariell Zimran for helpful comments, as well as to Seminar Participants in the University of Michigan Economic History Workshop, Florida State University Department of Economics, the 2021 Virtual Cliometric Conference, 2021 NBER Development of the American Economy Summer Institute, Economic History Association, Young Economists Symposium, and Vanderbilt Empirical and Applied Microeconomics Seminar. This research was supported in part by an NIA training grant to the Population Studies Center at the University of Michigan (T32AG000221). The content is solely the responsibility of the author and does not necessarily represent the official views of the National Institutes of Health. All errors are my own.

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 benefit changes and retirement has proven difficult to uncover for Social Security 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 the first work history-based (defined benefit) national pension for nongovernmental workers – affecting 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, with the voluntary retirement age set at 65 and mandatory retirement imposed at 70.[3] In urging President Franklin Roo-

[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,

sevelt 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. Given the significance of the RRA for guiding the creation of Social Security (Huibregtse, 2010), economic analysis of the RRA and retirement is surprisingly absent.

Figure 1 panel (a) shows the RRA was passed during a time of significant decline in male elderly labor market involvement. The broader 20th century trend and commensurate rise in government elderly transfers suggests a causal relationship, with research finding that entitlement programs explain more of the early post-1930 decline (Parsons, 1991; Friedberg, 1999; Fetter and Lockwood, 2018) and Social Security more of the latter (see Krueger and Meyer (2002) and Coile (2015) for reviews).[4] Slow initial growth of Social Security complicates using the early program to infer conclusions regarding the expansions. The marked difference between the introduction of railroad retirement benefits and Social Security paired with the close correspondence in incentive structure allows this paper to shed new light into the historical relationship between Social Security and retirement.

The empirical analysis begins by estimating the industry-wide affect of being a railroad worker on LFP after the RRA, and uses these estimates to quantify how much of the aggregate, eleven percentage point decline in the 1930s (Figure 1 panel (a)) is explained by nationalizing railroad pensions. I then estimate elasticities of nonparticipation, or how responsive retirement timing is to changes in benefits from a non-zero amount. Both analyses

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).

[4]Significant changes to LFP measurement in the 1940 Census are discussed in Appendix A.I. Roughly 27 percent of men 65 plus received public assistance or Social Security in 1940. In 2000, the share receiving Social Security alone was over 95 percent (Carter et al., 2006, Series Bf634-648; Bf408-421). The growth of private pensions is an additional explanation (Samwick, 1998; Stock and Wise, 2008). Evidence for pre-1930 is sparse, but Costa (1998a) shows that Union Army pensions were an important driver of LFP declines towards the beginning of the century. LFP for lower, middle-aged groups began declining later, with explanations also centering on Social Insurance (e.g., Parsons, 1980; Hurd and Boskin, 1984; Bound and Waidmann, 1992)

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 analysis concludes with a discussion of the likely impact of Social Security expansions in explaining LFP declines in the 1950s, focusing primarily on benefit increases.

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.[5]

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

[5]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.

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).

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.[6] 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.[7] 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

[6]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.

[7]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).

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 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) 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.[8] While the literature has focused the rise of retirement at 65 after the Social Security Act, compulsory retirement provisions also provide 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 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.

[8]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).

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).[9] 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).[10] 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).[11] 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 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

[9] 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.

[10] 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).

[11] 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).

railroads using the 1900, 1910, and 1920 complete count Decennial Censuses.[12] 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.[13] 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.[14] By the early 1930s, supporters of national railroad retirement

[12] 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.

[13] 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.

[14] 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 Pennsylvania Railroad, the largest railroad employer, is illustrative; they did not cut pension benefits until

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.[15]

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").[16] 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

April 1, 1932 (The Pennsylvania Railroad Company, 1933, p.10).

[15] 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).

[16] 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).

of elderly workers (the system is therefore "pay as you go").[17]

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 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)).[18] 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.,

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

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

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.[19] 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.[20] 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).[21] Roughly 40 percent of workers 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

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

[20] 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).

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

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).[22] 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

[22] 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 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).

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 pension 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 re-

duced 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.[23]

- *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.[24] Another conclusion to

[23]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.

[24]Higher wages may be the outcome of unobserved preference for work. In the context of retirement,

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 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.[25] 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, sepa-

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.

[25] 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).

rated 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.[26]

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.

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

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.[27] A second limitation is that no question asks whether an individual is covered by a pension or is receiving benefits.[28] 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.

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

[27] 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.

[28] 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.

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.[29] 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

[29] The algorithm matches individuals by birth year $\pm$ 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.[30]

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.[31] 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

[30] 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.

[31] 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.

estimate weights to make the sample representative of the population at risk of being linked.[32]

- *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.[33] 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*

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

[33] 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.

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.[34] 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 (measured in the base year) accross industry, period, and age (in the later year).[35] Only two are significant (dummy for white and occupation score) and the estimates are negligible relative to the means.[36] 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).

[34] 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)

[35] Specifically, for each covariate $x_{i,t}$, $t \in \{1920, 1930\}$, I estimate the following specification via weighted least squares (see FN 32 for weight calculations):
$x_{i,t} = \beta_0 + \beta_1 \times \mathrm{RR}_{i,t} + \beta_2 \times \mathbf{1}\{t = 1930\} + \beta_3 \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_4 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} + \beta_5 \mathrm{RR}_{i,t} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_6 \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_7 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \varepsilon_{i,t}$

[36] 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.

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.[37] 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 70.[38] 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.

[37] 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).

[38] 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).

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, 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.[39] 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

[39] 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.

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 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.[40]

- *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.[41] 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 statis-

[40] 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.

[41] 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).

tically 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 1930-1940 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.[42] 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) ≥$50 per year (nominal) in 1939 as

[42] 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.

a proxy for pension receipt.[43] 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 elderly 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.[44] To provide evidence the main estimates constitute a plausible lower bound, I use the 1940 Census question asking about "usual industry".[45]

[43] 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).

[44] 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.

[45] 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

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 workers (columns (2) and (3)).[46] 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

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.

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

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 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.[47]

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.

[47] 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.

Few plans required more than 30 years of service.[48] 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.[49] 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 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 44). 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).[50]

[48] 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.

[49] 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 32). 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.

[50] 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

- *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.[51] Finally, given the complexity of how benefits changed at both tails of the distribution (see Appendix C.I), I omit workers with $\overline{w}_i \notin (\$125, \$300)$.[52] 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.

---

and tenure restrictions, measurement error should indicate that the below estimate represent a plausible lower bound.

[51] 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

[52] 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.

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).[53] 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 $\%\Delta B(\overline{w}_i)$.[54] 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

[53] 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).

[54] \$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.

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 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).[55] In sum, the evidence is consistent with the prediction in Section I.D that the 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 $a-1$.[56] Estimates of

[55] 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.

[56] 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 26) but is plausible for cohorts turning 65 after the RRA.

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.[57] 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.

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:

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

Which is $\left(\frac{.1007}{.347}\right) \times \frac{1}{.531} = .55$ (p-value=.033).[58] 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

[57] 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).

[58] 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 47). 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.

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.[59]

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 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

[59] 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.

to 35.2 percent, while real benefit levels among recipients increased by roughly 94 percent (SSA, 1959 p. 18; Haines et al., 2010).[60] 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.[61]

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 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

[60] 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.

[61] 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.

lower bound on the effect on LFP during this period.[62] 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.[63] 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

[62] 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.

[63] 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.

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 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.

References

Abramitzky, Ran, Leah Boustan, and Myera Rashid, "Census Linking Project: Version 1.0 [dataset]," *Data retrieved from, https://censuslinkingproject. org*, 2020.

_ , Leah Platt Boustan, Katherine Eriksson, James J Feigenbaum, and Santiago Pérez, "Automated linking of historical data," Technical Report, National Bureau of Economic Research 2019.

Aldrich, Mark, *Safety first: Technology, labor, and business in the building of American work safety, 1870-1939*, Vol. 13, JHU Press, 1997.

Alter, George and Samuel H Williamson, "Income, Disability, and Old Age Mortality among Early Twentieth Century Railroad Retirees," *Unpublished Manuscript*, 2018.

Asch, Beth, Steven J Haider, and Julie Zissimopoulos, "Financial incentives and retirement: evidence from federal civil service workers," *Journal of public Economics*, 2005, *89* (2-3), 427–440.

Bailey, Martha J, Connor Cole, Morgan Henderson, and Catherine Massey, "How well do automated linking methods perform? Lessons from US historical data," *Journal of Economic Literature*, 2020, *58* (4), 997–1044.

Behaghel, Luc and David M Blau, "Framing social security reform: Behavioral responses to changes in the full retirement age," *American Economic Journal: Economic Policy*, 2012, *4* (4), 41–67.

Bell, Felicitie C and Michael L Miller, *Life tables for the United States social security area, 1900-2100* number 120, Social Security Administration, Office of the Chief Actuary, 2005.

Blinder, Alan, Roger Gordon, and Donald Wise, "An empirical study of the effect of pensions and the saving and labor supply of older men," *Report submitted to the US Department of Labor*, 1978.

Boskin, Michael J, "Social security and retirement decisions," *Economic Inquiry*, 1977, *15* (1), 1–25.

Bound, John, "The Health and Earnings of Rejected Disability Insurance Applicants," *The American Economic Review*, 1989, pp. 482–503.

_ and Timothy Waidmann, "Disability transfers, self-reported health, and the labor force attachment of older men: Evidence from the historical record," *The Quarterly Journal of Economics*, 1992, *107* (4), 1393–1419.

Brown, Jeffrey R, Courtney C Coile, and Scott J Weisbenner, "The effect of inheritance receipt on retirement," *The Review of Economics and Statistics*, 2010, *92* (2), 425–434.

Brown, Kristine M, "The link between pensions and retirement timing: Lessons from California teachers," *Journal of Public Economics*, 2013, *98*, 1–14.

Burtless, Gary and Robert A Moffitt, "Social security, earnings tests, and age at retirement," *Public Finance Quarterly*, 1986, *14* (1), 3–27.

Carter, Susan B, Scott Sigmund Gartner, Michael R Haines, Alan L Olmstead, Richard Sutch, and Gavin Wright, "Historical statistics of the united states, volume one: population," *New York Cambridge Univ Pr*, 2006.

Chan, Sewin and Ann Huff Stevens, "What you don't know can't help you: Pension knowledge and retirement decision-making," *The Review of Economics and Statistics*, 2008, *90* (2), 253–266.

Clingman, Michael, Kyle Burkhalter, and Chris Chaplain, "Replacement Rates for Hypothetical Retired Workers," *Baltimore, MD: Social Security Administration, Office of the Chief Actuary. www. ssa. gov/OACT/NOTES/ran9/index. html*, 2014.

Cohen, Wilbur J and Robert J Myers, "Social Security Act Amendments of 1950: A summary and legislative history," *Soc. Sec. Bull.*, 1950, *13*, 3.

Coile, Courtney and Jonathan Gruber, "Future social security entitlements and the retirement decision," *The Review of Economics and Statistics*, 2007, *89* (2), 234–246.

Coile, Courtney C, "Economic determinants of workers' retirement decisions," *Journal of Economic Surveys*, 2015, *29* (4), 830–853.

Costa, Dora L, "Pensions and retirement: Evidence from union army veterans," *The Quarterly Journal of Economics*, 1995, *110* (2), 297–319.

_ , *The Evolution of Retirement. An American Economic History, 1880-1990*, University of Chicago Press, 1998.

_ , "The evolution of retirement: Summary of a research project," *The American Economic Review*, 1998, *88* (2), 232–236.

_ , *The evolution of retirement* number 9780226116082. In 'National Bureau of Economic Research Books.', University of Chicago Press, 1998.

_ , "Pensions and retirement among black Union Army veterans," *The journal of economic history*, 2010, *70* (3), 567.

DeWitt, Larry, "The history and development of the social security retirement earnings test," *Social Security Agency History: Research Notes and Special Studies by the Historian's Office, Special Study*, 1999, *7*.

Dorsey, Stuart, Christopher Cornwell, and David Macpherson, *Pensions and Productivity.*, ERIC, 1998.

Durand, John Dana, "Labor force in the United States, 1890-1960," 1948.

Ekern, Herman L, "Railroad Pensions," *Am. Lab. Legis. Rev.*, 1934, *24*, 124.

Fetter, Daniel and Matthew Pesner, "The Expansion of Social Security and the Decline of Elderly Public Assistance," 2021.

Fetter, Daniel K, "Local government and old-age support in the New Deal," *Explorations in Economic History*, 2017, *66*, 1–20.

_ **and Lee M. Lockwood**, "Government Old-Age Support and Labor Supply: Evidence from the Old Age Assistance Program," *American Economic Review*, August 2018, *108* (8), 2174–2211.

Friedberg, Leora, "The effect of old age assistance on retirement," *Journal of Public Economics*, 1999, *71* (2), 213–232.

_ **and Anthony Webb**, "Retirement and the evolution of pension structure," *Journal of Human Resources*, 2005, *40* (2), 281–308.

Gelber, Alexander M, Adam Isen, and Jae Song, "The effect of pension income on elderly earnings: Evidence from social security and full population data," *NBER Working paper*, 2016.

_ **, Damon Jones, and Daniel W Sacks**, "Estimating adjustment frictions using nonlinear budget sets: Method and evidence from the earnings test," *American Economic Journal: Applied Economics*, 2020, *12* (1), 1–31.

Graebner, William, *A history of retirement*, Yale University Press New Haven, 1980.

Gratton, Brian, "The poverty of impoverishment theory: The economic well-being of the elderly, 1890–1950," *the Journal of economic History*, 1996, *56* (1), 39–61.

Gruber, Jonathan, "Disability insurance benefits and labor supply," *Journal of political economy*, 2000, *108* (6), 1162–1183.

Gustman, Alan L and Thomas L Steinmeier, "What People Don't Know about Their Pensions and Social Security," in John B. Shoven William G. Gale and Mark J. Warshawsky, eds., *Private Pensions and Public Policies*, Brookings Institution Press, 2004, 2004, chapter 4.

Haber, Carole, "Mandatory retirement in nineteenth-century America: the conceptual basis for a new work cycle," *Journal of Social History*, 1978, *12* (1), 77–96.

Haines, Michael R., Inter university Consortium for Political, and Social Research, "Historical, Demographic, Economic, and Social Data: The United States, 1790-2002," 2010.

Harbison, Frederick H, "Seniority in mass-production industries," *Journal of Political Economy*, 1940, *48* (6), 851–864.

Hausman, Jerry A and David A Wise, "Social security, health status, and retirement," in "Pensions, labor, and individual choice," University of Chicago Press, 1985, pp. 159–192.

Hayward, Mark D, Melissa A Hardy, and William R Grady, "Labor force withdrawal patterns among older men in the United States," *Social Science Quarterly*, 1989, *70* (2), 425–448.

Helgertz, Jonas, Joseph Price, Jacob Wellington, Kelly J Thompson, Steven Ruggles, and Catherine A Fitch, "A new strategy for linking US historical censuses: A case study for the IPUMS multigenerational longitudinal panel," *Historical Methods: A Journal of Quantitative and Interdisciplinary History*, 2022, *55* (1), 12–29.

_ , Steven Ruggles, John Robert Warren, J. David Hacker Matt A. Nelson Joseph P. Price Evan Roberts Catherine A. Fitch Ronald Goeken, and Matthew Sobek, "IPUMS Multigenerational Longitudinal Panel: Version 1.0 [dataset]," *IPUMS*, 2020.

Huibregtse, Jon R, *American railroad labor and the genesis of the New Deal, 1919-1935*, University Press of Florida, 2010.

Hurd, Michael D and Michael J Boskin, "The effect of social security on retirement in the early 1970s," *The Quarterly Journal of Economics*, 1984, *99* (4), 767–790.

(ICC) Interstate Commerce Commission, "Annual report on the statistics of railways in the united states," *Washington, DC: Interstate Commerce Commission.(From 1954 renamed Transport Statistics in the United States.)*, Various Years.

Imbens, Guido W, Donald B Rubin, and Bruce I Sacerdote, "Estimating the effect of unearned income on labor earnings, savings, and consumption: Evidence from a survey of lottery players," *American economic review*, 2001, *91* (4), 778–794.

Krueger, Alan B and Bruce D Meyer, "Labor supply effects of social insurance," *Handbook of Public Economics*, 2002, *4*, 2327–2392.

_ and Jörn-Steffen Pischke, "The effect of social security on labor supply: A cohort analysis of the notch generation," *Journal of labor economics*, 1992, *10* (4), 412–437.

Latimer, Murray Webb, *Industrial pension systems in the United States and Canada*, Vol. 1, Industrial relations counselors, incorporated, 1932.

Lazear, Edward P, "Why is there mandatory retirement?," *Journal of political Economy*, 1979, *87* (6), 1261–1284.

Li, Xiaoyan, Michael Hurd, and David S Loughran, *The characteristics of social security beneficiaries who claim benefits at the early entitlement age*, Vol. 19, AARP, Public Policy Institute Washington, DC, 2008.

Manoli, Day and Andrea Weber, "Nonparametric evidence on the effects of financial incentives on retirement decisions," *American Economic Journal: Economic Policy*, 2016, *8* (4), 160–82.

Margo, Robert A, "The labor force participation of older Americans in 1900: further results," *Explorations in Economic History*, 1993, *30* (4), 409–423.

Martin, Patricia P and David A. Weaver, "Social Security: A program and policy history," *Soc. Sec. Bull.*, 2005, *66*, 1.

Mastrobuoni, Giovanni, "Labor supply effects of the recent social security benefit cuts: Empirical estimates using cohort discontinuities," *Journal of Public Economics*, 2009, *93* (11-12), 1224–1233.

Mitchell, Olivia S and Gary S Fields, "The effects of pensions and earnings on retirement: A review essay," *National Bureau of Economic Research Working Paper Series*, 1981, (w0772).

Moen, Jon R, "The labor of older men: a comment," *The Journal of Economic History*, 1987, *47* (3), 761–767.

_ , "From Gainful Employment to Labor Force: Definitions and a New Estimate of Work Rates of American Males, 1860 to 1980," *Historical Methods: A Journal of Quantitative and Interdisciplinary History*, 1988, *21* (4), 149–159.

_ **and Brian Gratton**, "Tracking the majority: household, older workers, and retirement during the Great Depression," *Journal of Gerontology: Social Sciences B*, 1999, *55*, s28–s32.

Moffitt, Robert A, "Trends in Social Security Wealth by Cohort," in "Economic Transfers in the United States," University of Chicago Press, 1984, pp. 327–358.

_ , "Life-cycle labor supply and social security: A time-series analysis," *Work, Health, and Income among the Elderly, Burtless, Gary, ed., Brookings: Washington, DC*, 1987.

Olea, José Luis Montiel and Mikkel Plagborg-Møller, "Simultaneous confidence bands: Theory, implementation, and an application to SVARs," *Journal of Applied Econometrics*, 2019, *34* (1), 1–17.

Parsons, Donald O, "The decline in male labor force participation," *Journal of political Economy*, 1980, *88* (1), 117–134.

_ , "Male retirement behavior in the United States, 1930–1950," *The Journal of Economic History*, 1991, *51* (3), 657–674.

Quinn, Joseph F. and Richard V. Burkhauser, "Retirement and Labor Force Behavior of the Elderly," in Linda G. Martin and Samuel H. Preston, eds., *Demography of Aging*, National Academy Press, 1994, chapter 3.

Railway Age, Simmons-Boardman Publishing Company, Various Years 1921-1934.

Ransom, Roger L and Richard Sutch, "The labor of older Americans: Retirement of men on and off the job, 1870–1937," *The Journal of Economic History*, 1986, *46* (1), 1–30.

Rosenbloom, Joshua L and William A Sundstrom, "The sources of regional variation in the severity of the Great Depression: evidence from US manufacturing, 1919–1937," *The Journal of Economic History*, 1999, *59* (3), 714–747.

(RRB) Railroad Retirement Board, *Annual Report of the Railroad Retirement Board*, US Government Printing Office, Various Years.

Ruggles, Steven, Sarah Flood, Ronald Goeken, Josiah Grover, Erin Meyer, Jose Pacas, and Matthew Sobek, "Ipums usa: Version 11.0 [dataset]," *Minneapolis, MN: IPUMS*, 2021, *10*, D010.

Samwick, Andrew A, "New evidence on pensions, social security, and the timing of retirement," *Journal of Public Economics*, 1998, *70* (2), 207–236.

Sass, Steven A, *The promise of private pensions*, Harvard University Press, 1997.

Schreiber, David B, *The Legislative History of the Railroad Retirement and Railroad Unemployment Insurance Systems* number 53, US Government Printing Office, 1978.

Silverman, AG and Joseph J Senturia, "Retirement Payments for Railroad Workers," *Soc. Sec. Bull.*, 1939, *2*, 3.

Song, Jae and Joyce Manchester, "Have people delayed claiming retirement benefits-Responses to changes in social security rules," *Soc. Sec. Bull.*, 2007, *67*, 1.

(SSA) Social Security Administration, "Social Security Bulletin: Statistical Supplement.," Various Years.

(SSA) Social Security Administration and Office of Policy and Office of Research, *Fact Sheet: SOCIAL SECURITY*, Government Printing Office, 2022.

_ , *Fast Facts and Figures About Social Security 2022*, Government Printing Office, 2022.

Stock, James H and David A Wise, "Pensions, the Option Value of Work, and Retirement," *Econometrica: Journal of the Econometric Society*, 1990, pp. 1151–1180.

_ **and** _ , *The Pension Inducement to Retire: An Option Value Analysis*, University of Chicago Press, 2008.

The Pennsylvania Railroad Company, *The Pennsylvania Railroad Company Annual Report* Various Years.

Thompson, Gregory Lee, *The passenger train in the motor age: California's rail and bus industries, 1910-1941*, Ohio State University Press, 1993.

United States Federal Coordinator of Transportation, "Annual earnings of railroad employees, 1924-1933," 1935.

U.S. Congress, *Railroad Employees Retirement System* 1934.

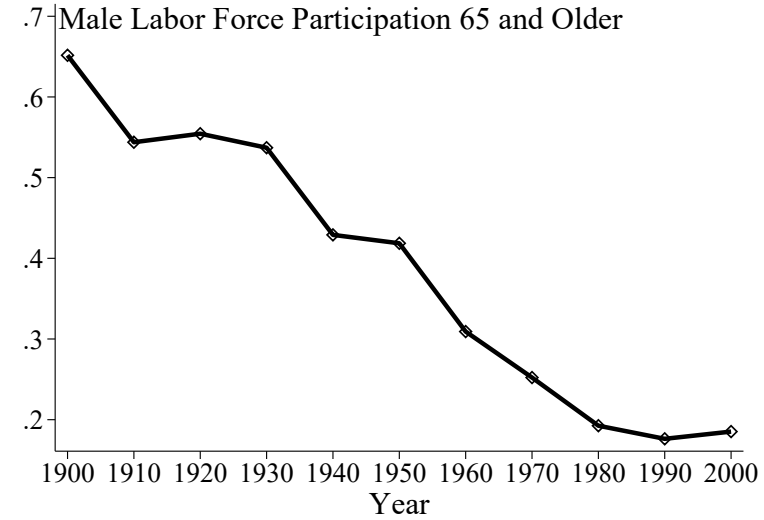
U.S. Supreme Court, "Railroad Retirement Bd. v. Alton R. Co.," 1935.

Williamson, Samuel H, "The development of industrial pensions in the United States during the twentieth century," 1997.

