Public Pensions and Retirement: Evidence from the Railroad Retirement Act

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May, 2023

Abstract

This paper estimates how public pensions affect retirement timing by examining the Railroad Retirement Act of 1937, which replaced private railroad pensions with a national program comparable in many ways to Social Security. Leveraging linked decennial census records between 1910-1940, the first part of the analysis compares male labor force nonparticipation in 1940 relative to 1930, between workers previously in railroad versus other industries with broad pension coverage, and by age. Higher benefits led to earlier retirement, largely driven by exit at age 65. The second part of the analysis also exploits the switch from flat to progressive benefits in average wages to estimate the elasticity of nonparticipation with respect to benefits for men aged 65-69. The central estimate of 0.55 indicates a large retirement response. Application of these estimates to Social Security expansions in the 1950s suggests rising benefits was the key driver of earlier retirement among the already-insured male population during this era.

JEL-Classification: H55, J21, J26, J32, N32, N42

Keywords: Public Pensions, Railroad Retirement, Social Security, Labor Supply, Elasticity of Nonparticipation

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1. Introduction

Old Age and Survivors Insurance (henceforth Social Security) provides roughly 30 percent of elderly income in the United States (USSSA 2022a) and cost the federal government over $1.1 trillion in 2022 (USSSA 2022b). However, the pillar of U.S. social insurance has an uncertain future as population aging, low elderly labor force participation (LFP), and high recipiency may soon deplete the trust fund (USSSA 2022b). Proposed solutions often feature reductions in benefits, but the degree to which these would affect future solvency depends on how labor supply and claiming, or taxes collected and benefits paid, respond.[1] Yet, the relationship between Social Security benefit changes and retirement has proven difficult to uncover, in part 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 typically comprised of marginal changes that have been long-anticipated by retirement age.

This paper estimates the effect of changes to public pension benefits on retirement timing by investigating the Railroad Retirement Act (RRA) of 1937. The RRA replaced existing private railroad pensions with a national program comparable to Social Security, affecting workers in what was then one of the largest U.S. industries. Unexpected until the early 1930s, the new benefit formula increased the lifetime stream of pension income for workers – many of whom were nearing retirement ages – on the order of 30-50 percent, a magnitude far exceeding any modern U.S. reform. In urging President Franklin Roosevelt to sign the original 1934 RRA, Senator Robert

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

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, 153), which was created one year later.

Using research designs that account for the endogenous relationship between wages, benefits, and retirement through various comparisons across industry, age, year, and earnings, this paper achieves two primary and related goals. The first is to quantify the role of the RRA in explaining the 1930s decline in elderly male LFP which, as shown in Panel A of Figure 1, represents a key decade of change in the longer 20th century trend towards greater levels of retirement. The second goal is to estimate elasticities relating retirement timing to benefit increases. Panel B shows that much of the 1930s decline is attributable to a higher spike in the retirement hazard (difference in LFP by age) at age 65, which was the eligibility age both for railroad retirement and Social Security.

To guide the empirical analysis, I develop testable predictions from economic theory of pensions and retirement applied to the structure of pre and post-RRA railroad pensions.[2] The timing of the RRA should lead to a larger retirement response relative to reforms announced earlier in the life cycle, many workers will now find it optimal to retire at age 65 when they previously would have chosen to work longer, and newly progressive benefits are expected to induce lower wage workers to retire earlier. The model illustrates why comparisons of retirement across workers

[2]The benefit structure contains little intertemporal substitution incentives, which greatly simplifies the relationship between wages, age, and optimal retirement timing. 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.

of different wages or benefits will likely produce elasticity estimates that are biased and too large.

The primary analysis sample is based on the 1920, 1930, and 1940 full count decennial censuses. One limitation preventing earlier research on the RRA is that a worker's pre-retirement industry is generally unobservable in the census. I address this by leveraging recent advances in historical census record linkage, appending 1920-1930 and 1930-1940 linked samples to study LFP before and after the RRA according to industry as measured in the previous decade. 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 maintaining their employment during the economic downturn.