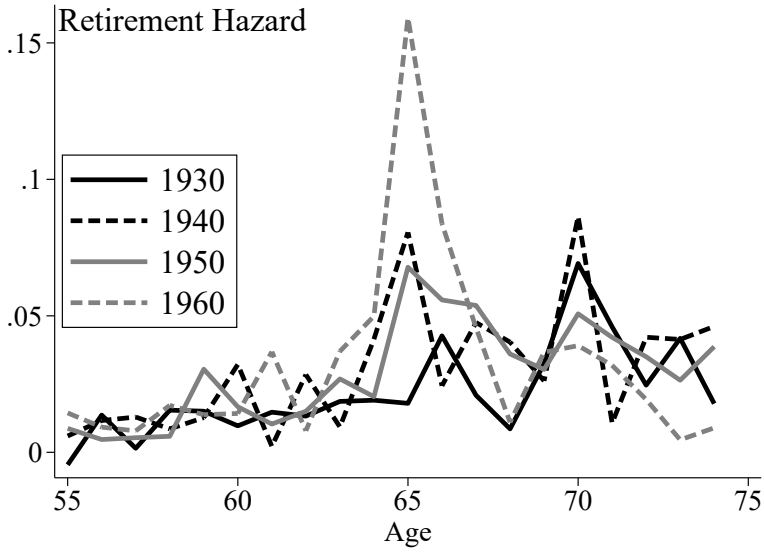
Figures and Tables

Figure 1

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

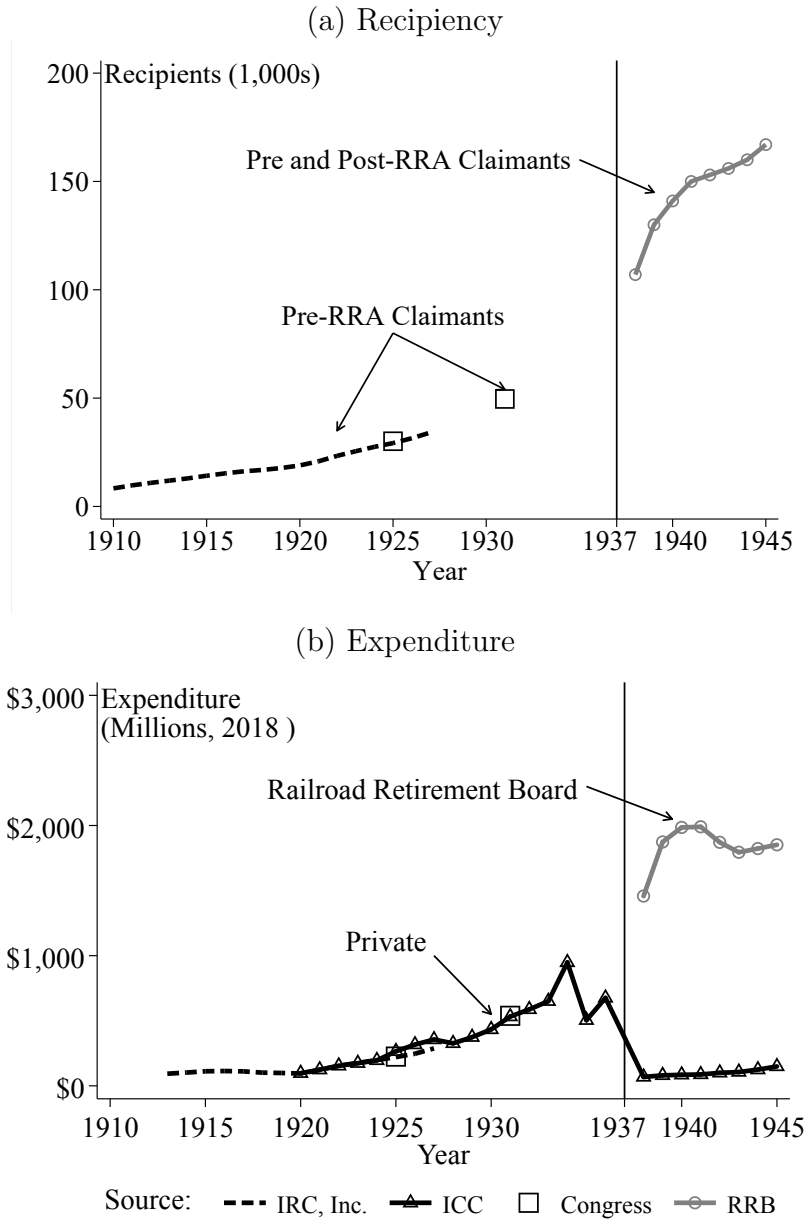


(b) Retirement Timing of Men: 1930-1960



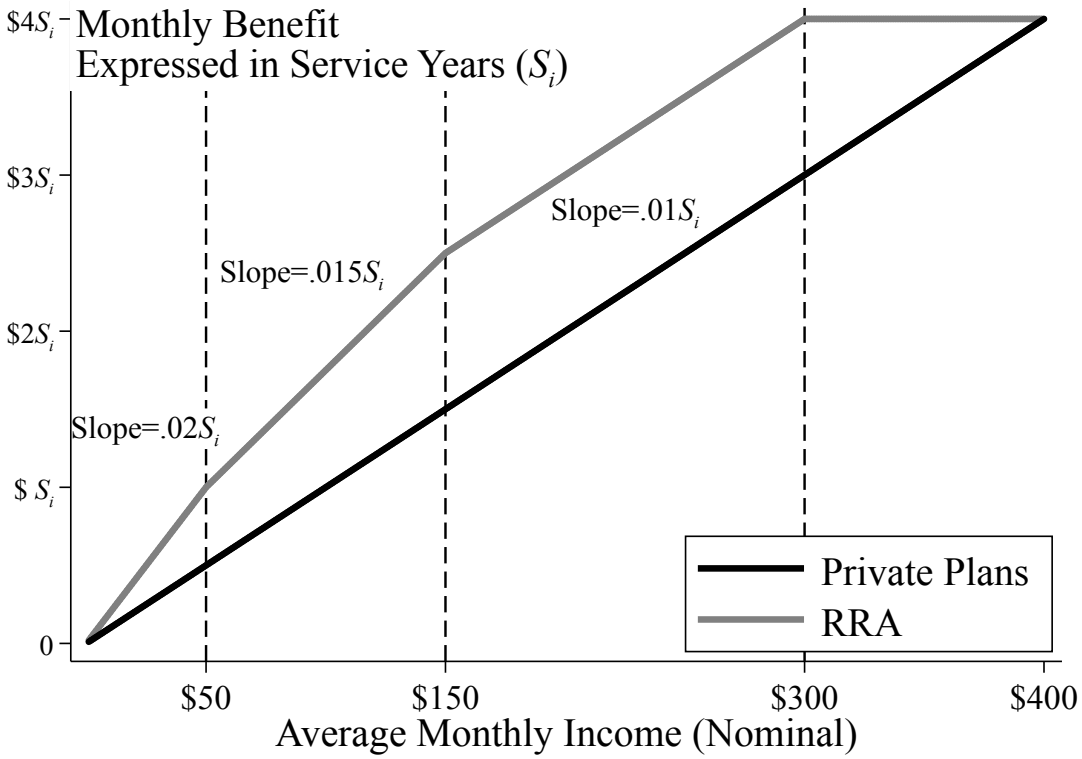
Notes: Panel (a) plots elderly male labor force participation (LFP) 65 plus (share of individuals 65 plus not in the labor force) as estimated from 1 percent Decennial Census samples (Ruggles et al., 2021). For 1940 and after, the series is formed from the unified IPUMS "employment status" question. 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 13.)

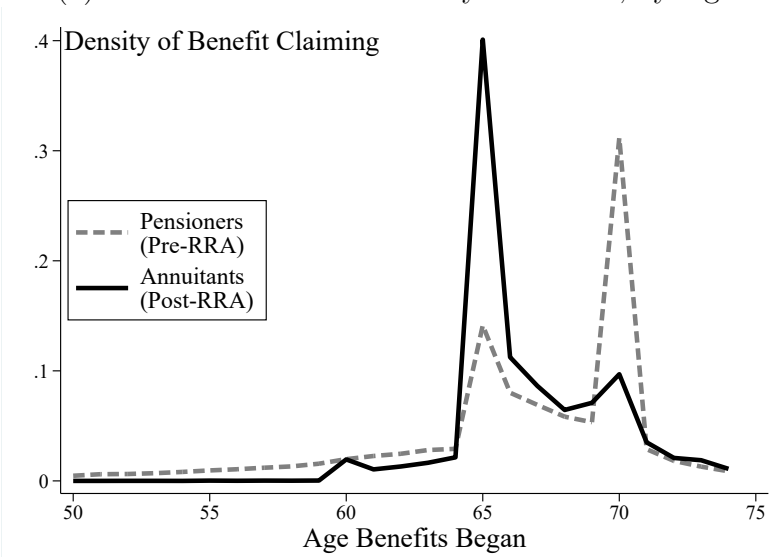
Figure 3: Newly Progressive Benefits Under the RRA



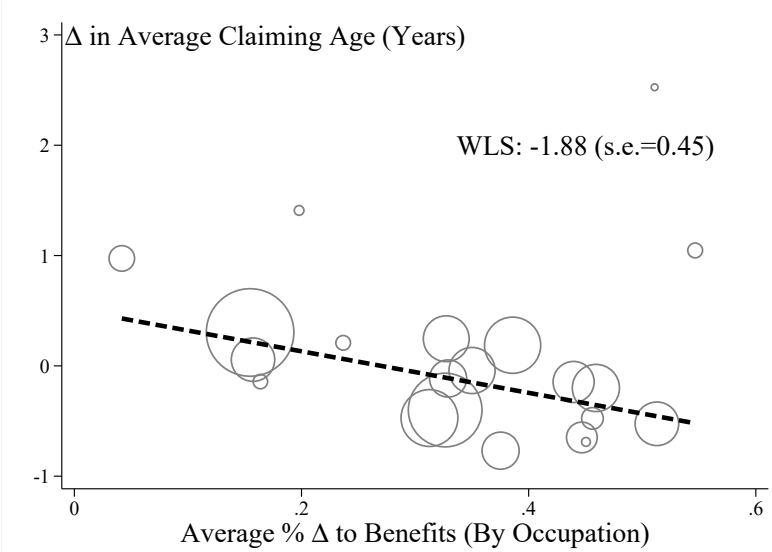
Notes: This figure plots a typical pre-RRA private railroad pension benefit formula (see Table A.1) in black, and the formula common to all railroad workers covered by the RRA in gray. The y-axis is in terms of service years S_i. The slope values represent the slope of the schedule in the relevant region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

Figure 4: Claiming Ages and Benefit Changes

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

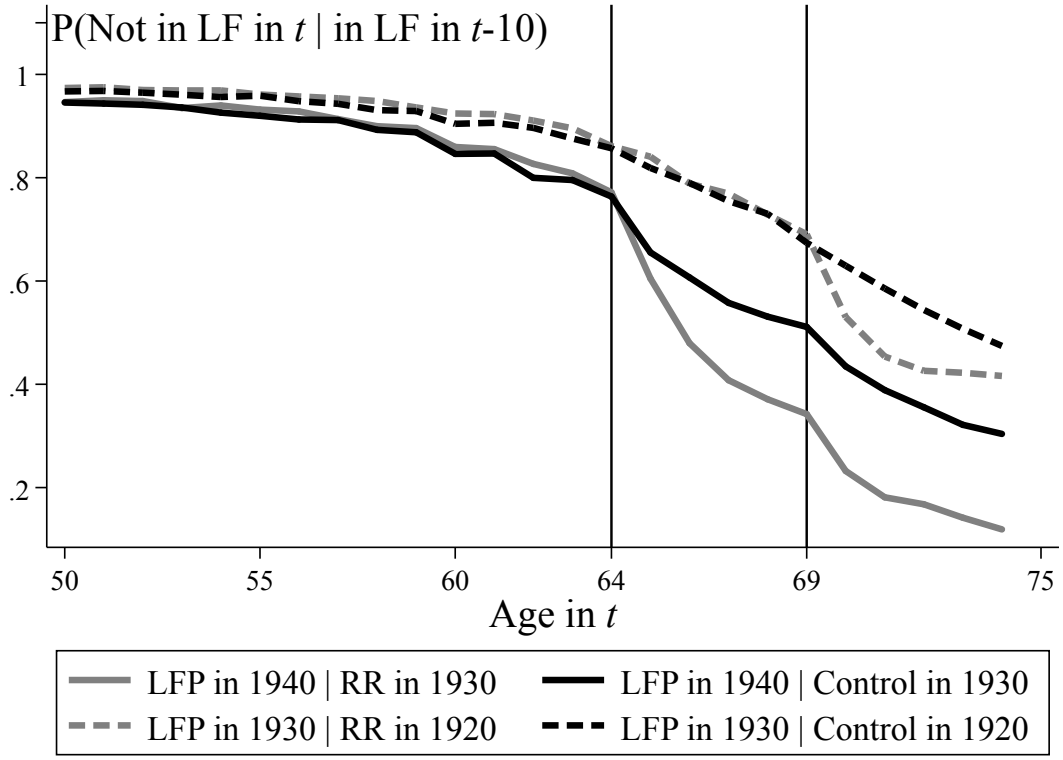


(b) Benefit Growth and Retirement Timing



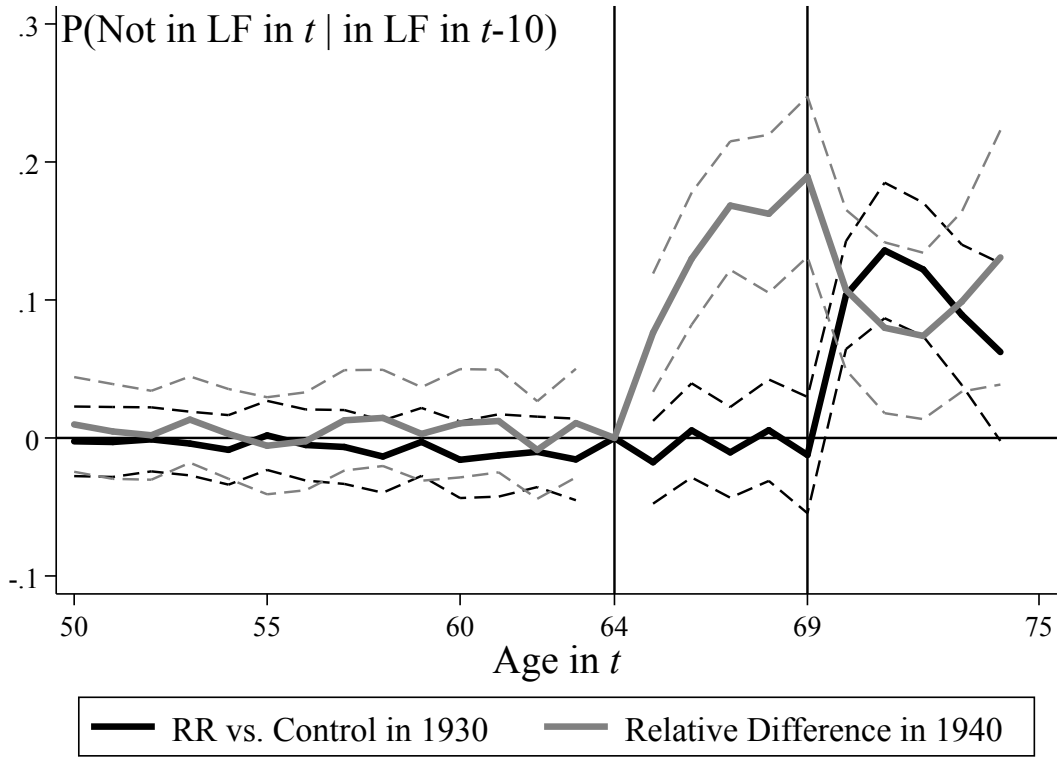
Notes: Data for panel (a) come from the RRB (1938 p. 99; 1941 pp. 211,212). The gray, dashed line is the density of benefit claiming age (in years) for pre-RRA private railroad pension plans, 90 percent of whom claimed between 1926-1935. I include both retirees under "age" and "disability" annuities (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 25 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 26 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 32).

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 32). 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: $x_{i,t} = \beta_0 + \beta_1 \times \text{RR}_{i,t} + \beta_2 \times \mathbf{1}\{t = 1930\} + \beta_3 \times \mathbf{1}\{a(i,t+10) \geq 65\} + \beta_4 \text{RR}_{i,t} \times \mathbf{1}\{t = 1930\} + \beta_5 \text{RR}_{i,t} \times \mathbf{1}\{a(i,t+10) \geq 65\} + \beta_6 \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i,t+10) \geq 65\} + \beta_7 \text{RR}_{i,t} \times \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i,t+10) \geq 65\} + \varepsilon_{i,t}$. All regressions are reweighted using weights generated to match the 1940 population at risk of being linked (see FN 32).

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	21,717	76.5	67.8	68.1	3.8
	(2.2)					
66	13	19,229	74	62.9	63.6	6.3
	(2.4)					
67	16.8	17,182	71.7	58.7	59.5	6
	(2.4)					
68	16.2	17,548	69.4	55.8	56.6	6
	(2.9)					
69	18.9	15,203	66.2	52.1	53	6.4
	(3)					
70	10.7	18,205	60.2	45.7	46.3	4
	(3)					
71	8	10,861	57.2	42.8	43.2	2.6
	(3.2)					
72	7.4	12,935	54.1	39.3	39.7	2.5
	(3.1)					
73	9.9	11,659	50.6	36.2	36.7	3.6
	(3.3)					
74	13.1	10,324	47.1	32.5	33.2	4.6
	(4.7)					
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	.013	$RR_{i,1930} \times \%\Delta B(\overline{w}_i)$	-.019	.004	.003	.001	.001
(Falsification			(.013)	(.004)	(.004)	(.005)	(.005)
Test)		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 49). Standard errors are clustered at the level of wage bin.

Appendix to (For Online Publication Only)

Matthew Pesner
University of Michigan

A Additional Results

A.I Defining Labor Force Participation

The primary analysis uses the unified IPUMS variable "employment status" to measure LFP outcomes in 1930 and 1940. It is important to note that 1940 marked a departure from the concept of "gainful employment" (GE) to the modern definition asking about work during a specific reference period, and measures of GE from 1930 or earlier years may overstate participation relative to the modern definition (Durand, 1948; Costa, 1998a; Moen, 1988).[1] Recall the main analysis sample is first restricted to those employed in the base year of each link (1920 and 1930) before linking. This necessitates use of the GE measure for defining who is potentially linked. The primary reason I do not use GE for 1940 *outcomes* is because the focus is on changing retirement behavior in 1940 and measures based on GE are both less consistently measured and less accurate portrayals of labor market involvement.[2] While there is no reason to expect changes in LFP measurement will lead to differential measurement error in the dependent variable by industry and age in a window around 65, I explore sensitivity to defining LFP to instead indicate if an individual was GE in both years (has a "non-occupational response" in 1940).

Figure A.13 plots the results from (3) using GE as the LFP (outcome is not GE). The patterns are quite similar to those in Figure 6, with flat existing differences at all ages <70, large existing effects at 70, spikes in relative effects at 65, and declines in relative effects at age 70, while relative estimates are slightly smaller in magnitude than prior. This suggests that the estimated effects of the RRA on nonparticipation are not an artifact of changes to how LFP is measured in the Census.[3]

A.II Specification Choice

My use of microdata with detailed industry, age, and geographic information allows for a variety of specification checks that limit comparisons to more or less restrictive groups. In this section I show the patterns and magnitudes in Figure 6 are maintained across a variety of robustness checks including various sets of county-level fixed effects, controlling for covariates, clustering at different geographic levels, and limiting comparisons to be within occupational income score or within occupation.

Specifically, Figure A.14 plots the point estimates of (3) for 9 variants as well as the baseline results in bold. The variants are: (1) the baseline specification without the use of weights; (2) the baseline specification plus controls for race, number of children, and marital status (all in $t-10$);

[1] Differences in defining elderly LFP, which largely hinge on the labor market classification of the long-term unemployed, suggest somewhat different trends in retirement prior to Social Security (e.g., Ransom and Sutch, 1986; Moen, 1987; Margo, 1993).

[2] The exact instructions to Census enumerators on how to classify GE changed over time. Moen (1988) documents that, while the 1930 Census was the first in which they were instructed to check that an individual usually worked at least one day a week in the occupation, it appears that these instructions were not systematically followed.

[3] The likelihood that an individual gives a non-occupational response given they worked in 1939 is very small in the sample, which rules out a similar exercise for the elasticity results.

(3) the baseline specification plus county-by-age fixed effects; (4) the same as (3) plus county-by-railroad status fixed effects; (5) the same as (4) plus county-by-period fixed effects; (6) the same as (5) but clustering at the state level; (7) the same as (5) but including occupation score fixed effects; (8) the same as (7) but including occupation score-by-county fixed effects instead; (9) the same as (8) but including occupation-by-county fixed effects instead. [4] The patterns are quite similar as in Figure 6, with flat existing differences at all ages under 70, large existing differences beginning at age 70, spikes in relative effects in 1940 at 65, and declines in relative effects at age 70.

Robustness across these specifications has the following direct implications: the baseline specification indicates simple comparisons of means across groups within age is enough to identify the effect of the RRA while (1) shows that using weights generated to make the sample representative of the population at risk of being linked is, in practice, not important; (2) shows that controlling for other variables shown to impact retirement (see Section II) does not change conclusions; (3), (4), and (5) show that controlling arbitrarily for for geographic, time, industry, and age variation that may correlate with labor market participation does not change conclusions; (6) shows that the geographic level of clustering is not important; finally, and perhaps most importantly, (7), (8), and (9) show that limiting comparisons to be among workers of similar earnings or occupation does not change results.

Applying the procedure of Section IV to estimate the share of 1930s aggregate LFP explained by the RRA places the share between 11-12 percent of the previously unexplained share across all specifications save (2) (without weights), which places the share slightly lower at 9 percent.

A.III Linkage Algorithm

My choice of Helgertz et al. (2020) as the main linkage algorithm for both sets of analyses is primarily because of the larger resulting sample sizes, which are particularly helpful for the elasticity estimation strategy in Section V that requires much more stringent sample restrictions. As described in Section V, the use of the "exact-conservative" method from Abramitzky et al. (2020) for further linkages back to 1910 is chiefly due to availability of the algorithm for non-consecutive Census years.

This section first shows that the first set of results based instead on a sample using the algorithms provided by Abramitzky et al. (2020) are similar. These authors provide 4 algorithms, so for the sake of completeness I present results for each one, as well as the intersection, which is the most conservative procedure (Bailey et al., 2020).[5] Figure A.15 plots the nonparticipation results for each of the Abramitzky et al. (2020) links with the main results using Helgertz et al. (2020) from Figure 6 in bold. As with Figure A.14, I omit confidence intervals for expositional purposes. Applying the procedure of Section IV to estimate the share of 1930s aggregate LFP explained by the RRA places the share between 9-11 percent of the previously unexplained share across the

[4] I omit confidence intervals for expositional purposes. With the exception of the unweighted regression (specification (2)), all of the confidence intervals contain 0 for preexisting and differential differences at ineligible ages, and preexisting differences at pension-eligible ages through age 69 (note that the unweighted specification still produces differences that are close to zero). All confidence intervals don't contain zero for the differential effects at newly eligible ages.

[5] The advantages and disadvantages of linkage techniques from various groups of researchers is a current and fruitful literature in economic history; see Bailey et al. (2020) and Abramitzky et al. (2019) for more details.

five linkage algorithms. These results show the patterns and magnitudes are all maintained across linkage type.

I next show that the choice of algorithm used for linkages back to 1910 does not impact the elasticity estimates. Table A.8 shows that the hazard elasticity estimates at age 65 are consistent across the other three algorithms provided by Abramitzky et al. (2019), as well as the intersection. The specific linkage technique does not impact any of the results.

A.IV Choice of Treatment and Control Groups

As summarized in Section II and detailed in Appendix B, I select railroad industries (and some occupations) based on the RRA legislation and comparisons between the number of workers with credited earnings in 1940 and the 1940 complete count Census. I select control industries based on comparisons between Latimer (1932) and the 1930 complete count. I first test sensitivity to control industries by re-running (3), where the control group now includes all non-railroad, non-agricultural linked workers in the United States (conditional on working in the base year and for ages 50-74 in the later year, as with the main analysis). These results are shown in Figure A.16. Given the discussion in Section I of seniority rights on railroads and high unemployment among younger workers in that industry, it is not surprising that 1930 LFP among railroad workers in their 50s was higher than average, or that the 1930-1940 decline was larger. Further, pre-existing differences at ages between 65-70 are expected because of pre-RRA pensions. Nonetheless, particularly at ages 65 and above, the patterns present in Figure 6 are borne out with the (much) larger control group (and are larger in magnitude). Similar patterns using all other workers has the attractive feature of ensuring that results using *any possible choice* of a larger control group will lie within these and the main results, and therefore be broadly similar. On the other hand, non-zero existing and relative differences at pension-ineligible ages lends further support to my choice of control industries, for which these differences are not distinguishable from zero.

I next investigate whether dropping any control industry alters the point estimates. Table A.5 presents estimates from separate specifications – each leaving out one of the 21 control industries – in each row. For expositional purposes, I present estimates from a summary "triple-differences" specification of (3), with indicators for 65-69 and 70-74 in place of dummies for ages 65-69 and 70-74, respectively, and 0 in place of dummies at lower ages. For reference, the first row shows the results from the full analysis sample. The effects remains quite stable at both age groups and are always significant at the 99 percent level. Particular industries included in the control group are not driving results.

Finally, I also investigate whether any railroad occupations are driving estimated effects.[6] While there are 113 occupations represented among railroad workers, I test sensitivity to leaving out the 23 occupations with over 1,000 workers (summed across 1920 and 1930) as well as a 24th regression leaving out the remaining workers ("all other occupations"). Table A.6 presents estimates from the same summary specification as in Table A.5 and shows effects barely change for both age groups and are always significant at the 99 percent level. Workers in particular railroad occupations are not driving results.

[6] I focus on occupations because over 97 percent of railroad workers are in the 1950 industry code 506 (see Appendix B) so omitting particular industries is infeasible.

A.V Differential Mortality

If differential mortality among railroad and control workers led to differential linkage linkage probabilities at higher ages, then results may be biased by selection on correlates with mortality that also impact retirement (such as poor health). There is reason to expect this is not the case – although railroads were notoriously dangerous at the turn of the 20th century, they were much safer by the 1930s, with comparable mortality probabilities to other industries (Aldrich, 1997).[7] Indeed, while proponents of the RRA argued it would further the goal of safety for older workers, trends in working conditions had made this a nonissue. The weakness of this argument ended up being a major reason for failure of the original RRA in *Railroad Retirement Board V. Alton Railroad Company* (1935), with Justice Owen Roberts writing in the majority opinion that "Incontrovertible statistics obtained from the records of the interstate Commerce Commission show a steady increase in safety operation, during this period of alleged increasing superannuation... We think it not unfair to say that the claim for promotion of safety is virtually abandoned." (U.S. Supreme Court, 1935, pp. 12-13).

Robustness to occupation fixed effects (see Appendix A.II) lends some support to the idea that specific occupational hazards are not driving labor force nonparticipation.[8] Nonetheless, I take a few steps to rule out differential selection due to mortality. My strategy is twofold: I first show that patterns of linkage probabilities over the age profile are quite similar to patterns in age-specific 10-year survival probabilities based on death registration areas, so that *differential linkage* between railroad and control workers may provide a decent proxy for differential mortality. I then estimate the main specification for all workers at risk of being linked in 1920 and 1930, where the outcome is an indicator for whether they are linked to the following period.

I proceed by using period life tables from the Social Security Administration for 1920, 1930, and 1940, which give the probability of surviving to age $a+1$ conditional on age a (Bell and Miller, 2005). I linearly interpolate the 1-year survival rates by age for intercensal years and calculate the age-specific probability of 10-year survival for ages 40-64. I next calculate the probability of linkage to the following Census by age for all men ages 40-64 in 1920 and 1930, using the same algorithm as in the main analysis (see Section II. Figure A.17 plots the 10-year survival probabilities and linkage probabilities, both in levels and logs, between 1920-1930 and 1930-1940. While the probability of survival is expectedly much higher than the probability of linkage, particularly at earlier ages, the trends illustrate that they follow similar patterns of negative growth over the age profile. Indeed, regressions of ln(P(link)) and ln(10-yr survival) on age in 1920 yield quite similar slope coefficients of -0.018 (s.e.=0.002) and -0.017 (s.e.=0.001). The same is true in 1930, with coefficients of -.020 (s.e.=0.002) and -.017 (s.e.=0.001), respectively. The patterns suggest *differential* probabilities of linkage might represent a reasonable proxy for *differential* mortality.

I next estimate (3) on the full sample of workers in railroad and control industries in 1920 and 1930 ages 40-64, where the outcome is an indicator equal to 1 if the worker is linked to the following year (the sums are now indexed relative to age 54, and the outcome is contemporaneous).

[7] Aldrich (1997) calculates in 1900 there were 2.21 deaths per 1,000 railroad workers annually, but by 1934 this number had dropped to 0.52, roughly comparable to manufacturing, depending on the state (between 0.1 and 0.29), and safer than mining (.9).

[8] Note also that Alter and Williamson (2018) find that any occupational differences in mortality among pensioners of the Pennsylvania Railroad System between 1900-1920 are removed when income is controlled for.

Null results will indicate differential mortality is unlikely. Figure A.18 plots the results from this regression, where the x-axis is now in terms of age in the base year.[9] The results show little evidence of differential selection into linkage based on railroad-worker status by age, with the 95 percent confidence intervals including 0 at every most ages in the sample, and all ages save for 57 and 62, neither of which correspond to a focal retirement age in the following decade. Reassuringly, there does not appear to be any evidence of differential selection due to mortality.

A.VI Elderly Public Assistance and Social Security

Old Age Assistance

Old Age Assistance (OAA) was a means-tested public assistance program created by the Social Security Act of 1935, and the largest elderly transfer program in 1940. Eligibility generally began at 65 and generosity varied substantially across counties (Fetter, 2017). While the cross-cohort and cross-industry comparisons of my research design and robustness to fine county-by-age controls in Appendix A.II go far in ruling out other programs at age 65 as driving results, if railroad workers lived in areas that provided more (or less) generous OAA benefits, it might imply that aggregate elderly railroad counterfactual labor supply was over (or under) estimated.

I use data from Fetter and Lockwood (2018) on county OAA recipiency per person and payments per recipient (generosity) in 1939 to test whether these are related to the share of county employment ages 40-64 on railroads in the 1930 complete count census. To the extent that the RRA affected 1939 OAA recipiency, it would crowd out some need, which may indicate that the federal government recouped some expenditure on the RRA through OAA.[10] On the other hand, if OAA programs are differentially generous in higher railroad counties the differential attractiveness of alternative pensions could complicate interpretation of estimates as counterfactual railroad labor supply, especially for low wage railroad workers

A regression of the county share 65 plus receiving OAA on the share employment in 1930 controlling for fixed effects and clustering at the state level implies a 1 percent increase in railroad share is associated with a 0.219 percent (s.e.=0.05) decline in OAA share. Given that the mean railroad share in 1930 was 3.48 percent and the mean 1939 OAA share of the elderly was 27.0 percent, this is quite small, but may be indicative of positive fiscal externalities on federal and state OAA savings. A similar regression with nominal payments per recipient as the outcome indicates that a 1 percent increase in share is associated with a \$.08 higher OAA monthly payments, a negligible effect comprising roughly 0.5 percent of average monthly OAA payments at the time. These results indicate that the OAA program did not vary in a systematic way that would bias my results and provide some suggestive evidence that the RRA may have crowded out a small share of OAA expenditure.

[9] As described in the main text, the algorithm matches individuals across census years within 3 years of age. Of course, for individuals not linked, there is no way to assess what age they reported in the second census year, so I proceed by effectively assuming the age match was precise (so that age 54 in $t-10$ corresponds perfectly to age 64 in t. Since the interest lies in general patterns over 25 ages, this does not change the interpretation much.

[10] This is consistent with Fetter and Pesner (2021), who show that states with initially larger shares of employment covered by Social Security had relatively smaller OAA programs, and that the large expansions in Social Security coverage to new groups in the 1950s also crowded out significant OAA expenditure.

Social Security

The uniformity of Social Security rules across geography suggest little scope for these benefits to bias the effect of the RRA on retirement. Social Security covered all workers in commerce and industry with the exception of railroads indicating that, while speculative, it is likely that Social Security would have originally covered these workers in the absence of the RRA. As with OAA, Social Security benefits were far less generous than railroad, utility, and manufacturing pensions (see FN 3), so although control workers likely had access to the program, their private pensions were more attractive. Further, since the counterfactual of the RRA was likely coverage under both private and public pensions, this seems like a reasonable counterfactual. Thus, if access to Social Security inflated control worker exit relative to a state of the world in which they only had access to private pensions, this should be reflected in my estimates.

A.VII The Depression and Industry-Specific Trends in Employment

The Depression exacerbated a previously mounting secular decline in railroad revenue (see discussion in Section I.A. While flat differences in labor force nonparticipation at pension-ineligible ages (Figure 6) provides strong evidence against differential changes in employment opportunities between railroad and control workers at these ages, it is theoretically possible that industry-specific trends in labor market tightness bias comparisons at higher ages beginning right at 65. If railroad workers were more likely to have exited the labor force because of a lack of employment opportunity anyway, this would overstate estimates. On the other hand, as discussed in Section IV, seniority rules ensuring those on the job the longest were given preferential treatment to remain employed was originally the primary motivation behind passage of the RRA, which would bias results in the other direction.

While I cannot test this directly, stark geographical variation in unemployment rates in the 1930s suggests a natural check on outside economic opportunity. I estimate the main specification (3), separately by quartile of the 1930 county level unemployment rate for men ages 40-64 *not working on railroads*. I plot these coefficients in Figure A.19 with each panel representing results for each quartile. Across quartiles, the patterns are quite similar to that of Figure 6. In a specification that instead interacts each independent variable in (3) with dummies for quartiles 2-4, F-tests only reject equality across quartiles at the 5 percent level at ages 62, 66, and 70. This supports the notion that estimated effects are not a product of the high unemployment environment.

A.VIII Using Elasticities to Estimate the Effect of 1950s Social Security Benefit Increases

To calculate the effect of the 1950s *benefit increases* on claiming at ages 65-69, I focus on those individuals who were already eligible for benefits under the 1939 Amendments. 44 percent of eligible men in this age group were receiving (had claimed) benefits in January, 1950 (SSA, 1959 p. 18). Nominal benefit levels had not been adjusted since 1939 and high inflation during the 1940s had led to stark declines in real benefits (benefits were not pegged to inflation until 1975). Thus, by the late 1940s, replacement rates (benefits as a share of average wages) had declined substantially to less than 20 percent for a worker with "medium" earnings (Clingman et al., 2014). Indeed, replacement rates have never gotten so low since.

The goal is to use the elasticity estimates to infer how much of the increased claiming is due to higher benefits. I relate the decadal benefit and claiming changes– rather than annual changes – chiefly because frictions may lead to some gap between benefit increases going into affect and currently eligible or near-eligible cohorts responding (Manoli and Weber, 2016; Gelber et al., 2020). Further, year over year changes in claiming rates are likely impacted by idiosyncratic labor market tightness, while a decadal shift in claiming levels should reflect more structural changes. The thought experiment is that cohorts 65-69 in 1960 faced broadly higher benefits that were unexpectedly increased at some point after they were 55-59, and that cohorts aged 65-69 in 1950 provide a reasonable counterfactual to their claiming absent benefit expansions (I discuss this latter point in more detail below).

The observed change in claiming is (0.7-0.44), or 26 percentage points. 1960 claiming is only available in the published volumes for all eligible men (as of 1960), so I assume that the share of claiming among eligibles was the same in 1960 among those who would have been eligible under the 1939 Amendments and those that were not. I view this assumption as plausible because, in 1960, both groups faced the same benefit formula and were subject to the "new start" formula which only counted coverage quarter after 1950 in determining eligibility (Cohen and Myers, 1950).[11]

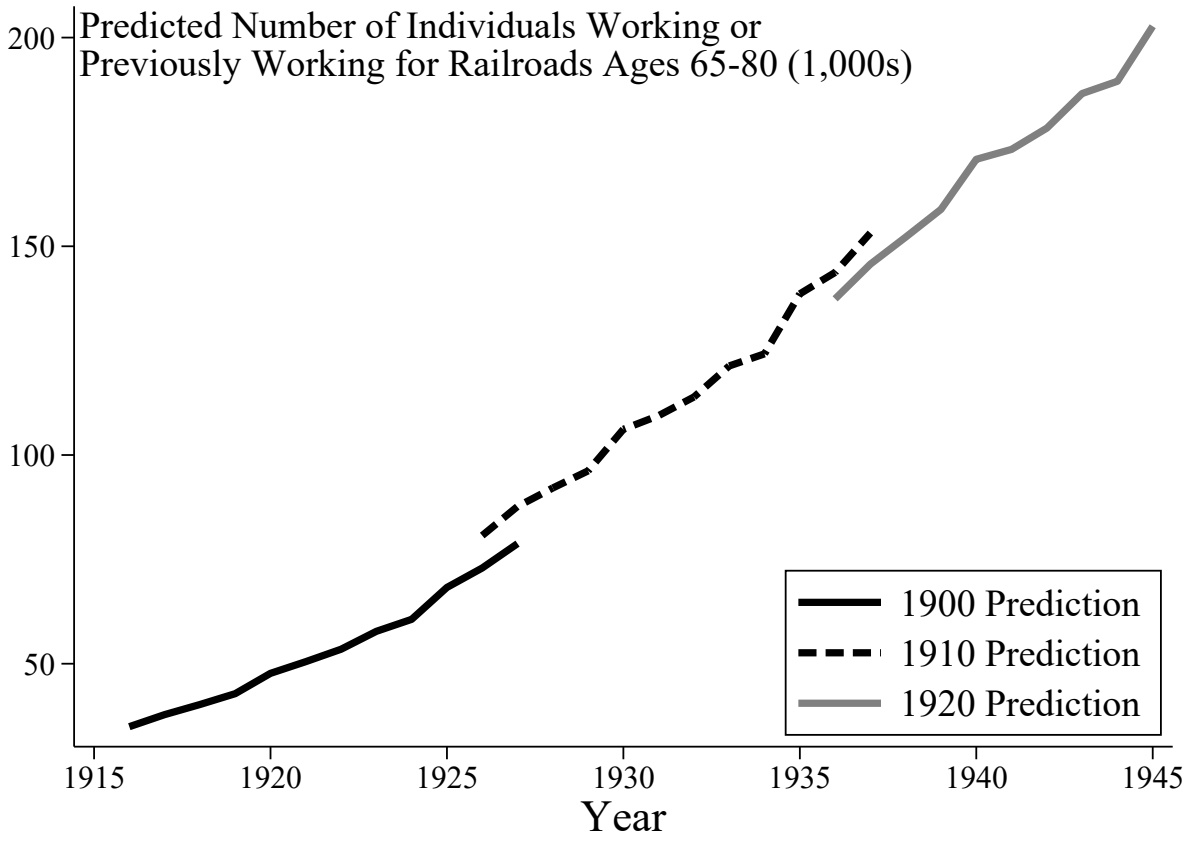
I proceed via two strategies; I first using the hazard elasticity estimate at age 65 of 0.18 at age 65, effectively assuming that all claiming between 65-69 occurred at 65 and hence a conservative estimate of the increase in claiming attributable to benefit increases. This implies that the 94 percent increase to real benefits of the 1950s led to a $0.18\times.94$ or 16.9 percentage point increase in claiming, explaining 16.9/26 or roughly 65 percent of the increase. I next use the elasticity of nonparticipation estimate for ages 65-69 of .55. Applied to the 1950 claiming share of 44 percent, this implies a claiming share by 1960 of $1.55\times.94\times.44$ or 64 percent, explaining (.64-.44)/(.7-.44) or roughly 77 percent of the increased claiming.

Note that, due in large part to the end of WWII, an existing trend had been towards more claiming in the mid-1940s (SSA, 1959 p. 18). It is impossible to know whether increased claiming would have continued into the 1950s had real benefit levels continued to decline, although it seems unlikely given the already-meager replacement rates. Any increase in claiming that would have taken place, however, reduces the counterfactual increase to be explained, thus increasing the share due to benefit expansions. Further, as noted in the main text, the percentage of claiming explained should be a lower bound for the share of LFP declines explained. For both of these reasons, I view this range as a plausible lower bound of the effect of benefit expansions in driving declining LFP in the 1950s.

[11] Earlier covered workers had more quarters of coverage, which may imply more incentive to retire, so that this claiming rate is underestimated. On the other hand, the combination of new eligibility and higher benefits occurring late in life for previously ineligible workers may mean they responded more, which would go in the opposite direction.

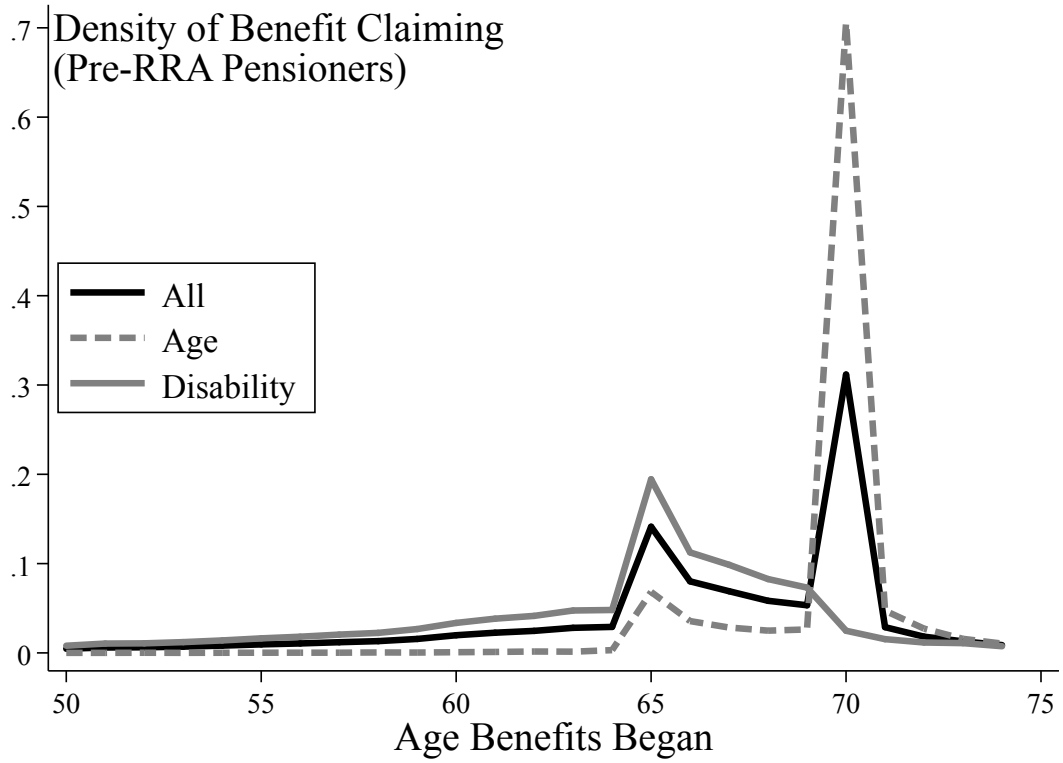
Appendix Figures and Tables

Figure A.1: Aging in the Railroad Industry: 1916-1946



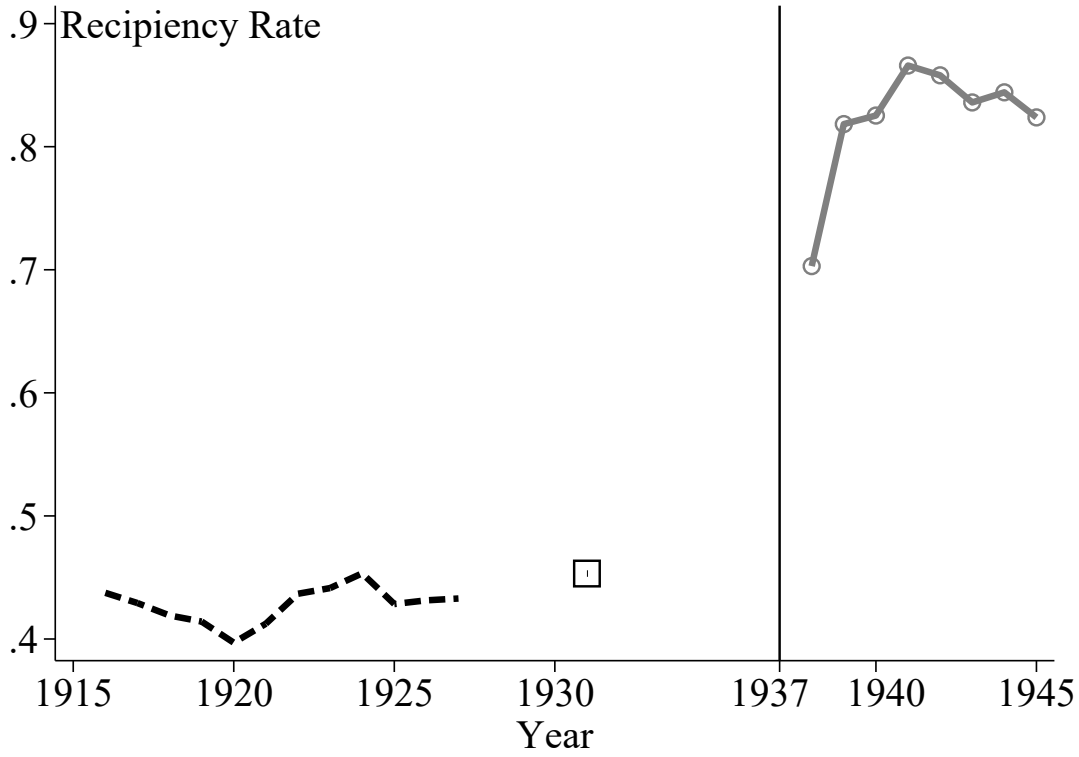
Notes: Plots the predicted number of individuals 65-80 who had worked for railroads between 1916 and 1945. Using the 1900, 1910, and 1920 complete count Censuses (Ruggles et al., 2021), I take the population of male railroad workers by age 49-64 and estimate populations in ensuing years using age-specific 1-year mortality probabilities (Bell and Miller, 2005) for 1900, 1910, and 1920, and linear interpolations (by age) for years in between. The time series begins in 1916 for the 1900 prediction (black, solid line) when these first cohorts are 65-80, 1926 (black, dashed line) for the 1920 prediction, and 1936 (gray, solid line) for the 1930 prediction.

Figure A.2: Pre-RRA Density of Pension Claiming Age, by Type of Claim



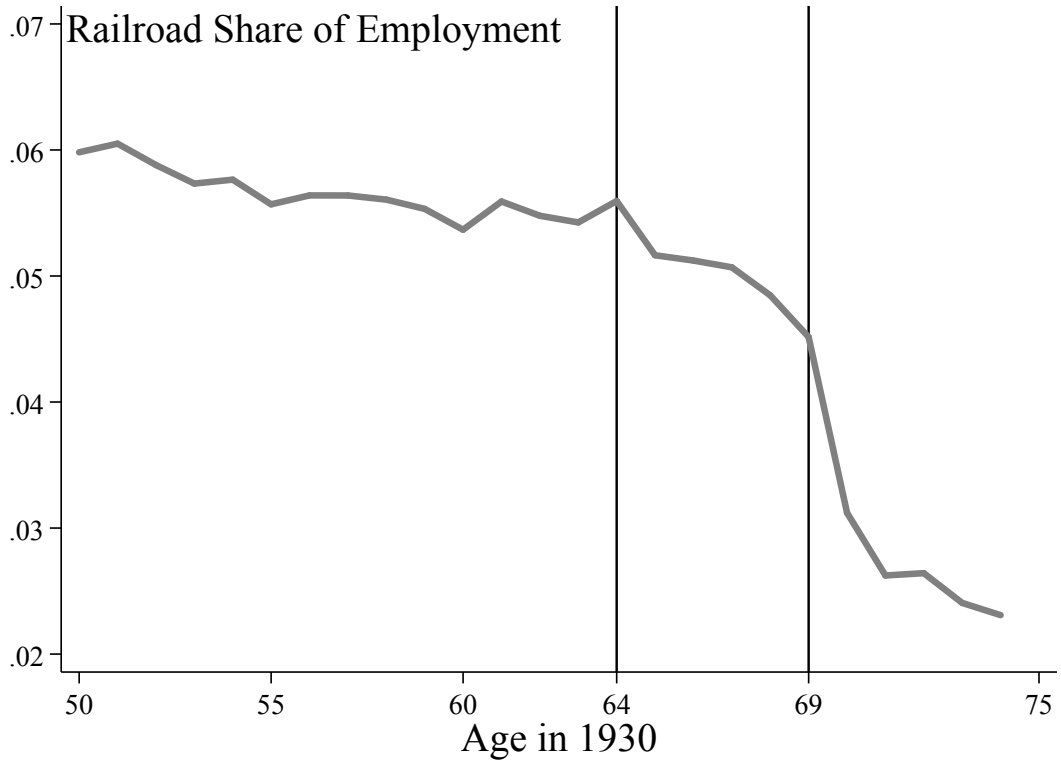
Notes: Data come from RRB (1938 p. 99). Decomposes the empirical probability density of retirement by age among all current "pensioners" in 1938 (black, solid line) into those retired under "age" (gray, dashed line), and "disability" (gray, solid line); see FN 25 for further details. Roughly 4.1 percent of pensioners claimed at ages outside of 50-74. Also see the notes to Figure 4 for more details.