The semi-parametric specification for the first analysis is motivated by a research design that compares labor force nonparticipation of all linked railroad workers in 1940 relative to that of 1930, relative to that of other industrial pension-covered "control" workers, and for each age 50-74 relative to 64.[3] Estimates based on almost 1 million men show that the RRA led to large increases in nonparticipation only at ages 65 and older – *directly explaining* at least 12 percent of the previously unexplained 1930-1940 population-level male LFP decline ages 65-74 – while null results at ineligible ages support internal validity of the design. Further analyses suggest most labor force *exit* occurred at age 65, consistent with expected patterns. Existing

[3]The research design can be thought of as a generalized triple differences comparison, where the third difference (pension-eligible ages) is measured continuously. Section 3 details the procedure for selecting control workers from industries that had broad pension coverage in the late 1920s, which results in public utilities and certain manufacturing industries. None of the results are sensitive to the particular choice of comparison industries.

differences in 1930 nonparticipation only begin at age 70, which was the common age of compulsory retirement for private railroad pensions. Various checks establish the link between nonparticipation and pension receipt, show results are not an artifact of linkage error, and are robust to alternative linkage algorithms.

The above-mentioned estimates represent a combination of responses to new eligibility for some and varied benefit increases for others, and are of limited use for understanding pensions and retirement in other settings. The analysis proceeds by leveraging the switch from flat to progressive benefits under the RRA to estimate elasticities relating retirement timing to benefit growth for a sample of likely pre-RRA pension-eligible workers (intensive margin).[4] Motivated by little difference in 1930 retirement behavior at ages less than 70, the cross-sectional research design compares retirement in 1940 between railroad and control workers who worked in 1939, using their 1939 wage income to estimate benefit growth. The semi-parametric specification includes flexible wage-bin effects, limiting coefficients to be based on comparisons between workers with similar earnings.

Retirement responds sharply to benefit increases. The semi-elasticity at age 65 of roughly 0.18 accounts for 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. The elasticity is almost 50 percent larger when excluding control workers and wage dummies, consistent with the expected bias. Under weak assumptions, I use these semi-elasticities of labor force exit (a

[4]The most important factor determining pre-RRA pension eligibility was length of service. The defining aspect of this sample is the use of an additional set of linkages back to 1910 to determine who had likely worked in the railroad industry for 30 years or more by 1940, well above the typical requirement (see Section 2).

flow) to develop an estimate of the elasticity of labor force nonparticipation (a stock) for workers aged 65-69, which is also large, around 0.55. The analysis concludes by using these elasticities to discuss the likely role of Social Security benefit increases in explaining the elderly male LFP decline of the 1950s, which was similarly large (Figure 1, Panel A) and similarly driven by increased exit at age 65 (Panel B). I estimate that between 65 and 77 percent of the increased claiming among eligible men aged 65-69 is attributable to rising benefits over this decade.

A major theme of this paper is to provide 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 estimates based on Social Security in later periods (e.g., Krueger and Pischke 1992; Coile and Gruber 2007; Gelber et al. 2016). While the former suggest a substantial role for pensions in driving the observed increase in retirement, the latter often suggest little. The results indicate that the conclusion of declining responsiveness over the 20th century by Costa (1998a) survives comparisons of elasticities across only similarly incentivized programs, while the estimates are some of the first for a U.S. Social Security-like program based on comparisons between workers with similar wages across industries, leverage unexpected and large benefit increases late in the life cycle, and are for a period 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 and how timing interacts with previous earnings during a period in which microdata containing such information is scarce. The results show that the RRA led many to retire at 65 and also indicate that the modern, positive relationship between earnings and retirement age (Li et al. 2008) existed in 1940 in this industry at least in part because of pension incentives.[5] At the same time, the compulsory retirement provision under the RRA (also at age 70) provides one explanation for the large spike in the 1940 retirement hazard at age 70 (Figure 1, Panel B). I highlight that forward-looking economic models of retirement typically applied to modern settings do well at predicting retirement behavior in this setting.

The large, existing 1930 spike in railroad nonparticipation at age 70 also expands our understanding of early 20th century U.S. industrial pensions and retirement. While research has explored the development of industrial pensions (e.g., Haber 1978; Williamson 1997) and evidence suggests the importance of pension income in the early 1930s (Moen and Gratton 1999), little evidence of how early pensions affected retirement exists (see Alter and Williamson 2018 for an exception). As Panel B of Figure 1 shows, the 1930 retirement hazard is highest at age 70 (as is the case in 1920). This paper is perhaps the first to note 70 as a focal retirement age among U.S. men in the pre-New Deal period and to provide empirical evidence for private pensions as one explanation.

[5] Male nonparticipation measured in the census first spiked at age 65 in 1940 because of federal public assistance (Fetter and Lockwood 2018) and at age 62 in 1970 after early retirement was legislated for men (Burtless and Moffitt 1986). Recent evidence suggests changes to the Social Security full retirement age leads to both later benefit claiming (Song and Manchester 2007; Deshpande et al. 2021) and retirement timing (Behaghel and Blau 2012; Mastrobuoni 2009), while earlier work found higher wages are associated with delayed retirement (see Mitchell and Fields 1981, 47 for a review).

2. Railroad Pensions: Background and Expected Retirement Response

2.1. Private Railroad Pensions

U.S. private pensions originated in the railroad industry in the 1870s (USRRB 2018).[6] In 1900, the Pennsylvania Railroad 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).[7] By 1931, 84 plans covered over 90 percent of railroad workers.

The rise of railroad pensions coincided with a quickly expanding industry, characterized by both a rapidly increasing and aging workforce. Employment rose from roughly 620,000 in 1900 to 1.6 million in 1920, when industry revenue peaked (excluding World War II; Ruggles et al. 2021; Carter et al. 2006, Series Df927–955).[8] The 1920s represented a turbulent time for the industry. Technological advancements (Graebner 1980, 154) and a secular decline in passenger traffic from the spread of automobiles (Thompson 1993, 63-64) led to layoffs, with union-negotiated seniority provisions ensuring that workers with longer service maintained their right to employment (Ekern 1934; Harbison 1940). Because of common maximum-age limits

[6]Consistent with economic models of pension provision (Lazear 1979), plans arose in part because of 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 who could do a bigger day's work. This may be efficiency, but Lord save the industrial world from such efficiency!" quoted in Railway Age (1921, 407) from the *Wall Street Journal*.

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

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

in hiring (Haber 1978), those with longer service were typically older, so layoffs fell largely on younger workers. To illustrate the scope of aging, using the 1900, 1910, and 1920 full count decennial censuses I predict the number of individuals 65-80 who were working or had worked for railroads.[9] Online Appendix Figure A.1 shows that, between 1920 and 1940, the stock of individuals aged 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 in railroad pension recipiency (Panel A) and expenditure (Panel B) tracked the rapidly aging stock of current and former railroad workers. Online Appendix Figure A.3 plots the recipiency rate (i.e., the 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.[10] The success railroads had found funding pensions out of general revenues proved insufficient during the Depression, leading to benefit cuts in the early 1930s and further retention of older workers at the cost

[9]The restriction to male workers is not quantitatively important, as few women worked in the industry, particularly at older ages. For 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, so each series begins 16 years after the census. The prediction underestimates those 65 and older but does not account for transitions out of the railroad industry. Online appendix Figure A.1 shows the predicted values line up well across overlapping years, suggesting the magnitude of these issues is (jointly) small.