Figure A.3: Railroad Pension Recipiency Rate: 1916-1945



Notes: This figure shows the estimated recipiency rate of railroad pensions per elderly railroad workers or retirees ages 65-80. Each color and shape represents both a different source for recipiency and a different estimate of the elderly railroad (working and retired) population. Recipiency sources are Latimer (1932) for 1910-1927 (black, dashed line); U.S. Congress (1934) for 1931 (black box), and Carter et al. (2006) Series Bf746-761) for 1938 and later (gray line with circles). Recipiency for 1910-1927 are from a non-exhaustive set of reporting railroads (see Latimer, 1932 beginning p. 159) for more details; information on the exact firms included in the survey is redacted). Population denominators are the predicted number of individuals 65-80 who had worked for railroads between 1916 and 1945. Using the 1900, 1910, and 1920 complete count Censuses (Ruggles et al., 2021), I take the population of male railroad workers by age 49-64 and estimate populations in ensuing years using age-specific 1-year mortality probabilities (Bell and Miller, 2005) for 1900, 1910, and 1920, and linear interpolations (by age) for years in between. The 1900 prediction is applied to recipiency through 1927, the 1910 for recipiency in 1931, and 1920 for recipiency in 1938 and later.

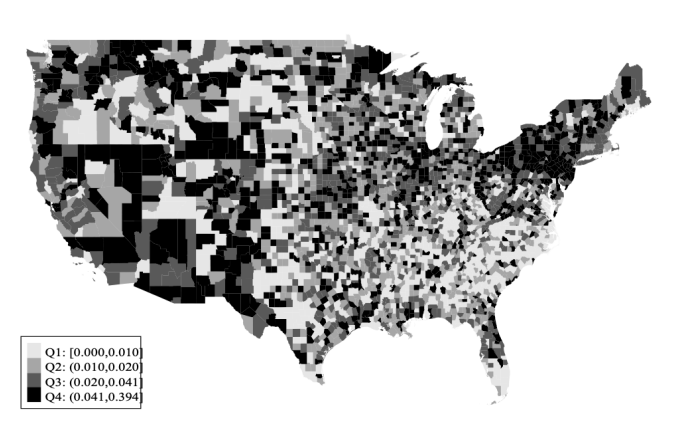
Figure A.4: Railroad Share of 1930 Employment



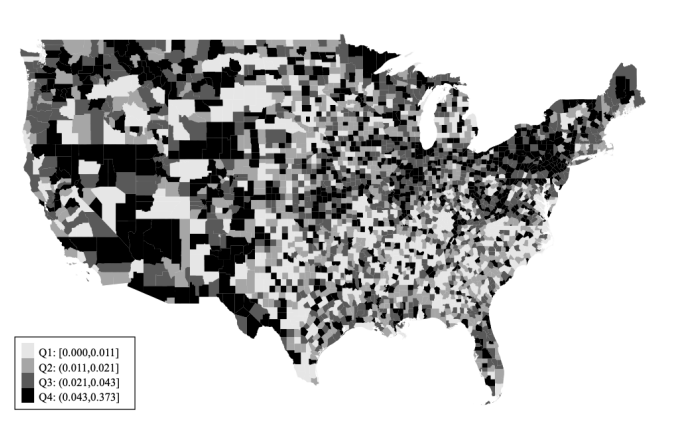
Notes: Data are from the 1930 complete count decennial census (Ruggles et al., 2021). Plots the 1930 railroad share of all non-agricultural employment (all 1950 industry codes other than 105) by age, defining railroad employment as in Section II and Appendix B.

Figure A.5: Railroad and Control Industry Share of Employment Ages 40-64 in 1920 and 1930

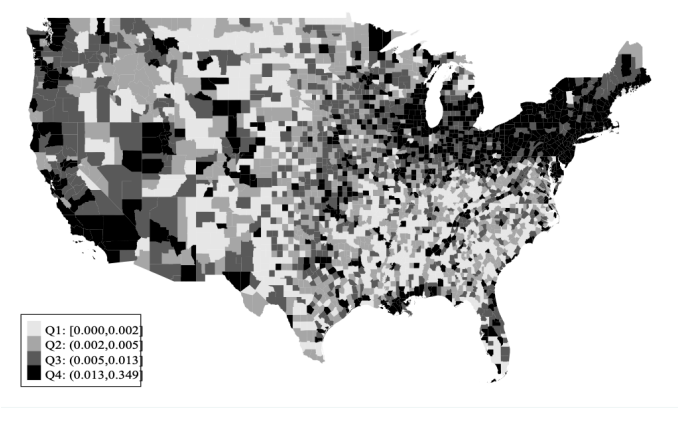
(a) Railroad 1920



(b) Railroad 1930



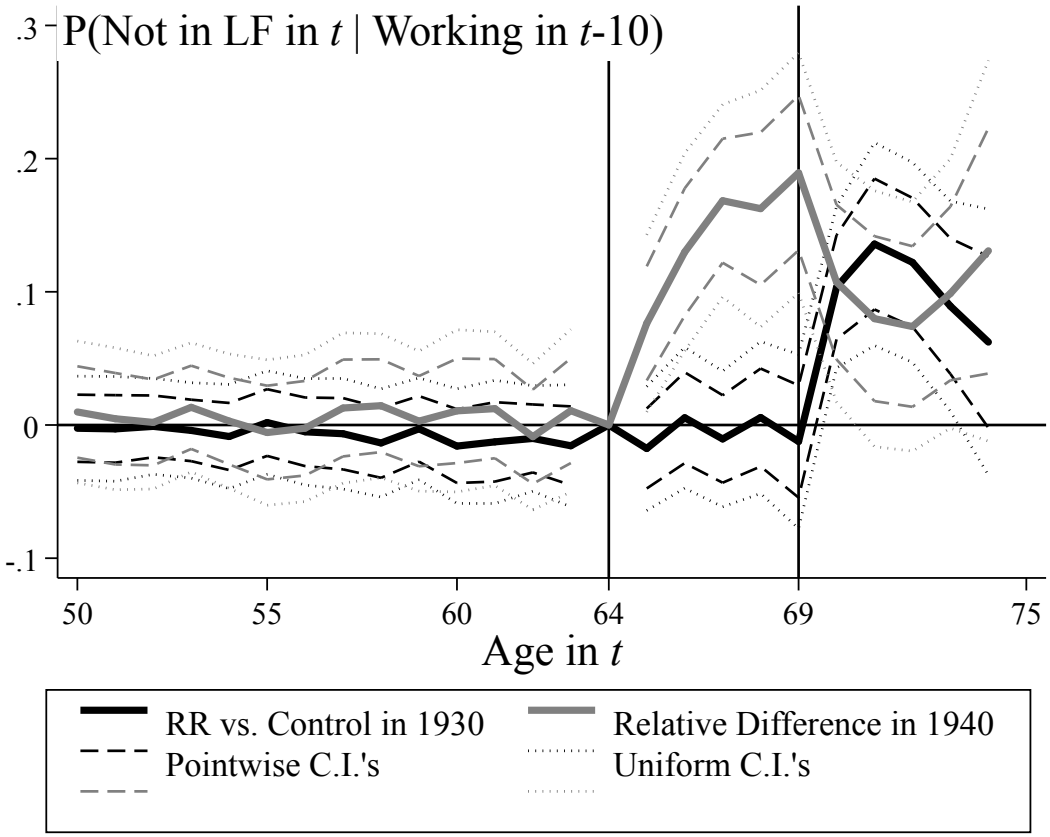
(c) Control 1920



(d) Control 1930

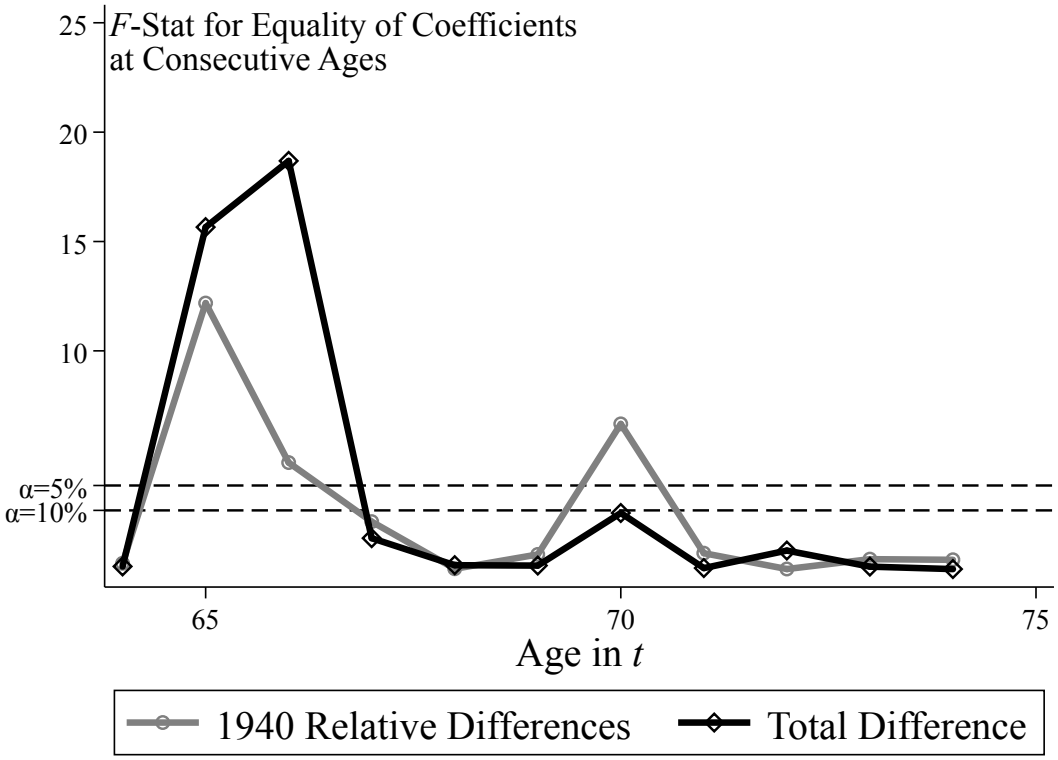
Notes: Data are from the complete count Decennial Censuses (Ruggles et al., 2021) for 1920 (Panels (a) and (c)) and 1930 (Panels (b) and (d)). Plots the county share of employment ages 40-64 who were working on railroads (panels (a) and (b)) or industries I classified as covered by pensions (panels (c) and (d)) (see Section II and Appendix B for more details).

Figure A.6: Uniform Confidence Bands for the Effect of the RRA on Labor Force Nonparticipation



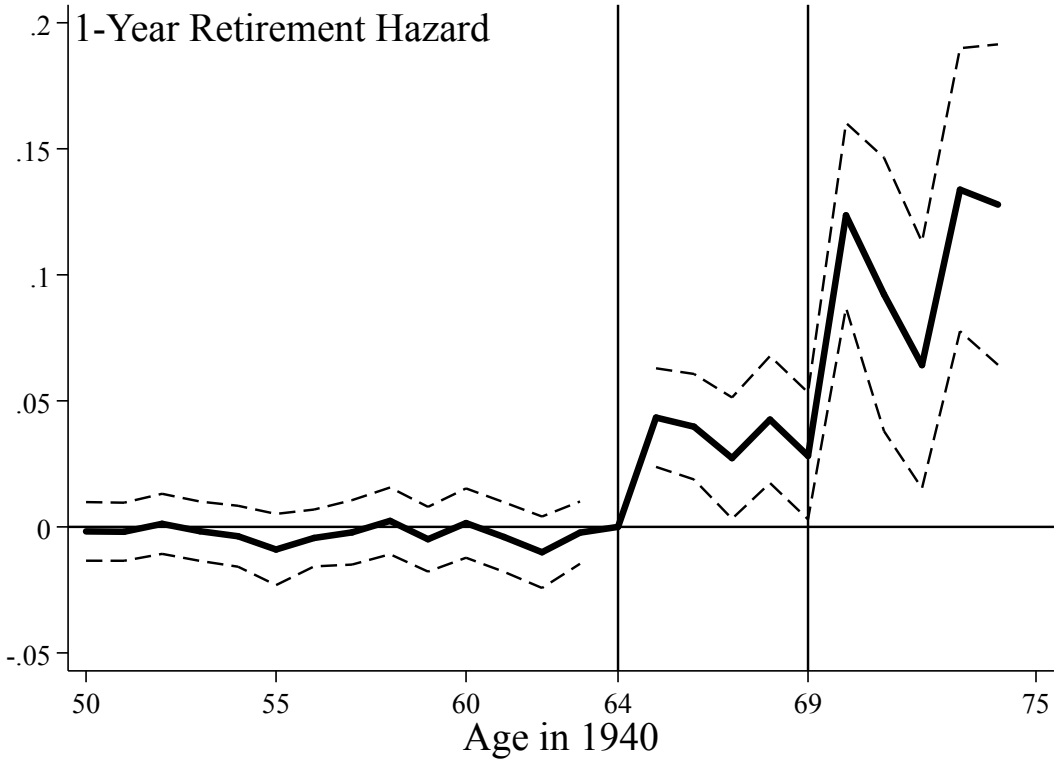
Notes: Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). 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 32). 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, with standard errors clustered at the county level for each set of coefficients, respectively. Dotted lines represent uniform confidence intervals (Montiel Olea and Plagborg-Møller, 2019).

Figure A.7: F-tests of Consecutive Coefficients Show Evidence of Retirement Bunching at 65 and 70



Notes: Plots a series of F-statistics from tests of equality of consecutive coefficients for the point estimates from (3) plotted in Figure 6 (see figure notes for more details). The gray line plots the F-statistics for the relative differences in nonparticipation in 1940 by age ($\widehat{\gamma}_{a(i)} = \widehat{\gamma}_{a-1(i)}$), while the black line plots F-statistics for the total effect (the sum of baseline and relative effects; $\widehat{\gamma}_{a(i)} + \widehat{\rho}_{a(i)} = \widehat{\gamma}_{a-1(i)} + \widehat{\rho}_{a-1(i)}$). Because age 64 is omitted, the statistics at ages 65 and 64 are simply the squares of the t-statistics at ages 65 and 63, respectively.

Figure A.8: The RRA Led to Higher Retirement Hazards at Eligible Ages

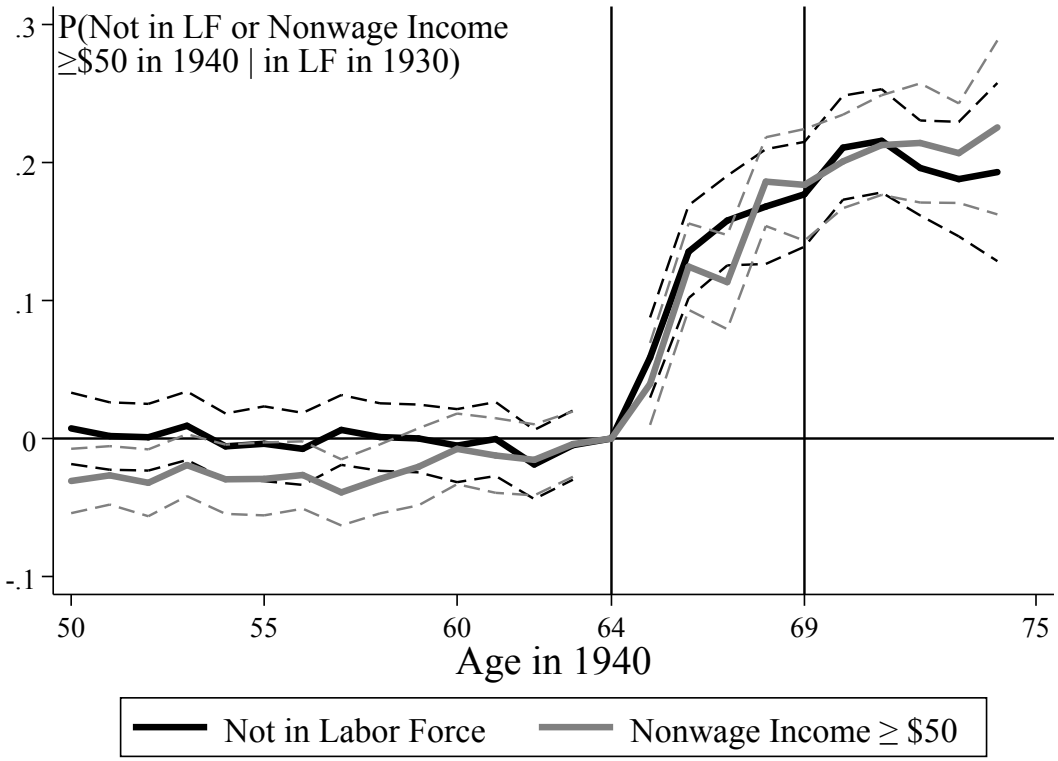


Notes: Sample is comprised of all male individuals ages 50-74 in the 1940 complete count Censuses (Ruggles et al., 2021) who were linked to the 1930 Census, and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). The sample is further restricted to those who had positive weeks of employment in 1939. Estimates are from (4) where the outcome indicates whether the individual was not in the labor force, so that estimates represent differences in the retirement hazard, or probability of retiring conditional on working last year. Estimates are reweighted using weights generated to match the population at risk of being linked (see FN 32). Dashed lines are point-wise 95 percent confidence intervals, and standard errors are clustered at the 1930 county level.

Figure A.9: Effects Using Railroad Time-Comparisons

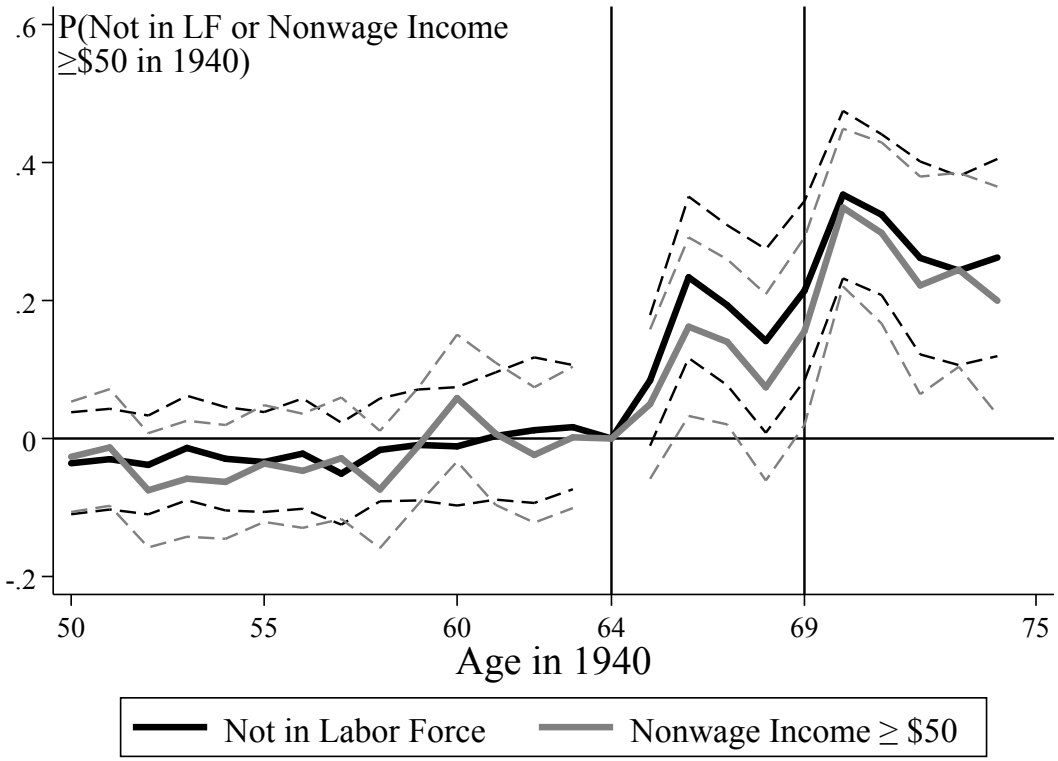
Notes: Sample is comprised of all male individuals ages 50-74 in the 1940 complete count Censuses (Ruggles et al., 2021) who were linked to the 1930 Census, and who were working on railroads (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Estimates are from a restricted version of (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 32). The black, solid line plots the differences in labor force nonparticipation between railroad in 1940 relative to 1930. Black dashed lines represent pointwise 95 percent confidence intervals, with standard errors clustered at the county level.

Figure A.10: Evidence Equating Nonparticipation to Pension Receipt



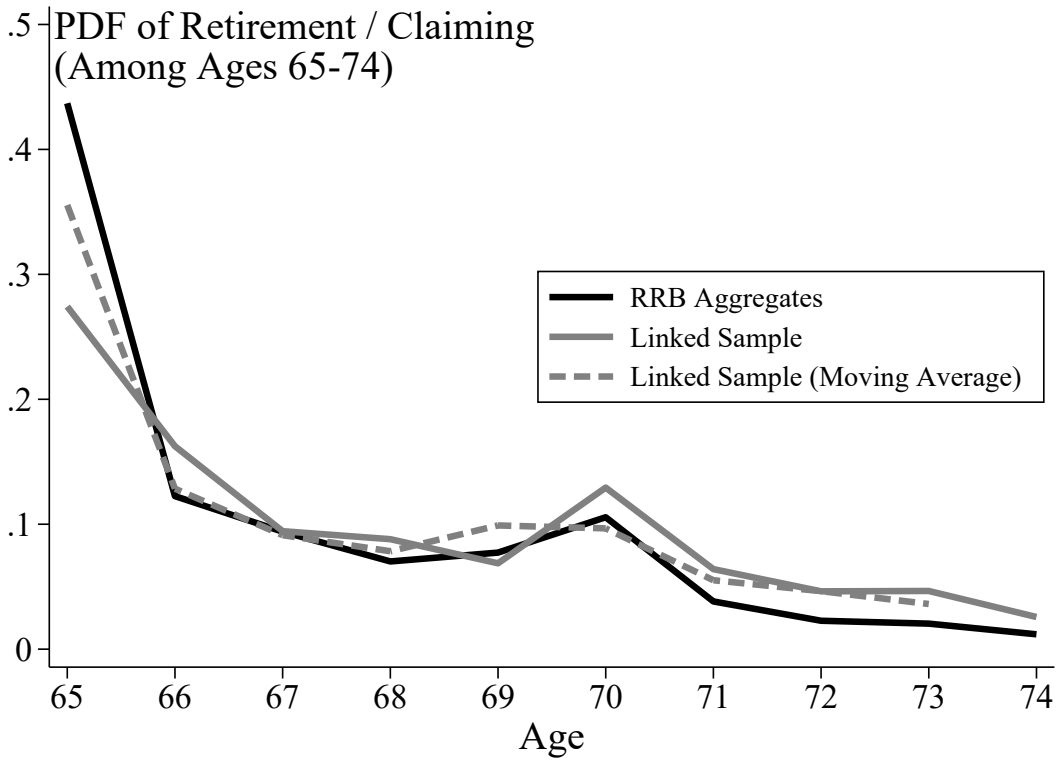
Notes: Sample is comprised of all male individuals ages 50-74 in the 1940 complete count Censuses (Ruggles et al., 2021) who were linked to the 1930 Census, and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Estimates are from (4) where the outcome indicates whether the individual was not in the labor force (black line) or an indicator for whether the individual received non-wage income in excess of $50 (nominal) in 1939 (gray line). Estimates are reweighted using weights generated to match the population at risk of being linked (see FN 32). Dashed lines are point-wise 95 percent confidence intervals, and standard errors are clustered at the 1930 county level.