[10] The most comprehensive source for aggregate private railroad pension recipiency is Latimer (1932). The study sampled a non-exhaustive set of railroads (firm names are redacted); however, the focus was on firms with "formal" pensions – essentially those with rules stipulating the same benefits for individuals of equivalent age and 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 (ICC) expenditure series to the Latimer (1932) series in Figure 2, the former of which is based on all railroads engaged in interstate commerce. They are close to identical in overlapping years.

of their younger counterparts.[11]

2.2. The Railroad Retirement Acts

By the early 1930s, supporters of national railroad retirement legislation emphasized that it would provide both unemployment reduction and a testing ground for Social Security (Graebner 1980, 153). The result was the 1934 RRA, which set up the first federally administered and financed public pension for nongovernmental employees in the U.S. The Act was declared unconstitutional in *Railroad Retirement Board V. Alton Railroad Company* (1935). In 1937 a revised RRA established the program. All rules described below are according to that legislation.[12]

In stark contrast to the Social Security program established in 1935, the RRA of 1937 credited up to 30 years of prior service, permitting "the immediate retirement, on relatively high annuities, of large numbers of aged workers still employed" (Silverman and Senturia 1939, 3). Coverage was granted to employees of railroads engaged in interstate commerce (see online Appendix B). The newly formed Railroad Retirement Board (RRB) assumed the payments for almost all existing pensioners

[11]With the exception of six plans, none had a designated trust fund (Railway Age 1934, 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 April 1, 1932 (Pennsylvania Railroad Company 1933, 10).

[12]See Schreiber (1978) for a detailed description of the rules and legislative histories of each act and online Appendix A.5 for some further discussion of the failure of the RRA in the courts. 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).

(henceforth pre-RRA claimants) while paying benefits to retirees who had worked some time in covered employment beginning January 1st, 1937 (henceforth post-RRA claimants).[13] The Treasury funded early payments, while future benefits were financed by equivalent payroll taxes on employers and employees of 2.75 percent up to a maximum of $300 per month.[14]

2.3. Changes to Pension Parameters

Online Appendix Table A.1 provides a digitized version of a Bureau of Railway Economics study detailing the parameters of all pre-RRA railroad pensions in 1932 (reproduced from Railway Age 1934, 144-146).[15] The third column shows 1928 employment (USICC 1928) matched to plans (see online Appendix B). Because the first analysis 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, pre-RRA railroad pensions were defined benefit, given by functions of average monthly wages ($\overline{w}_i$) and service years (S_i) for workers aged (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

[13] Railroads maintained some pension expenditure on their balance sheets after the RRA (Figure 2, Panel B)– generally for auxiliary executive programs – but it was quite small, constituting roughly 3 percent of RRB expenditure in 1940 (USRRB 1940; USICC 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, 10).

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

[15] This section describes the most important changes to pension incentives from the private plans to the RRA; see online Appendix C.1 for further details.

a benefit factor $k(\overline{w}_i)$ and S_i:

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

at least three important features are worth noting. First, benefits were not progressive (i.e., featured a replacement rate of average wages $(B(\overline{w}_i, S_i, a_i)/\overline{w}_i)$ linear in S_i) most commonly with $k(\overline{w}_i) = 0.01 \times \overline{w}_i$, and the work period for which $\overline{w}_i$ was computed (not shown) was the 10 years preceding retirement date in all but two plans. The available evidence strongly suggests railroads strictly adhered to flat benefits.[16] Second, roughly 70 percent of workers were subject to compulsory retirement provisions; for 99 percent of them, the age was 70. Third, $\underline{a}$ and $\underline{S}$ are listed for two "types" of pensions – age and disability. While old age and disability insurance target distinct groups in modern settings, both types were often indistinguishable in their 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, S28).[17] Roughly 40 percent of workers faced

[16] Using the sample described below in Section 3.2.5 for Figure 4 (Panel B), I regress the replacement rate on average compensation across 23 occupations, finding a tight zero; the coefficient is less than 0.01% of mean replacement (p-value=0.38) and similar when weighted. This implies it is reasonable to abstract away from other small plan deviations, such as maximum and minimum monthly amounts, as they evidently did not bind in general.

[17] Of the roughly 50,000 pre-RRA claimants whose pensions were taken over, 56.4 percent were reported as retired under disability pensions, 41.9 percent under age pensions, and 1.7 percent under service pensions; a third, rarer type (USRRB 1938, 97). 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" (USRRB 1938, 97). These ideas are broadly consistent with Alter and Williamson (2018), who find that most retirements under the Pennsylvania Railroad pension for disability at age 65 occurred by employee request, indicating a retirement choice. Further, tying disability to $\underline{S}$ is, in practice, a reward for service more akin to old age pensions, while the most common value of $\underline{a}$ of 65 for disability already had a long

$\underline{a}$ (minimum across plan types) of 65, with another 12 percent facing $\underline{a}$ of < 65 (often 60 or 61). Plans often contained $\underline{S}$ but no associated $\underline{a}$, indicating that many of the remaining 48 percent had access to some type of early pension. 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 6.