Figure A.11: Evidence that Linked Data are Not Confounding Estimates: Usual Industry in 1940



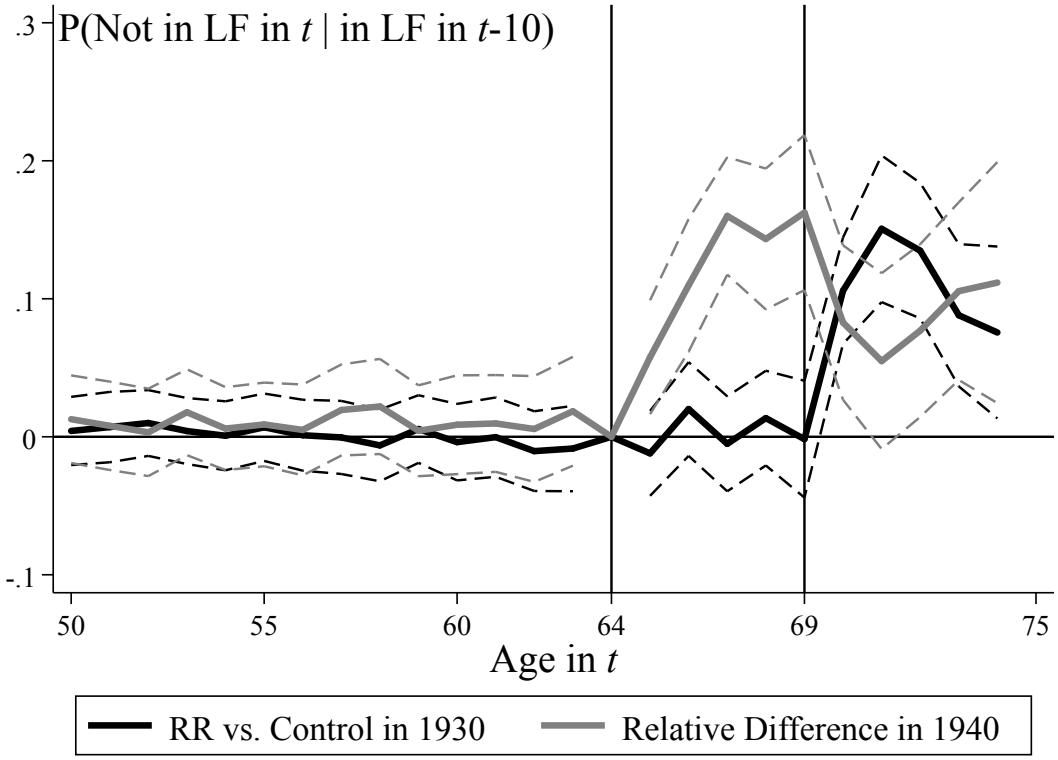
Notes: Sample is comprised of all men in the 1940 complete count Decennial Census (Ruggles et al., 2021) ages 50-74 and either working on railroads or "control industries" as defined by their "usual" occupation and industry (see Section II and Appendix B for railroad and control definitions; see Section IV for a discussion of usual occupation). Estimates are from (4) where the outcome indicates whether the individual was not in the labor force (black line) or an indicator for whether the individual received non-wage income in excess of $50 (nominal) in 1939 (gray line). Dashed lines are point-wise 95 percent confidence intervals, and standard errors are clustered at the 1930 county level.

Figure A.12: Post-RRA Density of Railroad Pension Claims are Similar to Labor Force Exit in the Census



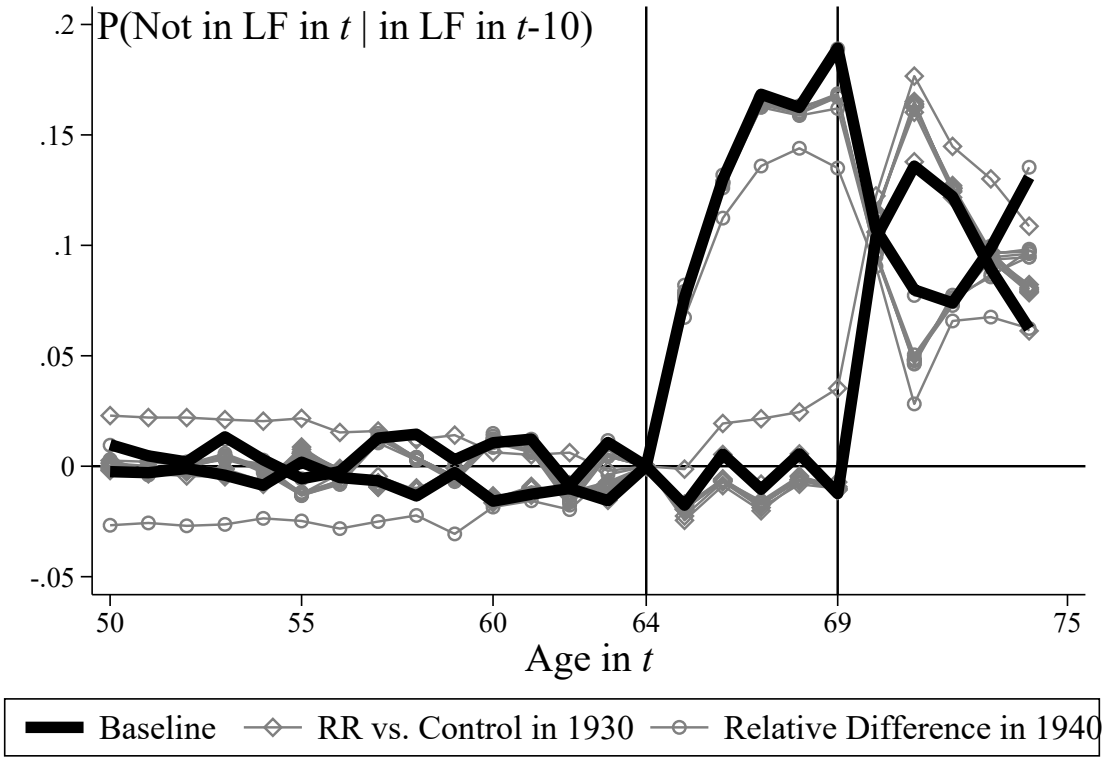
Notes: The black, solid line plots the conditional empirical probability density function of first benefit claiming age for annuitants (post-RRA claimants) who claimed at an age between 65 and 74 (inclusive) in fiscal year 1940 (between July, 1939 and June, 1940). Data come from the RRB (1941 pp. 211,212); see notes to Figure 4 for further details. The gray, solid line is the empirical probability density of the count by age reporting not being in the labor force for all railroad workers in the main analysis sample (see notes to Figure 6) ages 65-74 in 1940, further conditioned on working positive weeks and having positive wage earnings in 1939, collapsed to the age level using weights generated to match the population at risk of being linked (see FN 49). The gray, dashed line is the same as the previous series, but uses as the density an average of the density at a and $a+1$, except for the density at 65, which adds 1/2 of that at age 66.

Figure A.13: Effects are Similar Using Gainful Employment to Measure LFP



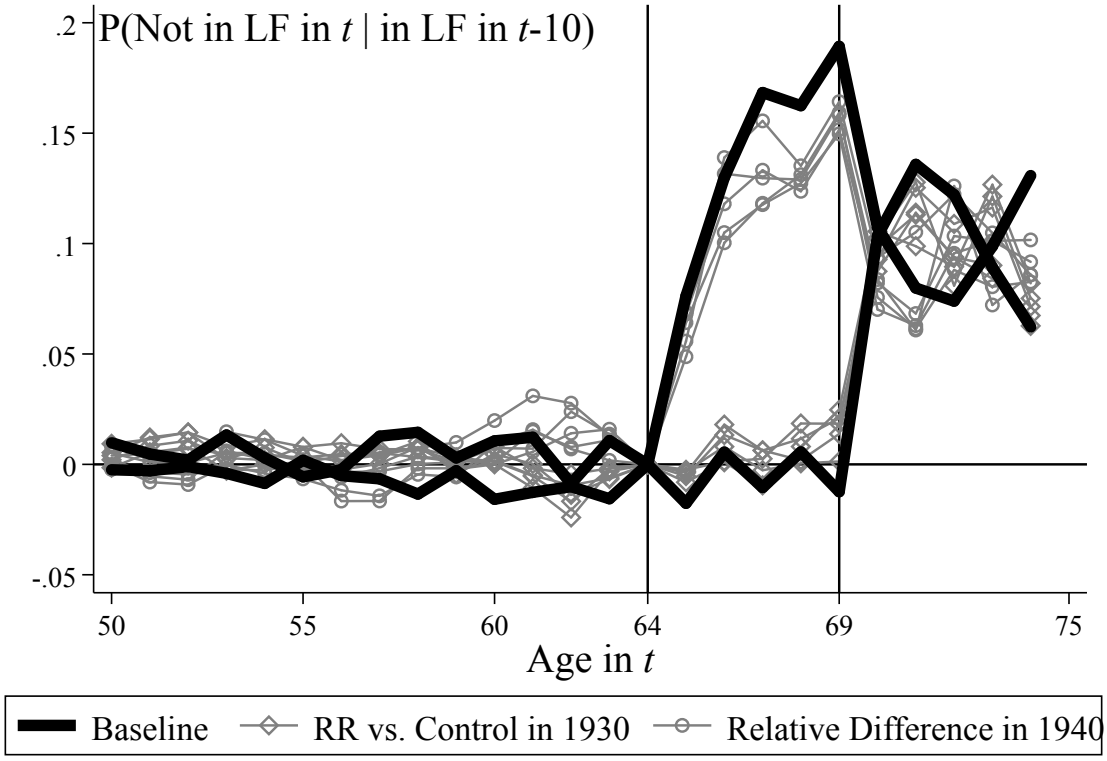
Notes: Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Estimates are from (3) where the outcome is a consistent measure of no gainful employment between 1930-1940. This corresponds to 1950 occupation codes ≥ 980, excluding 985 and 990. Estimates are reweighted using weights generated to match the population at risk of being linked (see FN 32). 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 $t-10$).

Figure A.14: Patterns of Labor Force Nonparticipation are Similar Across a Variety of Specification Checks



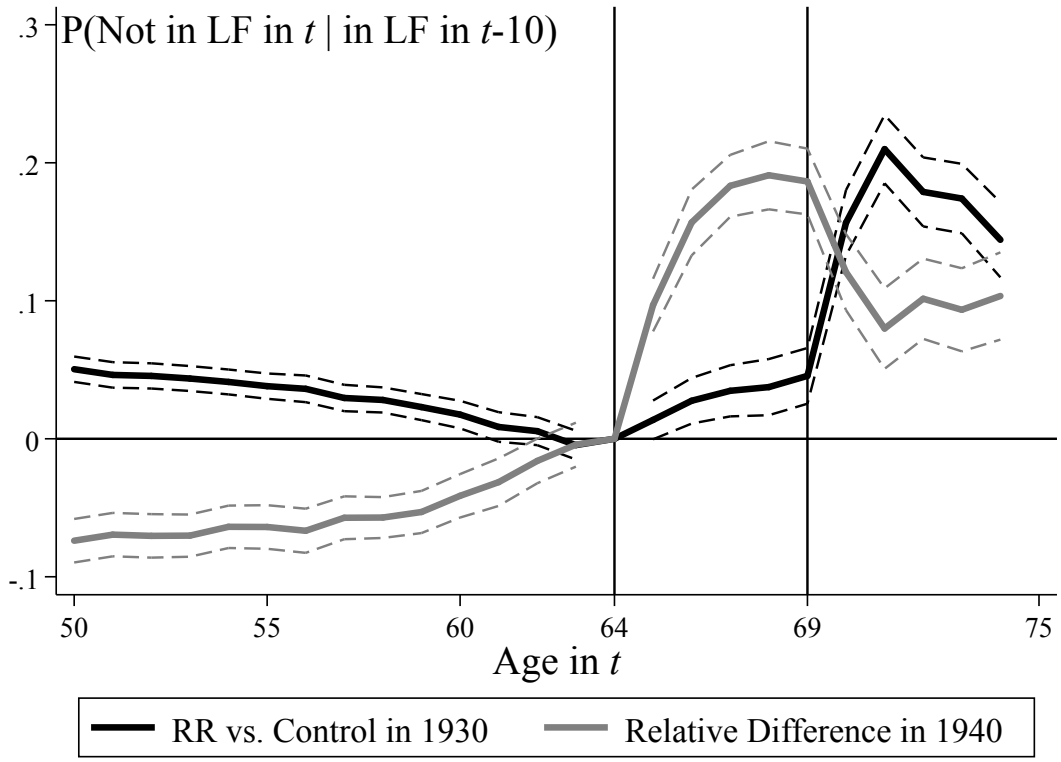
Notes: Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who were working on railroads or industries I classified as covered by pensions (in $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Estimates are from 9 variants of (3) where the outcome indicates whether the individual was not in the labor force: (1) the baseline specification without the use of weights; (2) the baseline specification plus controls for race, number of children, and marital status (all in $t-10$); (3) the baseline specification plus county-by-age fixed effects; (4) the same as (3) plus county-by-railroad status fixed effects; (5) the same as (4) plus county-by-period fixed effects; (6) the same as (5) but clustering at the state level; (7) the same as (5) but including occupation score fixed effects; (8) the same as (7) but including occupation score-by-county fixed effects instead; (9) the same as (8) but including occupation-by-county fixed effects instead. The black, solid line plots the coefficients from the preferred specification (Figure 6). The gray solid lines with triangles plot 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 solid lines with circles plot the relative differences in 1940 ($\widehat{\mu}_{a(i)}$ under age 65; $\widehat{\gamma}_{a(i)}$. Confidence intervals are omitted for expositional purposes.

Figure A.15: Patterns of Labor Force Nonparticipation are Similar For Each ABE Linkage Technique and For MLP



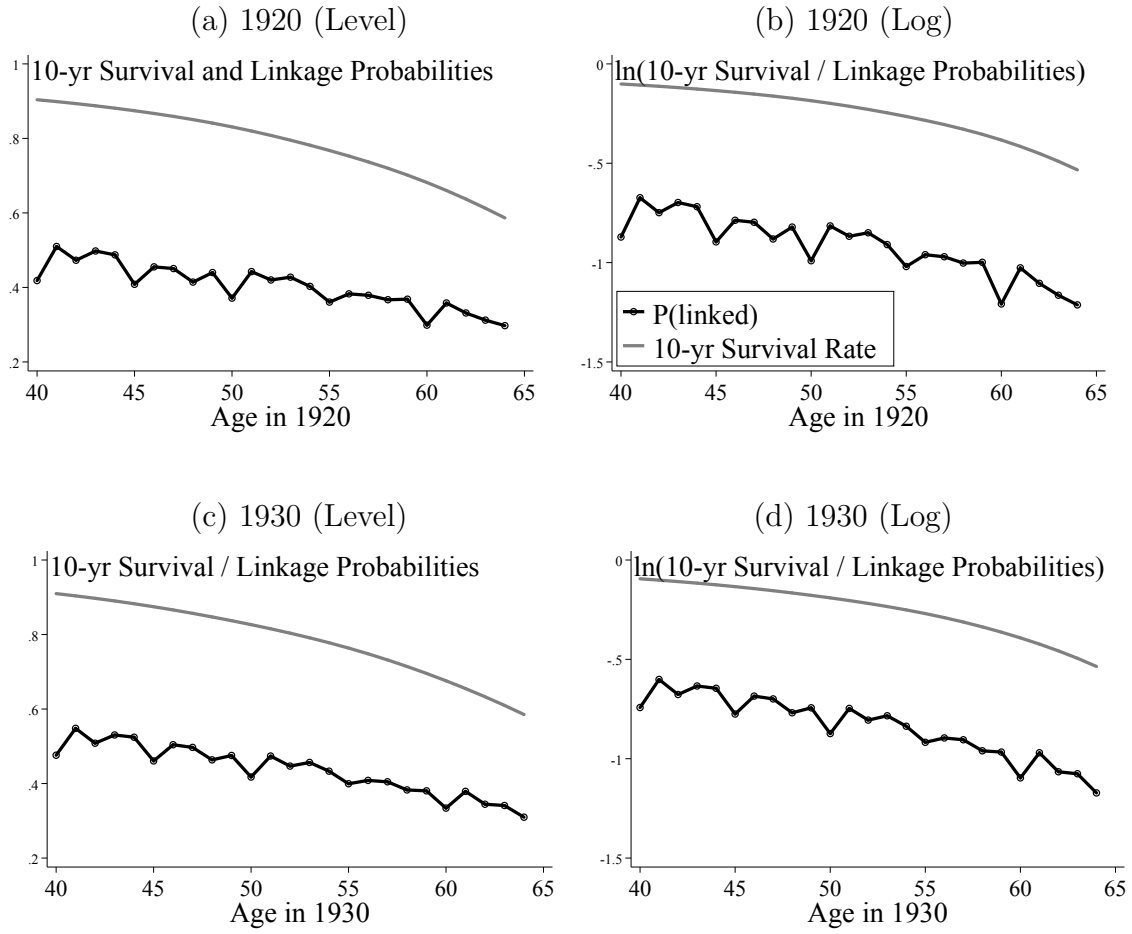
Notes: Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who were working on railroads or industries I classified as covered by pensions (in $t-10$), with the primary results using links available from (Helgertz et al., 2020) in black (Figure 6) and the gray lines using the 4 available techniques from (Abramitzky et al., 2019), as well as the intersection. Estimates are from (3) where the outcome indicates whether the individual was not in the labor force. The gray solid lines with triangles plot 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 solid lines with circles plot the relative differences in 1940 ($\widehat{\mu}_{a(i)}$ under age 65; $\widehat{\gamma}_{a(i)}$. Confidence intervals are omitted for expositional purposes.

Figure A.16: Patterns of Labor Force Nonparticipation When Comparing to All Non-Agricultural Linked Workers



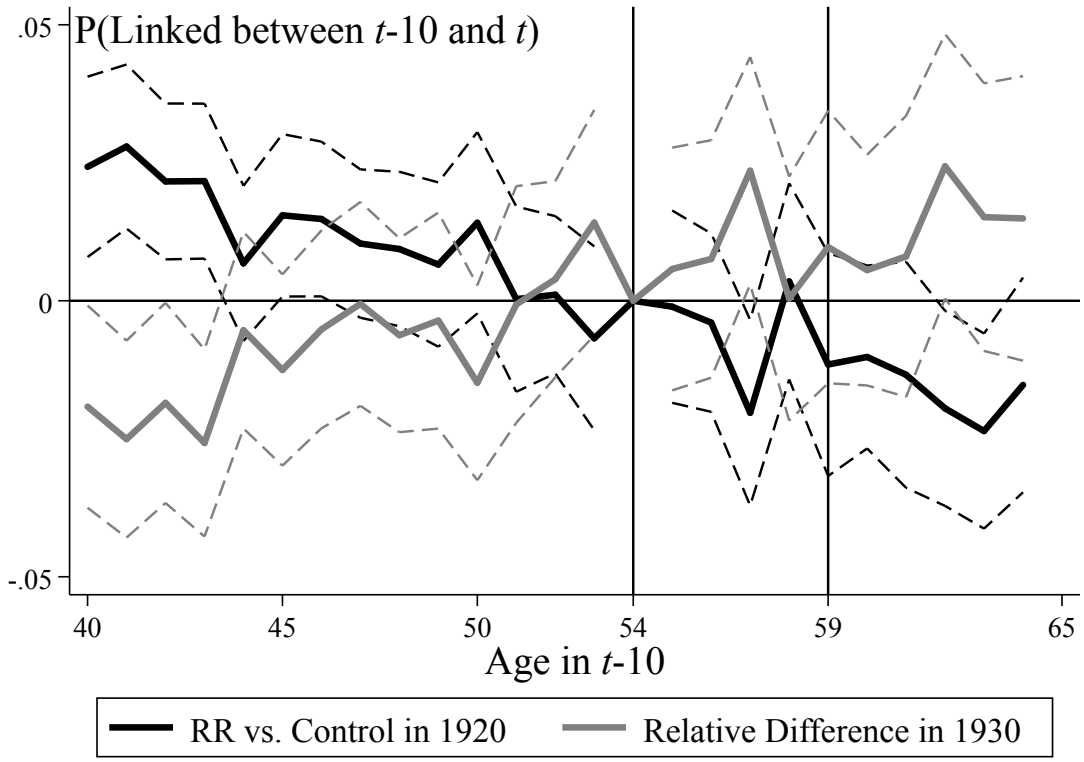
Notes: Sample is comprised of all male individuals ages 50-74 in the 1930 and 1940 (period t) complete count Censuses (Ruggles et al., 2021), who were linked to the previous Census (period $t-10$), and who working in $t-10$ excluding agricultural workers (1950 industry codes 105,116, and 126), using 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. The black, solid line plots the differences in labor force nonparticipation between railroad and all other nonagricultural workers in 1930 ($\widehat{\pi}_{a(i)}$ under age 65; $\widehat{\rho}_{a(i)}$ ages 65+) and the gray line plots the relative differences 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 for each set of coefficients.

Figure A.17: 10 Year Survival and Linkage Probabilities by Age in 1920 and 1930



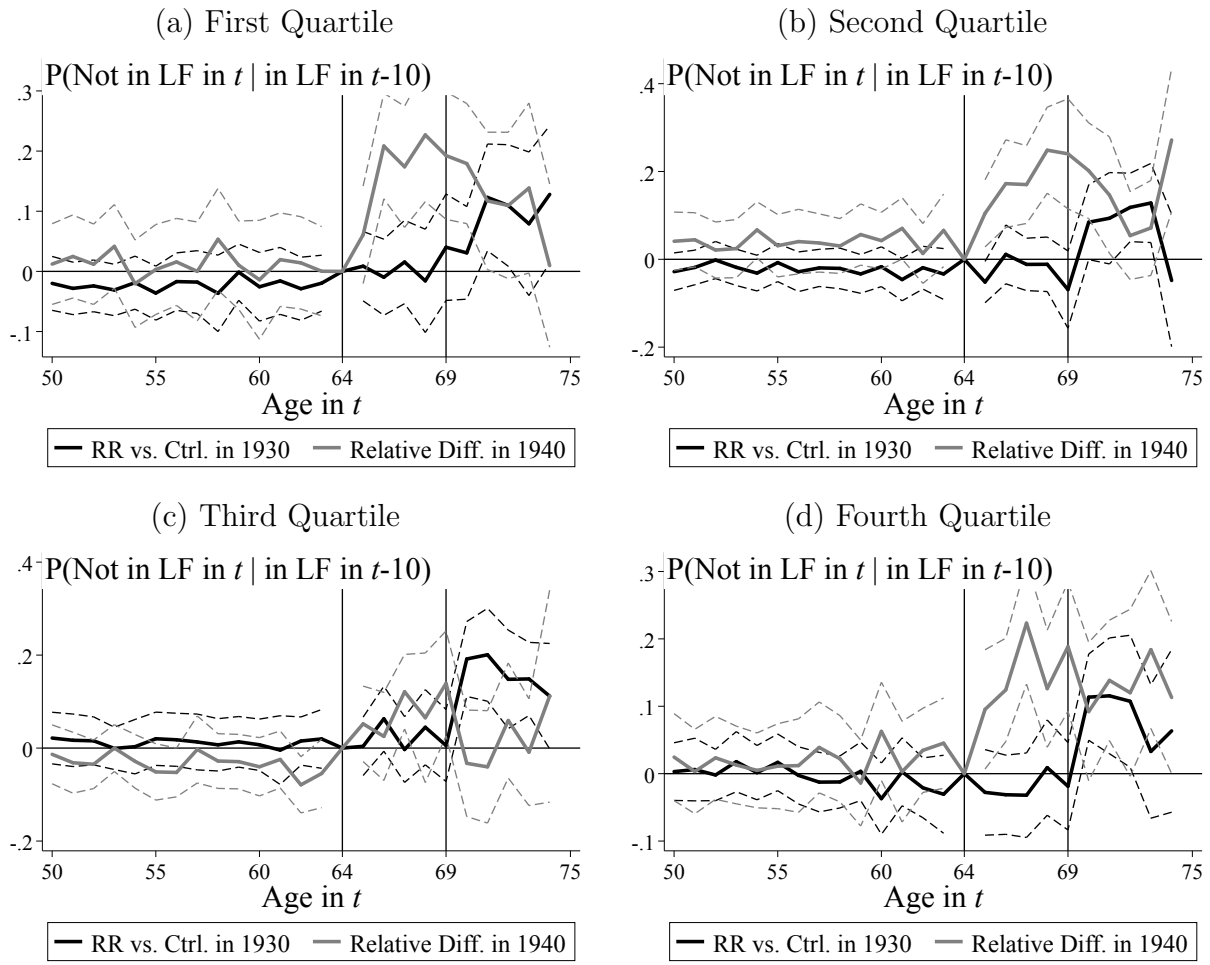
Notes: This figure plots age-specific 10-year survival probabilities (in gray) and probabilities of linkage to the next Census (in black) for men ages 40-64 in 1920 (panels (a) and (b)) and in 1930 (panels (c) and (d)). These rates are presented in levels (panels (a) and (c)) and natural logs (panels (b) and (d)). Age-specific 10-year survival probabilities are calculated from period life tables for 1920, 1930, and 1940, which give the probability of surviving to age $a+1$ conditional on age a (Bell and Miller, 2005). I linearly interpolate the 1-year survival rates by age for interceding years to calculate the age-specific probability of 10-year survival. I next calculate the unadjusted probability of linkage by age from the sample of all male individuals ages 40-64 in the 1920 and 1930 complete count Censuses (Ruggles et al., 2021) who were working on railroads or industries I classified as covered by pensions. Linkage probabilities are derived using links available from (Helgertz et al., 2020).