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 individuals eligible for benefits). The new adjustment factor $k(\overline{w}_i)$ was progressive in $\overline{w}_i$, given by 2 percent of the first \$50, 1.5 percent of the next \$100, and 1 percent of the balance. S_i was capped at 30, with a maximum monthly benefit of \$120. The period for which $\overline{w}_i$ was calculated was a weighted average of 1924-1931 and post-1936 (see online Appendices C.1 and C.3). Both pre and post-RRA benefits contained a strict earnings test; that is, both required labor force detachment as precondition for benefit receipt (see online Appendix C.1).

By relaxing eligibility restrictions and increasing benefits, the RRA led to dramatic increases in both railroad pension recipiency and expenditure. By July, 1937, nearly 50,000 pre-RRA claimants had been taken over by the RRB, and in 1940, over 140,000 individuals received annuities under the act (Figure 2, Panel A). Put differently, the estimated recipiency rate rose from roughly 45 percent to 83 percent between 1931 and 1940 (online Appendix Figure A.3). Over the same period, real

history denoting "retirement" and "the old" (Costa 1998a, 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. 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.

average monthly payments per recipient increased by roughly 30 percent. This partly reflects changes in eligibility; in Section 6, I estimate the increase in average benefits for individuals eligible under private plans (generally corresponding to higher S_i and $\overline{w}_i$) was higher, around 50 percent. The average 1940 benefit was quite generous relative to other contemporary elderly transfers and the cost of living.[18]

2.4. Expected Effects on Retirement Timing

When the RRA was passed, many railroad workers nearing retirement ages experienced sudden and unexpected increases in pension wealth (discounted lifetime stream of benefits). A typical railroad worker I will examine is one in their late 50s during the Great Depression who had worked for decades expecting a pension according to relatively constant rules.[19] Such workers had seniority rights to employment and learned sometime between 1934 and 1937 of a federal entitlement to much higher benefits when they turned 65.

Because early beneficiaries paid little in contributions, economic theory predicts 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, 156) and occurred close to retirement ages for many, life cycle models predict a large

[18]The nominal 1940 average monthly benefit (\$65.60) was over three times the amount given for elderly public assistance ($\approx$ \$20), Social Security for a worker only ($\approx$ \$22), and median rent ($\approx$ \$18), and nearly double that of Social Security for a worker and spouse ($\approx$ \$36) (USRRB 1940; Carter et al. 2006 Series Bf649-662; Bf461-475; Ruggles et al. 2021). Average railroad wages were \$159.42 (USICC 1942, 59).

[19]The relatively flat recipiency rate (online Appendix Figure A.3) provides evidence of minimal changes to eligibility in the decades leading up to the RRA. Further, the evidence in footnote 16 shows that the typical adjustment factor of 1% was largely adhered to and, as noted earlier, was based on the Pennsylvania Railroad's formula circa 1900.

supply response (Moffitt 1987; Krueger and Meyer 2002).[20] In considering the information set available to workers making their retirement decision, note that the great majority of individuals who will be studied in 1930 had claimed their pension too early to have taken into account benefit cuts of the early 1930s or later increases under the RRA, while those under 68 in 1940 were too young to have been eligible before the RRA.