Figure A.18: No Differential Probabilities of Linkage for Railroad vs. Control Workers



Notes: Sample is comprised of all male individuals ages 40-64 in the 1920 and 1930 complete count Censuses (Ruggles et al., 2021) who were working on railroads or industries I classified as covered by pensions. Estimates are from a version of (3) where the outcome is contemporaneously measured and indicates being linked to the following Census year, using links available from (Helgertz et al., 2020). Ages are given for the base-year with the omitted age being 54. The black, solid line plots the differences in linkage probability between railroad and control workers in 1920 ($\widehat{\pi}_{a(i)}$ under age 55; $\widehat{\rho}_{a(i)}$ ages 55+) and the gray solid line plots the relative differences in 1930 ($\widehat{\mu}_{a(i)}$ under age 55; $\widehat{\gamma}_{a(i)}$ ages 55+). Black and gray dashed lines represent pointwise 95 percent confidence intervals, with standard errors clustered at the county level for each set of coefficients, respectively.

Figure A.19: Estimates Are Similar by Quartile of the 1930 Unemployment Rate



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). 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 32). Estimates are presented separately by quartile of the 1930 county-level unemployment rate for non-railroad male workers ages 40-64. 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 A.1: Characteristics of Private Railroad Pension Plans

							Age		Disability		
(1) Name	(2) Year Est.	(3) 1928 Emp.	(4) Emp. PDF	(5) Emp. CDF	(6) Pension Types	(7) Comp. Ret. Age	(8) Elig. Age	(9) Serv. Req.	(10) Elig. Age	(11) Serv. Req.	(12) Adj. Factor
Pennsylvania Railroad Company, The	1900	173,447	.112	.112	A,D	70	70		65	30	.01
New York Central Lines	1910	160,382	.104	.216	A,D,S	70		15		20	.01
Southern Pacific Lines	1903	63,905	.041	.257	A,D	70	65	20	61	20	.01
Baltimore & Ohio Railroad Company	1884	61,874	.04	.297	A,D		65	35	65	10	.01
Delaware, Lackawanna & Western Railroad Co., The	1902	60,984	.039	.337	A,D	70		25		25	.01
Atchison, Topeka & Santa Fe Sys.	1907	58,040	.038	.374	A,D		65	15		15	
Union Pacific System	1903	51,062	.033	.407	A,D	70		20	61	20	.01
Louisville & Nashville R.R.	1901	47,477	.031	.438	D						.01
Illinois Central Railroad Co.	1901	46,548	.03	.468	A,D	70	70	25		25	.01
Chicago and North Western Railway Company	1901	45,996	.03	.498	A,D	70		20		20	.01
Reading System	1902	44,585	.029	.527	A,D	70	70 (65)	25 (35)		30	.01
Chicago, Burlington & Quincy Railroad Company	1922	43,670	.028	.555	A,D	70	65	20		25	.01
Chesapeake & Ohio Ry.	NR	36,622	.024	.579	A,D			20			.01
Missouri Pacific Railroad Company	1917	35,903	.023	.602	A,D	70		25		25	.01
Erie R.R. System	1927	35,335	.023	.625	A,D	70					.01
Chicago, Rock Island & Pacific Railway Company	1910	35,123	.023	.648	A,D	70		20		25	.01
New York, New Haven & Hartford R.R.	1890	31,134	.02	.668	D						.01
Great Northern Railway Company	1916	27,941	.018	.686	A,D	70		20		25	.01
International-Great Northern RR	1926	27,941	.018	.704	A,D	70		25		25	.01
Norfolk and Western Railway Company	1917	27,571	.018	.722	A,D,S	70	70			20	.01
Pullman Company	1914	26,815	.017	.739	A,D	70		20		20	.01
Northern Pacific Railway Company	1922	25,841	.017	.756	A,D	70		20	O(U) 61	20(25)	.01
Atlantic Coast Line Railroad Company	1904	24,622	.016	.772	A,D	70	70	10	O(U) 61	10(20)	.01
St. Louis-San Francisco Lines	1913	22,759	.015	.787	A,D	70		15		20	.01

Notes: See end of table notes.

Table A.1: Characteristics of Private Railroad Pension Plans (Continued)

							Age		Disability		
(1) Name	(2) Year Est.	(3) 1928 Emp.	(4) Emp. PDF	(5) Emp. CDF	(6) Pension Types	(7) Comp. Ret. Age	(8) Elig. Age	(9) Serv. Req.	(10) Elig. Age	(11) Serv. Req.	(12) Adj. Factor
Boston & Maine R.R.	1901	20,635	.013	.8	D						
Lehigh Valley R.R.	1907	19,446	.013	.813	A						
Seaboard Air Line Ry.	NR	17,532	.011	.824	D						.01
Wabash Ry.	1915	17,207	.011	.835	D						.01
Central R.R. Co. of New Jersey	1903	14,600	.009	.844	A,D			30			.01
New York, Chicago & St. Louis Railroad Company (Nickel Plate Road)	1914	13,701	.009	.853	A,D	70	70	10		20	.01
NYC. & St.L.	1923	13,701	.009	.862	D						
Missouri-Kansas-Texas Lines	1918	13,676	.009	.871	D						.01
Texas and Pacific Ry.	1925	13,445	.009	.88	A,D		65	25		25	.01
Minneapolis, St. Paul & S.S. Marie Ry. Co. (Subsidiary of Canadian Pacific with separate plan)	1910	12,895	.008	.888	A,D	70	65	15		15	.01
Delaware & Hudson Company, The	1908	11,729	.008	.896	A,D		70			25	.01
Pere Marquette Ry.	1925	10,259	.007	.902	D						.01
Denver & Rio Grande Western Railroad Company	1917	10,108	.007	.909	A,D	70		20		25	.01
Gulf Coast Lines	1926	9,932	.006	.915	A,D	70		25		25	.01
Nashville, Chattanooga & St. Louis Railway Company (Subsidiary of Louisville and Nashville Railroad Company. Parent company has no formal plan.)	1914	8,570	.006	.921	D				O(U) 61	10(20)	.01
Chicago, St. Paul, Minneapolis & Omaha Ry. Company (Subsidiary of Chicago & north Western Railway Company with separate plan)	1906	8,332	.005	.926	A,D	70		20		20	.01
Central of Georgia Ry.	1917	8,260	.005	.932	A,D	70		25		25	.01
Alton R.R.	NR	7,830	.005	.937							

Notes: See end of table notes.

Table A.1: Characteristics of Private Railroad Pension Plans (Continued)

							Age		Disability		
(1) Name	(2) Year Est.	(3) 1928 Emp.	(4) Emp. PDF	(5) Emp. CDF	(6) Pension Types	(7) Comp. Ret. Age	(8) Elig. Age	(9) Serv. Req.	(10) Elig. Age	(11) Serv. Req.	(12) Adj. Factor
St. Louis Southwestern Lines	1915	7,329	.005	.941	A,D,S						
Chicago & Eastern Illinois Ry.	NR	6,794	.004	.946	S						
Elgin, Joliet & Eastern Ry.	1911	6,355	.004	.95	A,D,S	70		25		25	.01
Grand Trunk Western R.R.	1908	6,245	.004	.954	A,D	65		15		20	.01
Kansas City Southern Ry. (Inc. T & FS)	1920	5,129	.003	.957	A,D						.01
Buffalo, Rochester & Pittsburgh Railway Company	1903	5,011	.003	.96	A,D	70	65	20	60	20	.02
Minneapolis & St. Louis R.R.	NR	4,852	.003	.964							
Chicago, Indianapolis & Louisville Ry.	NR	4,302	.003	.966							
New York, Ontario & Western Ry.	1915	4,019	.003	.969	A,D			30			
Richmond, Fredericksburg & Potomac R.R.	1924	3,830	.002	.971	A,D	70				25	.01
Florida East Coast Railway Company	1916	3,662	.002	.974	A,D	70		10		10	.02
Canadian National Lines in New England	1908	3,490	.002	.976	A,D	65		15		20	.01
Colorado & Southern Railway Company (Subsidiary of C. B. & Q. with separate plan)	1922	2,942	.002	.978	A,D	70	65	20	65	25	.01
Fort Worth & Denver City Railway Company (Subsidiary of C. B. & Q. with separate plan.)	1922	2,578	.002	.98	A,D	70		20		25	.01
Central Vermont Ry. Inc.	1925	2,554	.002	.981	A,D			15			.01
Duluth, Missabe & northern Ry.	1911	2,379	.002	.983	A,D,S	70		25		25	.01
Spokane, Portland & Seattle Ry.	1926	2,297	.001	.984	A,D	70		20	65	20	.01
North Western Pacific Railroad Company	1912	2,229	.001	.986	A,D	70		20	61	20	.01
Georgia R.R.	1914	2,138	.001	.987							
Illinois Terminal R.R. System	NR	2,040	.001	.989							
Western Maryland Ry.	1916	1,994	.001	.99	A,D		65	20			.01
Bangor & Aroostook R.R.	1918	1,862	.001	.991							

Notes: See end of table notes.

Table A.1: Characteristics of Private Railroad Pension Plans (Continued)

(1)

Name	(2) Year Est.	(3) 1928 Emp.	(4) Emp. PDF	(5) Emp. CDF	(6) Pension Types	(7) Comp. Ret. Age	Age (8) Elig. Age	Age (9) Serv. Req.	Disability (10) Elig. Age	Disability (11) Serv. Req.	(12) Adj. Factor
Ann Arbor R.R.	1924	1,585	.001	.992	D						.01
Staten Island Rapid Transit Ry.	1917	1,364	.001	.993	A,D		65	35		10	.01
Bessemer & Lake Erie RR.	1911	1,364	.001	.994	A,D,S	70		25		25	.01
Lehigh & New England R.R.	NR	1,332	.001	.995							
St. Joseph & Grand Island Ry.	1921	1,175	.001	.995	A,D	70		20	61	20	.01
Canadian Pacific Ry. (Lines in VT.)	1903	924	.001	.996	A,D	65		25	merit		.01
Georgia & Florida R.R.	1924	811	.001	.997	A,D						
Duluth, Winnipeg, & Pacific Ry.	1908	687	<.001	.997	A,D	65		15		20	.01
Detroit & Toledo Shore R.R.	1922	675	<.001	.997	A,D						.01
Toledo, Peoria & Western R.R.	NR	659	<.001	.998							
Canadian Pacific Ry. (Lines in ME.)	1903	575	<.001	.998	A,D	65		25	merit		.01
Detroit & Mackinac Ry.	1910	565	<.001	>.999	A,D		65	30			.01
Lehigh & Hudson River Ry.	NR	551	<.001	>.999							
Lake Superior & Ishpeming Railroad Company	1920	497	<.001	>.999	A,D,S	70	70	25	60	25	.01
Texas Mexican Ry.	1929	478	<.001	>.999	A,D,S						
Wichita Valley Ry.	1922	341	<.001	>.999	A,D	70	65	20		25	.01
Atlanta & West Point (Incl.W.Ry.of Ala.)	1915	318	<.001	>.999							
Railway Express Agency, Inc.	1929	-	-	-	A,D,S	70		20	45	20	.01
Missouri-Illinois RR	1915	-	-	-	A,D	70		25		25	.01

Notes: Displays a digitized version of a Bureau of Railway Economics study (1934) on all private plans as of 1932 (reproduced in Railway Age, 1934 pp. 144-146). Column (2) gives the type of plan: formal (F), informal (IF) and indefinite (Ind); column (3) gives when the year the plan was established; column (4) gives the type of pension plan provided: age (A), disability (D), and service (S); column (5) gives the compulsory retirement age (if the plan had one); column (6) gives eligibility age for age-based pensions and column (7) the associated required minimum service; column (8) gives the eligibility age for disability-based pensions and column (9) the associated required minimum service; column (10) gives the eligibility age for service-based pensions and column (11) the associated required minimum service; and column (12) gives the adjustment factor $k(\overline{w}_i)$. O(U) refers to separate service requirements for over or under the provided age.

Table A.2: Linked Sample Representativeness

	Unweighted			Weighted		
	(1) Linked Sample Mean	(2) Unlinked Difference	(3) p-value	(4) Linked Sample Mean	(5) Unlinked Difference	(6) p-value
A. 1920 Only						
Railroad	.55	-.038	<.01	.51	-.0028	.034
Utilities	.067	.0018	<.01	.069	.000094	.89
Manufacturing	.39	.036	<.01	.42	.0028	.031
Age	46.3	1.6	<.01	48.3	-.49	<.01
Marital Status	.91	-.17	<.01	.72	.013	<.01
White	.96	-.085	<.01	.88	.000017	.99
Have Children	.78	-.29	<.01	.48	.0036	<.01
# of Children | Have Children	3.2	-.53	<.01	2.6	-.01	.016
Urban	.78	.027	<.01	.81	-.0034	<.01
Occupation Score	29.8	–2.6	<.01	27.6	-.36	<.01
Own House	.53	-.13	<.01	.41	-.013	<.01
B. 1930 Only						
Railroad	.5	.0041	<.01	.51	-.002	.078
Utilities	.095	-.011	<.01	.084	-.00041	.47
Manufacturing	.4	.007	<.01	.41	.0024	.028
Age	46.5	1.9	<.01	48.8	-.42	<.01
Marital Status	.92	-.17	<.01	.75	.0091	<.01
White	.96	-.076	<.01	.89	-.00047	.65
Have Children	.78	-.27	<.01	.5	.0023	.04
# of Children | Have Children	3	-.5	<.01	2.6	-.008	.029
Urban	.78	.02	<.01	.8	-.0019	.024
Occupation Score	30.1	–1.9	<.01	28.5	-.33	<.01
Own House	.62	-.12	<.01	.51	-.013	<.01
C. Full Sample						
Railroad	.52	-.013	<.01	.51	-.0023	<.01
Utilities	.083	-.0063	<.01	.078	-.00078	.075
Manufacturing	.4	.019	<.01	.41	.0031	<.01
Age	46.4	1.7	<.01	48.6	-.47	<.01
Marital Status	.92	-.17	<.01	.74	.01	<.01
White	.96	-.08	<.01	.88	-.00052	.52
Have Children	.78	-.28	<.01	.49	.002	.019
# of Children | Have Children	3.1	-.51	<.01	2.6	-.0054	.052
Urban	.78	.023	<.01	.8	-.0025	<.01
Occupation Score	30	–2.3	<.01	28.1	-.38	<.01
Own House	.58	-.13	<.01	.47	-.017	<.01

Notes: See sample notes in Table 1. Presents balance tests for a host of economic and demographic characteristics between the linked sample and population at risk of being linked, unweighted in columns (1)-(3) and weighted in columns (4)-(6) (see FN 32).

Table A.3: Balance Among Railroad and Control Industries by Year and Pension Eligibility Ages

	All Ages			Ages 50-64			Ages 65-74		
	(1) Railroad Mean	(2) Control-Railroad Difference	(3) *p*-value	(4) Railroad Mean	(5) Control-Railroad Difference	(6) *p*-value	(7) Railroad Mean	(8) Control-Railroad Difference	(9) *p*-value
A. 1920 Only									
Marital Status	.73	-.016	<.01	.71	-.017	.019	.75	-.017	<.01
White	.89	-.0023	.41	.92	-.002	.74	.87	-.0015	.62
Have Children	.48	.011	<.01	.42	.022	<.01	.5	.0054	.059
# of Children | Have Children	2.6	.16	<.01	2.2	.12	<.01	2.7	.17	<.01
Urban	.77	.082	<.01	.76	.08	<.01	.77	.083	<.01
Occupation Score	29.6	–4.1	<.01	28.9	–4	<.01	29.8	–4.1	<.01
Own House	.45	-.05	<.01	.52	-.056	<.01	.42	-.046	<.01
B. 1930 Only									
Marital Status	.77	-.0049	.064	.74	-.016	<.01	.78	-.0027	.34
White	.88	.026	<.01	.91	.018	<.01	.86	.03	<.01
Have Children	.49	.044	<.01	.41	.044	<.01	.52	.039	<.01
# of Children | Have Children	2.5	.19	<.01	2.1	.13	<.01	2.6	.19	<.01
Urban	.77	.059	<.01	.77	.057	<.01	.77	.06	<.01
Occupation Score	30.9	–4	<.01	30.9	–4.4	<.01	30.9	–3.9	<.01
Own House	.55	-.034	<.01	.62	-.042	<.01	.52	-.026	<.01
C. Full Sample									
Marital Status	.75	-.01	<.01	.73	-.018	<.01	.76	-.0092	<.01
White	.88	.013	<.01	.91	.0092	.017	.87	.016	<.01
Have Children	.48	.029	<.01	.42	.034	<.01	.51	.024	<.01
# of Children | Have Children	2.5	.18	<.01	2.1	.13	<.01	2.6	.18	<.01
Urban	.77	.069	<.01	.77	.067	<.01	.77	.07	<.01
Occupation Score	30.3	–4.1	<.01	30	–4.3	<.01	30.4	–4	<.01
Own House	.5	-.042	<.01	.58	-.052	<.01	.47	-.035	<.01

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). Presents balance tests for a host of economic and demographic characteristics between railroad workers and control workers in 1920 (Panel A), 1930 (Panel B) and the full sample (Panel C). Broken down by age group, columns (1), (4), and (7) presents the (weighted) means among railroad workers, columns (2), (5), and (8) the weighted differences between railroad and control workers, and columns (3), (6), and (9) associated p-values. Weights are generated to match the population at risk of being linked (see FN 32.

Table A.4: Balance Between Usual Occupations and 1930-1940 Linked Sample

	Not Usually in Same Industry	Difference Among Those Usually in Same Industry	p-value
	(1)	(2)	(3)
A. Railroad Workers			
Age	59.27	.915	<.01
Marital Status	.789	-.0211	.0419
White	.901	-.0249	<.01
Have Children	.482	.0088	.244
# of Children | Have Children	2.45	.0062	.755
Urban	.806	-.0351	<.01
Occupation Score	32.59	–1.78	<.01
Own House	.549	-.001	.887
B. Control Workers			
Age	58.29	1.16	<.01
Marital Status	.762	.0014	.905
White	.899	.0033	.65
Have Children	.543	-.0086	.389
# of Children | Have Children	2.74	-.0895	<.01
Urban	.853	-.0216	<.01
Occupation Score	27.02	-.188	.172
Own House	.518	-.0042	.616
C. All Workers			
Age	58.87	.975	<.01
Marital Status	.776	-.0095	.19
White	.899	-.0099	.0429
Have Children	.504	.007	.227
# of Children | Have Children	2.57	-.0185	.294
Urban	.824	-.0249	<.01
Occupation Score	30.21	–1.32	<.01
Own House	.535	-.0038	.468

Notes: Sample is comprised of all male individuals ages 50-74 in the 1940 complete count Censuses (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 $t-10$), using links available from (Helgertz et al., 2020) (See Section II and Appendix B for more details). Presents balance tests for a host of economic and demographic characteristics between workers that did and did not report usual industry in 1940, reweighted using weights generated to match the population at risk of being linked (see FN 32).

Table A.5: Effects are Robust to Leaving out Control Industries

Omitted Industry Name	(1) Omitted Industry Code	(2) Estimate (Ages 65–69)	(3) Estimate (Ages 70–74)	(4) N
baseline (none omitted)		.131 (.01)	.088 (.015)	956,391
blast furnaces, steel works, and rolling mills	336	.118 (.01)	.084 (.017)	842,242
other primary iron and steel industries	337	.131 (.01)	.077 (.016)	899,819
fabricated nonferrous metal products	347	.13 (.01)	.088 (.016)	950,971
not specified metal industries	348	.132 (.01)	.091 (.016)	942,953
agricultural machinery and tractors	356	.131 (.01)	.087 (.016)	950,053
electrical machinery, equipment and supplies	367	.131 (.01)	.089 (.016)	927,724
aircraft and parts	377	.131 (.01)	.088 (.016)	955,877
ship and boat building and repairing	378	.136 (.011)	.095 (.016)	917,386
railroad and misc transportation equipment	379	.133 (.01)	.084 (.016)	935,740
meat products	406	.132 (.01)	.092 (.016)	933,449
dairy products	407	.127 (.01)	.087 (.016)	945,337
canning and preserving fruits, vegetables, and seafoods	408	.132 (.01)	.091 (.016)	951,129
misc food preparations and kindred products	419	.131 (.01)	.085 (.016)	946,181
not specified food industries	426	.131 (.01)	.088 (.015)	955,724
dyeing and finishing textiles, except knit goods	437	.133 (.01)	.09 (.016)	948,556
misc textile mill products	446	.131 (.01)	.087 (.015)	953,527
misc fabricated textile products	449	.131 (.01)	.087 (.016)	955,504
petroleum refining	476	.137 (.01)	.09 (.016)	934,511
telephone	578	.139 (.01)	.088 (.016)	929,944
telegraph	579	.131 (.01)	.088 (.016)	948,397
electric light and power	586	.128 (.01)	.086 (.015)	928,906
gas and steam supply systems	587	.132 (.01)	.088 (.016)	940,654
electric-gas utilities	588	.131 (.01)	.088 (.015)	955,850

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). Estimates are from are summary version of (3) that imposes 0 for all coefficients on age dummies less than 65 and collapses dummies 65-69 and 70-74 into two indicators.The outcome indicates not in the labor force. Estimates are reweighted using weights generated to match the population at risk of being linked (see FN 32). Every row drops an individual industry in the main control sample.

Table A.6: Effects are Robust to Leaving out Railroad Occupations

Omitted Occupation Name	(1) Omitted Occupation Code	(2) Estimate (Ages 65–69)	(3) Estimate (Ages 70–74)	(4) N
baseline (none omitted)		.131 (.01)	.088 (.015)	956,391
accountants and auditors	0	.132 (.01)	.088 (.016)	952,462
civil-engineers	43	.132 (.01)	.088 (.015)	954,469
conductors, railroad	203	.134 (.01)	.091 (.016)	914,178
managers, officials, and proprietors (nec)	290	.132 (.01)	.089 (.016)	943,942
baggagemen, transportation	304	.131 (.01)	.087 (.016)	954,038
express messengers and railway mail clerks	325	.131 (.01)	.09 (.016)	949,599
telegraph operators	365	.131 (.01)	.089 (.016)	950,080
ticket, station, and express agents	380	.134 (.01)	.088 (.016)	944,176
clerical and kindred workers (n.e.c.)	390	.135 (.01)	.089 (.016)	931,548
blacksmiths	501	.132 (.01)	.089 (.015)	951,633
boilermakers	503	.131 (.01)	.087 (.016)	949,639
carpenters	510	.13 (.01)	.086 (.016)	942,584
electricians	515	.131 (.01)	.088 (.015)	954,089
foremen (nec)	523	.126 (.01)	.089 (.016)	923,027
inspectors (nec)	533	.13 (.01)	.087 (.015)	941,359
locomotive engineers	541	.138 (.01)	.096 (.016)	904,027
locomotive firemen	542	.131 (.01)	.088 (.016)	947,000
machinists	544	.131 (.01)	.087 (.015)	932,370
railroad and car shop-mechanics and repairmen	553	.129 (.01)	.088 (.016)	938,735
painters, construction and maintenance	564	.131 (.01)	.088 (.016)	953,531
plumbers and pipe fitters	574	.131 (.01)	.087 (.015)	953,659
stationary engineers	583	.131 (.01)	.088 (.015)	954,716
brakemen, railroad	624	.131 (.01)	.088 (.016)	932,859
motormen, street, subway, and elevated railway	661	.133 (.01)	.089 (.015)	955,043
switchmen, railroad	681	.129 (.01)	.086 (.015)	938,034
operative and kindred workers (nec)	690	.131 (.01)	.087 (.016)	944,903
guards, watchmen, and doorkeepers	763	.133 (.01)	.09 (.016)	951,896
policemen and detectives	773	.132 (.01)	.088 (.016)	955,357
porters	780	.133 (.01)	.088 (.016)	952,643
watchmen (crossing) and bridge tenders	785	.131 (.01)	.088 (.016)	955,060
laborers (nec)	970	.125 (.01)	.06 (.016)	870,113
not yet classified	979	.13 (.01)	.09 (.016)	910,498
all other occupations		.133 (.01)	.089 (.015)	943,804

Notes: See notes in Table A.5 with the exception that each row now removes one of the 23 railroad occupations with greater than 1,000 workers (summed across 1920 and 1930) and the last row leaving out the remaining workers ("all other occupations").

Table A.7: Comparing Railroad Workers in 1930 by Whether They were in the Railroad Industry in 1910

	Not RR Worker in 1910			RR Worker in 1910		
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Economic						
1939 Monthly Wages: Full Time Only (w_i)	142.6	74.6	1,393	188.8	72.1	1,052
1939 Monthly Wages: Full Time and Imputed (w_i)	135.2	74	2,086	190.7	76.2	1,589
Estimated Average Monthly Wage ($\overline{w}_i$)	123	66	2,086	172.5	68	1,589
Occupation Score	29.9	8.3	1,853	36.5	8.4	1,437
Own House	.68	.46	2,080	.75	.43	1,578
ln(House Value) | Own House	8.2	.86	1,335	8.4	.74	1,125
Demographic						
# of Children | Have Children	2.7	1.7	1,421	2.4	1.4	1,028
Have Children	.7	.47	2,086	.66	.48	1,589
White	.93	.17	2,086	.95	.15	1,589
Marital Status	.93	.26	2,086	.93	.26	1,589

Notes: Sample is comprised of all male individuals 65-70 in the 1940 complete count Census (Ruggles et al., 2021) who were linked to the 1930 Census and who were working on railroads 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, and who are successfully linked to the 1910 Census using the "exact-conservative" method provided by Abramitzky et al. (2019). Columns (1)-(3) provide statistics for individuals not reporting working for railroads in 1910, while columns (4)-(6) present the same for those who did. Construction of wage variables is described in detail in Appendix C.III. Demographic variables are from 1930. Averages are reweighted using weights generated by the procedure described in FN 49).

Table A.8: Hazard Elasticity Robustness (at Age 65)

	(1) Elasticity	(2) p-value	(3) N
Baseline	.181	$< .01$	461
	(.053)		
No Weights	.188	$< .01$	461
	(.054)		
ABE Exact Standard	.133	$< .01$	598
	(.04)		
ABE NYSIIS Standard	.166	.015	448
	(.062)		
ABE NYSIIS Conservative	.16	.076	315
	(.086)		
ABE Intersection	.157	.083	315
	(.086)		
Any 1910 County	.135	.013	683
	(.049)		
Using w_i	.193	$< .01$	534
	(.064)		
$\overline{w}_i \geq \$100$	.181	$< .01$	613
	(.04)		
$\overline{w}_i \geq \$125$	.185	$< .01$	471
	(.053)		
$\overline{w}_i \in [\$125, \$275]$	.176	$< .01$	446
	(.053)		
$\overline{w}_i \in [\$100, \$300]$	.179	$< .01$	603
	(.04)		

Notes: See notes to Table 3 for sample construction. Estimates are from (5) for individuals age 65 in 1940, where the outcome indicates whether the individual was not in the labor force, with the first column denoting the robustness check. The second row provides the estimates without weights, the next four rows use the other 3 linking algorithms provided by Abramitzky et al. (2019) as well as the intersection of the four algorithms, the next row does not exclude to those railroad workers in the same county in 1910 and 1930, the next row only restricts to average estimated wages above \$100, above \$125, between \$125 and \$275, and between \$100 and \$300.

B Data Appendix

B.I Matching 1928 Railroad Employment to Pension Plans

To order pensions by employment size, I use information from the Interstate Commerce Commission (ICC) Annual Reports (ICC, 1928) listed on pages CIII, 18, 19, 36, 37, 54, 55, 72, 73, 90, 91, 108, 109, 126, 127, 144, 145, 162, and 163. These contain average employment for each class 1 railroad in 1928. Most of the pensions are listed for specific firms and are matched to pension plans exactly, but some are for "systems" (which are typically a subsuming category in the ICC report). Usually the system is comprised of a primary parent company and subsidiaries. For example, the New York Central Lines includes the New York Central R.R. Co., Michigan Central R.R. Co., Pittsburgh & Lake Erie R.R. Co., Cincinnati Northern R.R. Co., Cleveland, Cincinnati, Chicago & St. Louis Ry. Co., and Evansville, Indianapolis, & Terre Haunte Ry. Co. Note that employment of the Pullman Co. is for the end of the 1928 year, employment on the Railroad Express Agency appears to be unavailable, and the Missouri-Illinois railroad was a subsidiary of the Missouri Pacific but there does not appear to be s separate employment level provided for it in the ICC reports (although it is reported as a separate pension). Hence, employment is omitted for these two companies.

B.II Constructing Recipiency and Payment Series'

Annual series' of railroad pension recipiency and expenditure (or payments) come from the following publications:

- Source: ICC (1920-1945)
  Contains: Annual expenditure by railroads on pensions from 1920-1944
- Source: Latimer (1932)
  Contains: Annual private railroad pension recipiency from 1910-1929
- Source: U.S. Congress (1934)
  Contains: Private railroad pension recipiency and expenditure in 1925 and 1931
- Source: Carter et al. (2006), Series Bf746-761
  Contains: Railroad retirement pension recipiency and benefits paid from 1938-1946

B.III Railroad and Control Industry Classifications

I use 1950 industry and occupation codes to classify two groups of workers: those who were likely covered by the RRA of 1937 and those who were working in non-railroad industries that had broad private pension coverage.

Workers Covered by the RRA

i. Coverage Rules

Covered workers were those working for employers "constituting the national system of railroad transportation" (RRB, 1938, p. 134). These companies fell into the following groups: Railroads and switching and terminal companies, express and Pullman companies, and electric railways engaged in interstate commerce (Silverman and Senturia, 1939).

ii. Classification of Covered Industries in the Census

I use the legislation in conjunction with comparisons between RRB information on the number of workers with credited earnings under the RRA in 1940 and employment totals in the 1940 complete count census. I base classifications predominantly using industry codes, with use of occupation codes as a secondary method, which ends up contributing a relatively insignificant share of the total number of included workers.

Table B.1 reproduces a table from (RRB, 1941 p. 162) listing the number of employees with credited earnings in 1940 by class of employer. I proceed by examining the primary industry codes (all for the 1950 harmonized set of codes) for railroads and discussing which classes each should cover. Note that railroad employment in the 1940 Census should be somewhat lower than the number with credited earnings in 1940, since the Census measures spot employment (reference period is March 24-30, 1940) whereas credited earnings applies to all who worked in covered employment at some point in 1940. The latter should be higher to the extent that temporary workers gained some coverage during the year but were working in a different industry at the time of the Census.

a. Industry Code 506: Railroads and railway express service

Industry code 506 is the primary industry code for railroad workers. Employment in the 1940 complete count Census in this industry was 1,296,041. The industry code is likely to include the following groups in Table B.1: Class I Railroads (1,422,500), Railroads other than Class I (25,400), Express Companies (78,000), and Sleeping-car companies (25,500). These total 1,551,400, so using just this industry would suggest 1,296,041/1,551,400 or 83.5 percent of these workers. Some of the other groups may be included in this total; the share accounted for in the Census among all covered workers is 1,296,041/1,670,900 or 77.6 percent.

b. Industry Code 516: Street railways and bus lines

Identifying who was covered within the class "electric railroads" involved "special additional considerations", because they needed to be involved in interstate commerce to qualify for coverage (RRB 1938, p. 139). Table B.1 shows that covered employment in this class was 17,500, or only 10.5 percent of 1940 total employment in the relevant industry code 516 (167,029). Given the low probability of coverage in this industry, I don't include any workers in this industry in my definition of coverage.

c. Additional occupation codes

There are 6 occupation codes that stand out as being covered, even if employees were not within the primary industry code 506. These are 203 (Conductors, railroad); 553 (Mechanics and repairmen, railroad and car shop); 624 (Brakemen, railroad); 681 (Switchmen, railroad); and 325 (Express messengers and railway). I also include these occupations for workers not in the electric railway industry (516), since they may account for some of the smaller groups still not matched in Table B.1. This adds another 35,998 workers, or just 2.8 percent of the number of workers in industry 506.

To summarize, the total number of workers I classify as covered by the RRA is 1,332,039, the total number of workers with RRB credited earnings in 1940 was 1,670,190, so that 1940 Census employment comprises roughly 79 percent of total RRB credited employment in 1940, a reasonable approximation.

Control Industries

For classifying "control industries" with broad pension coverage, I compare estimates of 1929 pension coverage from Latimer (1932) to employment totals in the 1930 complete count Census by industry. I then use these codes when including workers in the control group in either linked sample base year (1920 and 1930).

i. Classification Procedure

For classifying control industries, I focus on minimizing classifying workers falsely as covered by pensions when they were not rather than including as many covered workers as possible. Making the first mistake will attenuate estimated effects of the RRA, whereas missing covered workers will result in lower powered results. I show results are not sensitive to broadening or restricting the set of control industries in Section IV.

Table B.2 lists 1929 employment by industry type among firms that had pensions, reproduced from Latimer (1932). Note that, while this source constitutes the most comprehensive survey of the period, it is not universal. I compare these counts to total employment in the 1930 Census by industry. As with railroad employment, there are two reasons to expect that totals should not be identical in addition to the survey being non-exhaustive. First, private pension coverage was not nearly as universal in any industry as it was in railroads, which would indicate higher employment in the Census. Second, the Depression may have led to lower employment by the 1930 Census, which would indicate higher employment in Latimer (1932).

I follow the following procedure to choose which industry codes to include: I match the pension definitions in Table B.2 to comparable industries in the 1930 Census. I then aggregate employment in Census industries to the finest group available in Table B.2 and compare the ratio of Census workers to the pension-covered employment total. The latter is usually, but not always, higher. I choose a tolerance for the ratio of at most 3 (or greater than 1/3), and keep those industries satisfying this criteria. Table B.3 shows that this leaves utility industries and a subset of manufacturing.

ii. Summarizing included industries as "controls"

a. Utilities

This procedure matches public utilities well. Table B.2 shows that public utility pensions covered 696,975 workers. Table B.3 lists the 5 census industries which comprise the 2 pension sub-industries in public utilities. Total employment in the Census among these 5 industries is 689,151,versus 696,975 in total pension employment (a 98.9 percent match).

b. Manufacturing

Table B.3 shows that this procedure keeps a large subset of the manufacturing industries with pension coverage listed in Table B.2. The lowest ratio is for the petroleum industry, for which census employment is only 57 percent of covered employment. While it is unclear which industry these other covered workers are in, it is reasonable to expect that the majority of those in the census petroleum industry (code 476) were covered. The highest ratio is in food products, with around 277 percent of officially covered workers being counted in the Census.

c. Share of Pensioned workers

From Table B.2 there were an estimated total of 2,172,790 non-railroad pension-covered employees

in 1929 among those firms reporting employment. Comparing this with total employment in those industries kept in Table B.3, or 1,775,191, implies that my control group plausibly represents over 81 percent of all workers covered by private pensions.

Table B.1: RRA Credited Employment Totals in 1940 by Class of Employer

Class of Employer	Employees with Credited Earnings in 1940 (1,000s)
Class I Railroads	1,422.5
Lessor companies	<.050
Switching and terminal companies	71.1
Express companies	78
Sleeping-car companies	25.5
Electric railroads	17.5
Car loan companies	13.6
Miscellaneous companies	5.6
Railroad associations	5.1
Railway labor organizations	6.6
Total	1,670.9

Notes: Lists total 1940 credited employment under the Railroad Retirement Act by class of employer. Data come from RRB (1941, p. 162).

Table B.2: 1929 Industrial Pension-Covered Employment

Industry	Employment Among Firms Sampled
Manufacturing	
Food products	98,951
Textiles and their products	29,628
Iron and steel and their products	392,054
Wood products	1,583
Leather and its manufacturers	2,599
Rubber products	68,745
Paper and printing	17,981
Chemicals and allied products	
Explosives	22,144
Paints and varnishes	12,252
Petroleum	211,470
Other chemicals	20,109
Stone, clay and glass	3,504
Metals and metal products other than iron and steel	
Silverware	9,617
Other metals	54,789
Silverware and Other metals	64,406
Machinary not including transportation equipment	
Agricultural implements	57,359
Electrical machinery, appartus and supplies	165,736
Other machinery	26,004
Musical instruments and phonographs	9,280
transportation equipment, air, land, and water	58,612
Other manufacturing	20,800
Total manufacturing	1,283,217
Banking	35,791
Insurance	83,865
Railroads	1,572,628
Public Utilities	
Electric railways, light, heat and power	253,782
Cables, telephones and telegraphs	443,193
Mining	35,672
Merchandising	31,855
Transportation and Storage	3,377
Miscellaneous	2,038
Grand total, all industries	3,809,824

Notes: Lists 1929 employment by industry type among firms that had pensions. Data are from Latimer (1932) p. 47, which constitutes the most comprehensive survey of the period (but is not exhaustive). See Latimer (1932) for further details.

Table B.3: Control Industries Included in Primary Analysis Sample

(1) Pension Industry	(2) Pension Sub-Industry	(3) 1929 Covered Emp.	(4) Census Industry Name	(5) Census Industry Code	(6) 1930 Census Emp.	(7) 1930 Census Emp. (By Pens. Sub-Ind.)	(8) (7)/(3)	(9) Ratio Bins
Manufacturing	Petroleum	211,470	Petroleum refining	476	121,270	121,270	.57	(0,1)
	Silverware and Other metals	64,406	Fabricated nonferrous metal products	347	25,371	56,902	.88	(0,1)
			Not specified metal industries	348	31,531			
	Agricultural implements	57,359	Agricultural machinery and tractors	356	31,253	31,253	.54	(0,1)
	Textiles and their products	29,628	Dyeing and finishing textiles, except knit goods	437	30,890	58,452	1.97	[1,2)
			Miscellaneous textile mill products	446	17,199			
			Miscellaneous fabricated textile products	449	10,363			
	Iron and steel and their products	392,054	Blast furnaces, steel works, and rolling mills	336	502,252	706,936	1.8	[1,2)
			Other primary iron and steel industries	337	204,684			
	Electrical machinery, appartus and supplies	165,736	Electrical machinery, equipment, and supplies	367	218,388	218,388	1.32	[1,2)
	transportation equipment, air, land, and water	58,612	Aircraft and parts	377	5,750	115,957	1.98	[1,2)
			Ship and boat building and repairing	378	75,372			
			Railroad and miscellaneous transportation equipment	379	34,835			
	Food products	98,951	Meat products	406	112,331	273,678	2.77	[2,3)
			Dairy products	407	57,699			
			Canning and preserving fruits, vegetables, and seafoods	408	41,836			
			Miscellaneous food preparations and kindred products	419	55,927			
			Not specified food industries	426	5,885			
Utilities	Cables, telephones and telegraphs	443,193	Telephone	578	363,132	432,468	.98	(0,1)
			Telegraph	579	69,336			
	Electric railways, light, heat and power	253,782	Electric light and power	586	183,842	256,683	1.01	[1,2)
			Gas and steam supply systems	587	68,839			
			Electric-gas utilities	588	4,002			

Notes: Lists pension industries and sub-industries from Latimer (1932) (see notes to Table B.2), the relevant census industry name(s) and 1950 industry code(s), employment totals for these industries in the 1930 Census, and the ratio of census employment to 1929 covered employment, for those industries satisfying the criteria that this ratio be less than 3.

C Pension Rules, Expected Retirement Response, and Constructing Average Wages

C.I Details of Railroad Retirement Benefits

This section describes the details of railroad retirement benefits under the revised 1937 Act – the rules that would apply to retirees examined in 1940. This section draws heavily from RRB (1937) Section III.A.

- Eligibility

Appendix B describes which types of employers and their workers were covered under the RRAs. To receive credit for work prior to the RRA in determination of pension benefits, workers needed to be working for a covered employer (or have an "employment relation") on August 29th, 1935.[1]

- Retirement Ages

The RRA set the normal retirement age at 65, and mandated compulsory retirement at age 70. It also allowed for early retirement at age 60 if 30 years of service had been accrued, with annual benefits reduced by 1/180 for each month before age 65 that was claimed, or by 6.7 percent per year. I discuss early retirement – which does not appear to have been taken up by many (Figure 4 panel (a))– more in Appendix C.II below.

- Service

The RRA of 1937 set no limits on minimum service for eligibility (although less credited service meant smaller monthly benefits). The RRA set the maximum creditable service years at 30. However, the level of service years was fixed once a individual attained age 65; in other words, their benefit would not be increased for additional work due to service, even had they not achieved 30 years by that point. All service rendered before January 1st, 1937 was considered "prior service" with average wages between 1924-1931 applied to those credits.

- Average Wages

The Railroad Retirement Board exerted tremendous effort to retrieve records of prior wages and service years for workers from each railroad. They recognized that Unions had allowed railroads to cut wages by 10 percent beginning in 1932 with only "promises to provide new jobs and preserve old ones [to unions]" (Graebner, 1980 p. 156). As a response, the RRA stipulated that average monthly compensation earned during the period 1924-1931 be used in determining benefits for all credited months before the RRA, whereas the wages earned during RRA covered employment (January 1st, 1937 and after) were taken as applicable to only to that service. Thus, for workers who's retirement behavior is studied in this paper, the great majority of their average wages came from the period 1924-1931. It is important to note that, once an individual reached age 65, their wage for subsequent work would only enter into their average wages if it would increase the average.

[1] This was mostly intended so prevent workers who had left the railroad industry for a lengthy period of time from returning and gaining credit for previous work (Schreiber, 1978), and so is not applicable to most of the individuals studied in this paper, who were working for railroads in 1930 (and in the elasticity analysis in 1910).

- Benefit Formula
Most of the important rules governing how benefits were computed are described in Section I.B. The RRA stipulated a maximum benefit of \$120. It also included a rather complex formula governing minimum benefits for individuals 65 plus who had at least 20 years of service.[2]

- Earnings Test
The "earnings test", or reduction in benefits from earning a wage, was quite severe under the RRA. Annuities would be forfeited for months in which individuals performed "compensated service to any person or company whether or not an employer under the [RRA]" (RRB, 1937 p. 18). While restrictive, this type of earnings test was used on most private railroad pensions (see RRB, 1937 p. 45). It is also quite similar to the earnings tests of the early Social Security program or elderly public assistance.[3] This is consistent with the models of pensions and retirement described below, which presumes labor-force exit (zero wages) as a condition for pension recipiency.

C.II Forward Looking Models of Pension Incentives

The following section formalizes predictions of the effect of changes to pension incentives under the RRA on both aggregate retirement timing and differences in retirement timing by benefit percent change. The main predictions are that higher pensions should induce many to retire earlier, that there is little incentive to choose to retire at dates other than 65 and 70, and that lower average wage workers (with higher benefits as a share of their average wages) should retire earlier. These carry through whether the focus is on individuals who would have been eligible under pre-RRA benefits or not, but to mirror the empirical analysis, the focus of this exposition is generally on those who would have been eligible at 65 under private plans.

I first discuss a set of simplifying assumptions for the simulations that are in part motivated by the structure of benefits and average age-profile of wage growth. The assumptions are also intended to mirror the empirical analysis that necessitates these simplifying assumptions, and the second part argues for the rough equivalence between the percent change to annual benefits and percent change to pension wealth.

C.II.i Simplifying Assumptions

Assumption 1: *Monthly Benefits Held at the Age-65 Level*
Modern defined benefit plans typically allow $\overline{w}_i$ and S_i to reflect work after the earliest retirement date, and also have benefit factors $k(\overline{w}_i)$ that depend on retirement date (e.g., Social Security early

[2]The minimum benefit rule is as follows: If $\overline{w}_i \geq \$50$, the individuals receive \$40 unless the annuity would have been more. If $\overline{w}_i \in [\$25, \$50)$, individuals receive 80 percent of $\overline{w}_i$. If $\overline{w}_i \in [\$25, \$25)$, individuals receive \$20. If $\overline{w}_i < \$50$, individuals receive $\overline{w}_i$. Finally, the minimum is always at least as high as the minimum legislated OASI benefit. Pre-RRA claimants received the same benefits as they were receiving from their private plan, but the amount was adjusted up to account for any reductions that had occurred after December, 1930, resulting in roughly \$5 more per recipient (RRB, 1938 p. 96).

[3]The earnings test under Social Security was \$15 per month through 1950. See Gelber et al. (2020) for an analysis of the Social Security earnings test in modern settings and Fetter and Lockwood (2018) regarding earnings tests in Old Age Assistance in the late 1930s.

vs. full retirement ages). Hence, $\overline{w}_i, S_i$ and $k(\overline{w}_i)$ are all functions of retirement date.[4] In contrast, Railroad Retirement benefits featured no actuarial adjustments increasing benefits for delaying retirement, as well as no further credited service years (see above), with benefits largely based on the levels of $\overline{w}_i$ and S_i obtained at age 65. Therefore, the only way annual benefits could increase after age 65 was if wages were higher.

For the below model simulations I assume this case away.[5] To provide evidence that wages are, on average, declining at these ages, I conduct a series of regressions of 1939 wages on age, age-squared, and in some specifications occupation fixed effects, which all place the predicted peak wage between ages 57-60.[6] This suggest wages had been declining prior to attaining age 65, but what matters is how future wages (at age 65) compare to the average credited wage, which is drawn heavily from wages at younger ages (see Appendix C.III for further details). It does indeed appear that the average might be somewhat lower than that earned at age 65 or expected to be earned in ensuing years, but the difference, and how it would affect the average, is quite negligible. I estimate 1939 average wages for railroad workers aged 64 were around $150, whereas I estimate their average credited monthly wage (on average) to be around $135. Substituting in the higher wage into the average (based on 30 years of service) implies a change in average monthly wages of only roughly $.70 from an additional year of work. Even compounded over the remainder of a lengthy life, this is far less than the amount of pension wealth lost from an additional year of work.

Assumption 2: *No Early Retirement*

As noted above, early retirement was allowed on a reduced basis at 60, but it appears to to not have been taken up by many. Further, the cohorts studied in the elasticity analysis are 65 and older in 1940 or already 62 and above when benefits became broadly available in 1938, indicating they had at best a constrained choice over retiring early. I therefore abstract away from considering early retirement, while noting that it is interesting more did not take it up, particularly in light of the spike in early retirement observed today and how quickly it appeared in the aggregate data after it originally became available in Social Security.[7]

Assumption 3: *The Average Wage for their pre-RRA pension would have been the same*

Almost all of the pre-RRA pensions used the ten years of wages prior to claiming to compute average wages. This appears to have been true past age 65 regardless of whether it reduced earnings. Rather than impute wages further, my preference is to assume the average wages would have been the same as that calculated under the RRA. What matters is what individuals expected to get; on the one hand, had the private pensions continued and they turned 65 in 1940, their wages would have been very much reduced by the Depression (see Figure C.3). On the other-hand, the decade

[4]See Brown (2013) for the case of teacher pensions; Stock and Wise (1990) for private pensions; and Coile and Gruber (2007) for Social Security.

[5]Since potential future wages are never observed if an individual retires, studies typically make assumptions of constant wage growth Coile and Gruber (2007).

[6]This is true whether the wages are restricted to those under 65 or all individuals *observed working*. Given that a key result of this paper is that individuals who are still participating in the labor force at ages 65 and older are *higher wage workers*, including these workers would function to increase the maximum age.

[7]While speculative, this may be because this paper focuses on workers with good earnings prospects, since modern evidence indicates negative selection into early retirement.

preceding retirement ages surrounds peak wage ages, which may indicate they expected somewhat higher average wages. Either way, it is likely that average wages as calculated under the private plans and under the RRA would be highly correlated.

This discussion illustrates an important point regarding substitution and income effects. Under bot railroad and other industrial pensions, the substitution incentive from lower future wages inducing lower benefit levels should further early retirement; the income and substitution effect go the same way. Under the RRA, and under Assumption 1 above, there are no substitution incentives. Thus, the *change* in substitution incentives should induce people to work *longer* after the RRA. This may imply the estimated elasticities in this paper provide a lower bound on the income effect.

C.II.ii Approximate equivalence of growth in annual benefits and in pension wealth

Modern economic theory on pensions and retirement focuses on forward-looking measures that compare the gains from working versus retiring at any given point in the future (Stock and Wise, 1990; Friedberg and Webb, 2005; Coile and Gruber, 2007). These are functions of the present value of pension wealth, which for an individual claiming a defined benefit pension at date t is given by the following:

$$\text{PVPW}_t(\overline{w}_i, S_i) = \sum_{s=\max\{t,65\}}^{T} \left(\frac{1}{(1+r)^{s-t}} \right) \delta_{s|t} B(\overline{w}_i, S_i)$$

where $\delta_{s|t}$ is the probability of surviving to date s conditional on being alive in year t, r is the interest rate, and T is the age of death. Under the above assumptions, the monthly benefit begins at age 65 and does not subsequently depend on age (hence the benefit $B(\overline{w}_i, S_i)$ does not depend on t. Because the monthly benefit is therefore largely independent of retirement date, the percent change to monthly benefits $\%\Delta B(\overline{w}_i)$ defined in Section I.D is roughly equivalent to the percent change in pension wealth.[8] Further, this allows me to relax typical assumptions concerning age-specific probabilities of survival or discount values, or how these may correlate with wages or other characteristics.

C.II.iii Forward Looking Models of Pension Incentives and Retirement

- Peak Value

Coile and Gruber (2007) and Friedberg and Webb (2005) advocate for a "Peak Value" measure that compares the difference between $\text{PVPW}_t(\overline{w}_i, S_i)$ at its maximum date and that today (adjusted for the interest rate). The preceding discussion illustrates that $\text{PVPW}_t(\overline{w}_i, S_i, a_i)$ is maximized for the majority of workers under the RRA at age 65. Further, the retirement date for which the Peak value is maximized *changed* only for workers previously ineligible or eligible only at age 70, while staying the same for those previously eligible at 65 (or younger). Therefore, in the context of the RRA, this model predicts all eligible workers should retire at age 65 and that the *new* spike in claiming at age 65 (Figure 4 panel (a)) is driven entirely by new coverage. To some extent

[8] This may be one reason why the estimated elasticities in this setting are large, whereas in other settings in which there is less of a correlation between 1-year accruals to pension wealth and the value of pension wealth, accruals may matter less (Coile and Gruber, 2007).

the second prediction is likely true, but the remaining density at ages above 65 indicates the first cannot be. Perhaps more importantly, the Peak Value framework implies no scope for retirement date to depend on benefit levels, since benefit levels are fixed at age 65 for nearly all workers.

- Option Value

An alternative, structurally derived measure is the "Option Value" (Stock and Wise, 1990), which contrasts the future gains from retiring at any given date from continuing to work and builds in the relative trade off between wages and progressive benefits. For the common assumption of constant relative risk utility, the value of retiring at date R is:

$$V(R) = \sum_{s=t}^{R-1} \left(\frac{1}{(1+r)^{s-t}} \right) \delta_{s|t} [w_{it}]^{\gamma} + \sum_{s=R}^{T} \left(\frac{1}{(1+r)^{s-t}} \right) \delta_{s|t} [\kappa_i \times B(\overline{w}_i, S_i, a_i)]^{\gamma}$$

where γ if the coefficient of risk aversion and κ_i is the disutility of work ($\kappa_i > 1$). Importantly, κ_i is allowed to vary arbitrarily across individuals. The optimal retirement date is the date R^* that maximizes $V(R)$, subject to $R \leq 70$: $R^* = \max \left\{ \underset{R}{\text{argmax}} \{V(R)\}, 70 \right\}$, and the option value is given by $OV(R) = V(R^*) - V(R)$.

I consider an individual who is age 62 and simulate $OV(R)$ for many $(\overline{w}_i, \kappa_i)$ pairs. I use data from Bell and Miller (2005) for $\delta_{s|t}$ and consider a representative worker with $S_i = 30$. I assume they are required to retire at 70, had a maximum age of $T = 85$, and choose $r = 0.07$. I also assume that their wages (w_{it}) are time-invariant (w_i) and related by a constant factor of 1.11 to their average wage ($w_i = 1.11\overline{w}_i$).[9] I follow Coile and Gruber (2007) and choose $\gamma = 0.75$, and solve numerically for R^*. I do so under the pre-RRA and post-RRA benefit formulae, so that the representative individual who is 62 in 1937 is considering whether to retire at 65 in 1940 or to keep working past 1940, and thus related to available measurement for the empirical analysis.

Figure C.1 panel (a) plots R^* according to the pre-RRA benefit formula (Figure 3). The conclusion regarding timing of retirement from the Peak Value model is reversed, with 70 now the optimal retirement date for workers of all wages except at very high levels of disutility (Coile and Gruber (2007) and Stock and Wise (1990) both choose $\kappa = 1.5$, far lower than the threshold). By assuming that the relationship between observed and average wages is roughly independent of the level of wages, the simulation shows that there is no effect of the level of wage on retirement timing. In other words, because benefits are not progressive, the replacement rate is constant across the wage distribution, and so timing does not vary by $\overline{w}_i$.

In contrast, panel (b) shows that optimal retirement timing under newly progressive benefits is highly variable by $\overline{w}_i$. Conditional on κ_i, optimal retirement timing is increasing in average wages. If all workers have the same κ_i, then comparing retirement behavior across wages is an unbiased estimate of retirement responsiveness to benefit changes. But, if κ_i varies, and is lower for high wage workers as might be expected, comparisons of how individuals respond across wages will be biased upward. This illustrates the econometric issue in estimating the relationship between

[9] This is the ratio of wages in 1939 to predicted average wages for an individual who is 65 in 1940 (see Appendix C.III below for details). In practice, wages are predicted to be fairly constant over a 10 year interval around age 65 (see discussion above and below in Figure C.2). Replacing this assumption with the more empirically tractable assumption of declining wages will tend to further the incentive to retire later, but in practice is quite similar.

pension benefits and retirement in many settings in which all variation in benefits is driven by variation in wages (e.g., Social Security), and wages may proxy for other, unobserved preferences for continuing working (Moffitt, 1987; Krueger and Meyer, 2002; Coile, 2015).

Summary

The above exercises are useful for guiding the empirical analysis. The option value framework appears to be more realistic in this setting, so I focus on the following three predictions that it yields for pre-RRA eligible workers: First, replacement rates appear to have been too low to rationalize retirement before age 70 at reasonable levels of disutility, but if disutility is high enough, retirement should occur at age 65. Second, after the RRA more retirement should be clustered at age 65, but should still cluster at either end of the eligibility range (65 and 70). Further, the spike at 65 should be driven by lower wage workers who experienced a higher relative increase in their benefits. Third, any relationship between the disutility of work and wages will indicate that cross-wage (benefit) comparisons of retirement are biased. *a-priori* the relationship is expected to be negative, indicating positively biased estimates of retirement responsiveness.

Under the simplifying assumptions, neither model predicts the *observed* density at ages 66-69 (Figure 4 panel (a)). For a worker to find it optimal to retire at those ages under the OV framework, their expected wages must peak at that age and subsequently decline at significant enough a rate for them to not find it attractive to work through age 70. I do not attempt to model this case, but only note that it is clear the models do not capture all determinants of retirement. Part of the observed density at ages between 65 and 70 could also be due to a lack of understanding of the pension rules (see FN 22) or adjustment frictions in behavior (Manoli and Weber, 2016).

C.III Constructing Average Wages

I construct average wages using three sources of information: direct observation of wages reported in 1939 in the linked sample, average annual wages available from annual reports of the Interstate Commerce Commission, and the aggregate age profile of average 1939 railroad wages derived from the 1940 complete count Census.

- Interpolating Wages in 1939 for Workers with Less Than 52 Weeks

The use of individual level wages is crucial to define the percent change to benefits accurately and with minimal measurement error. Individual-level variation will capture any permanent wage differences that are due to differences in geographic region, worker productivity, firm-specific pay, or other unobserved heterogeneity.

Ideally, benefits would be estimated only using full-time wages in 1939, but restricting attention to those who worked 52 weeks limits measured retirement to have occurred in roughly the first quarter of 1940 (the reference week for employment was the last week in March, 1940). Instead, I interpolate 1939 wages for workers who did not work the full year by simply multiplying the wages they did earn by the ratio of 52 to reported weeks worked. A natural concern is that these were not earlier retirees but instead part-time workers experiencing "on the job retirement" (Ransom and Sutch, 1986). One indication of how prevalent this issue may be is to check whether part-time workers were more likely to be in lower paying occupations (relative to 1930) than full time workers. I find little evidence of *differential* occupational downgrading ; among railroad workers 65 and older who worked all of 1939, the share with a lower 1950 occupational score is 23.6 percent. Among

those with less than 52 weeks reported, the share is 28.5 percent. For all workers I then convert annual wages to monthly by dividing by 12.

- Imputing Previous Earnings

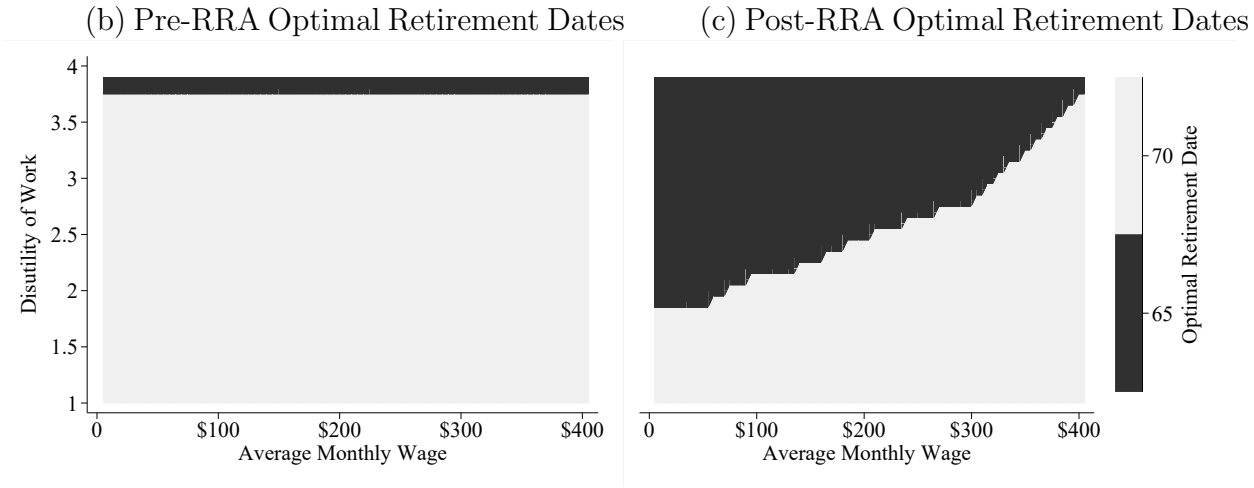
I next use the 1939 age-wage profile of railroad workers– derived from the complete count 1940 Census – to back-cast wages to earlier ages. Figure C.2 plots the profile of wages for ages 18-64 (solid, black line) and the predicted profile of wages (from a regression of wage on age and age-squared; gray, dotted line). I restrict attention to ages less than 65 because a major theme of this paper is that lower wage individuals were more likely to retire at age 65. There are three important features: first, the prediction does a good job of fitting the average wage-age relationship; second, the profile starts to level off around age 50; third, predicted maximum earnings is achieved at age 60.7. As described above in Appendix C.I, the average wages for the period 1924-1931 were taken as applicable for service credited prior to January 1st, 1937, so that individuals 65 and older in 1940 were at least 49 when wages first enter into their benefits. These features suggest individuals observed at ages 65 plus thus had, on average, slightly increasing and then falling wages prior to retirement, so that the average tends to cancel out. I impute previous wages by calculating the individual-level residuals from the predicted values and adding these to average wages at each previous age.

- Nominal Wage Growth

As with Social Security benefits until 1975, railroad retirement benefits were not pegged to inflation. I next incorporate nominal wage growth into the computation by using information available from the Interstate Commerce Commission (ICC, 1930 p. 24; 1940 p. 74). I use these for all years to maintain a consistent definition of which employees are included.[10] Average wages in the railroad industry between 1924-1940 are plotted in Figure C.3, with the solid line representing years in which wages enter in the benefit calculation. I use these average wages relative to 1939 to adjust down the age-adjusted previous wages described above. Finally, because I restrict to individuals with at least 30 years of service, I compute the average according to the rules by multiply the 1924-1931 average by 27, adding the wages for 1937 1938, and 1939, and dividing by 30.

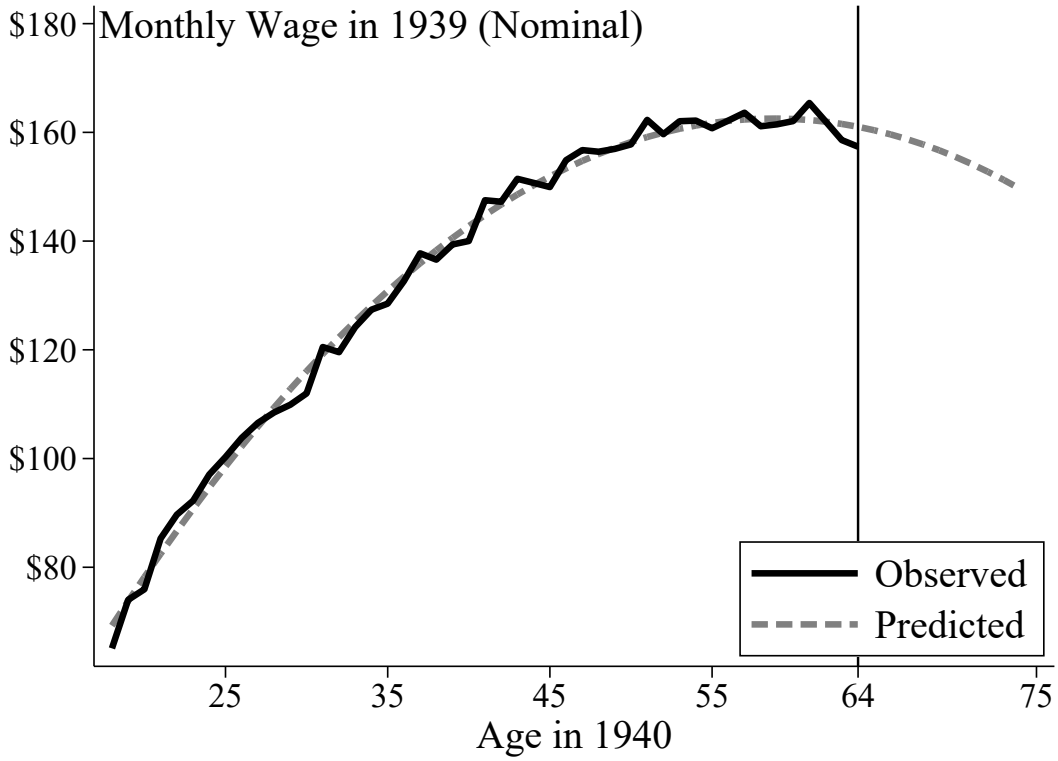
[10]There are more details on specific differences in wage growth by occupation included in a report by the Federal Coordinator of Transportation on earnings of railroad employees between 1924 and 1933 (United States Federal Coordinator of Transportation, 1935). While these estimates are somewhat comparable to those by the ICC, they are not the same. My preference is to obtain a consistent measure of annual nominal wage growth, rather than use more specific information in some years but separate sources over time.

Figure C.1: Forward Looking Measures of Pension Incentives



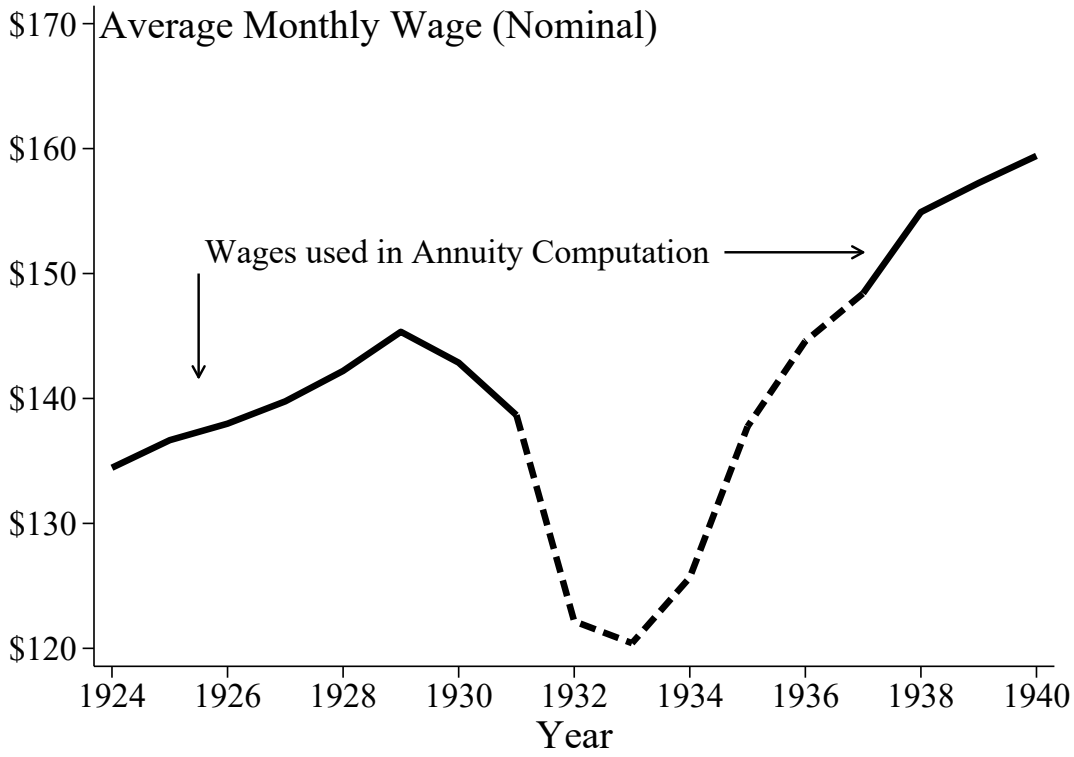
Notes: Panel (a) plots the results from simulating the Option Value model for various wage/disutility of work combinations for workers under pre-RRA plans who would have been eligible for benefits at age 65. Lighter shades refer to older optimal retirement ages. Panel (b) plots the same for benefits under the RRA.

Figure C.2: 1939 Age-Wage Relationship for Railroad Workers



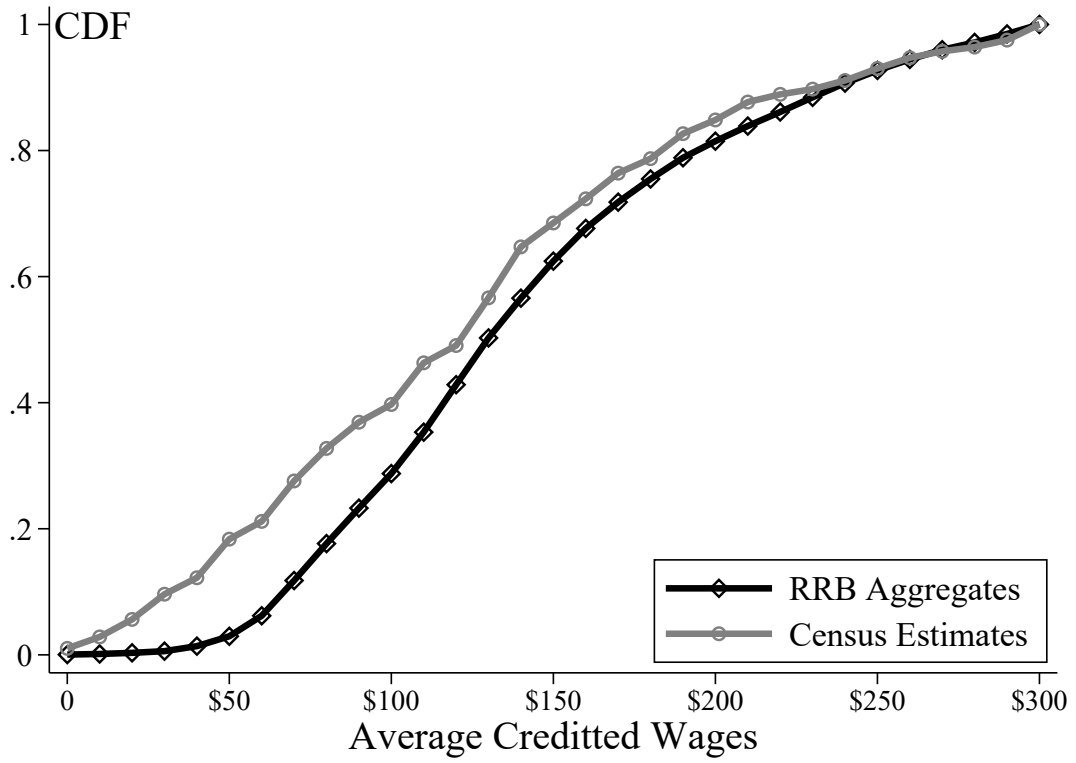
Notes: Sample is all male individuals ages 18-64 in the 1940 complete count Decennial Census (Ruggles et al., 2021) working for railroads according to the classification described in Section II and Appendix B. Sample is further restricted to those who earned positive wages in 1939 and earned less than the top-code ($5,000 in 1939 dollars). For workers working less than 52 weeks, wages are imputed by multiplying the listed earnings by the ratio of 52 to observed weeks worked. Plots average wages by age (black, solid line) and predicted wages (gray, dashed line) from a regression of wages on age and age squared, predicted out of sample for ages 65-74.

Figure C.3: Average Railroad Wages 1924-1940



Notes: Plots average wages in the railroad industry (as defined by the Interstate Commerce Commission) by year from 1924-1940. Data are from (ICC, 1930 p. 24; ICC, 1940 p. 74).

Figure C.4: Comparing Distribution of Average Wages in Census to RRB Aggregates



Notes: Compares RRB administrative information on the density of wages among retirees in 1940 (black solid line with diamonds) to estimated average wages from elasticity sample (gray solid line with circles). Average wages among retirees in 1940 is estimated from taking the difference of annuitants counts from RRB information on average wages among recipients in force as of fiscal year 1940 (RRB, 1940 p. 267) and as of fiscal year 1939 (RRB, 1939 p. 101). Average wages in the Census estimates are calculated according to the procedure described in Appendix C.III for the sample described in the notes to Table 3, except the sample is not restricted to railroad workers in 1910 but is restricted to those who were not in the labor force in 1940.