Many individuals with insufficient service to qualify for private pensions now also expected benefits retroactively applied to decades of work. Because wages were positively correlated with service (USRRB 1938, 102), cross-wage comparisons of retirement among all railroad workers 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 6 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 to

[20] 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, simpler pension rules are easier to understand (Asch et al. 2005). Second, the nationalization of railroad pensions was a prominent issue, discussed in various venues such as trade newspapers (Railway Age 1934, 144-146). Finally, older workers are more likely to know about their benefits (Gustman and Steinmeier 2004).

benefits as the percentage 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 equation (2) is that the linearity of both $B_{RRA}(\overline{w}_i, S_i)$ and $B_{Priv}(\overline{w}_i, S_i)$ implies the percentage change in benefits is independent of S_i and can be written as $\%\Delta B(\overline{w}_i)$.[21] Further, as shown in online Appendix C.2, $\%\Delta B(\overline{w}_i)$ is equivalent to the percentage change in pension wealth under a reasonable assumption of weakly declining wages after age 65, which allows me to relax the usual assumptions concerning survival probabilities or discount factors, or how these may correlate with wages or other characteristics. 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.

In online Appendix C.2 I develop a set of predictions from a simulation of an "Option Value" model (Stock and Wise 1990) – which contrasts the gains from retiring at any given future date with those from continuing to work – applied to the structure of pre and post-RRA benefits. These are intended solely as a guide to inform the reduced form analyses. Little dependence of monthly benefit amount on retirement age (once aged 65) greatly diminishes any intertemporal substitution incentives (Coile 2015) because pension wealth after the RRA is maximized for nearly

[21]This is useful primarily because I do not observe S_i in the data. As noted above, the period for which $\overline{w}_i$ was calculated differed somewhat for $B_{RRA}(\overline{w}_i, S_i)$ and $B_{Priv}(\overline{w}_i, S_i)$. I describe in further detail in online Appendix C.2 why it is unclear whether the reform led to somewhat a higher or lower determination of $\overline{w}_i$. Nonetheless, it is likely that they are quite correlated, and I make the necessary assumption (absent more detailed information) that they are the same for the empirical analysis of Section 6.

all workers at 65 (benefits are "marginally unfair"; Burtless and Moffitt 1986), implying much simpler predictions over how the timing of retirement should relate to age.

Predictions — There are three main predictions implied from the model simulation: First, the RRA should result in a greater density of retirement at age 65 while also preserving some density at the compulsory retirement age of 70. Second, the spike at 65 should be driven by lower wage workers who experienced a higher relative increase in their monthly benefits. Third, any systematic relationship between the disutility of work and wages will bias comparisons of retirement across workers of differing wages. *A-priori* the relationship is expected to be negative, indicating estimates will be too large.[22] These predictions are also borne out for ineligible workers, who are included in the first analysis relating railroad status to patterns of nonparticipation for the entire industry. The following section provides some preliminary evidence on the first two predictions.

2.5. Preliminary Evidence from Administrative Aggregates

Panel A of Figure 4 plots claiming distributions by age before and after the RRA.[23] The strict earnings test implies a close link between claiming and retirement

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

[23]Early RRB publications provide the most comprehensive information on claiming age for pre-RRA claimants, ninety percent of whom had retired between 1924 and 1935. This figure combines age and disability benefits (see discussion in Section 2.2.1). Online Appendix Figure A.2 reproduces this density along with separated densities for age and disability, and shows a larger spike in age

– 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 representing retirement timing (a point I provide direct evidence on in Section 6). 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. While some of the spike is likely driven by workers who were not eligible for pensions earlier than age 70, 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, the magnitudes of the spikes reverse. The model predicts that much of the new spike at age 65 should be due to benefit increases among existing eligibles. To provide initial evidence on this point, I digitize two series' at the level of 21 Federal Coordinator of Transportation (FCT) occupation codes containing average monthly benefits, average claiming ages, and the number of claimants in June, 1938, separated by pre and post-RRA claimants (USRRB 1938, 90, 104). I demean the (weighted) average of retirement age, difference the data, and relate this to benefit growth at the occupation level.[24] Note that this is the

benefits at 70 and disability benefits at 65 (with around 65 percent of the latter still claiming at ages 65 or older).

[24]For pre-RRA claimants, the aggregates are at the level of 21 FCT occupation codes; for post-RRA of 102 ICC occupation codes, which aggregate to FCT codes (USRRB 1938, 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 claimants, respectively. As noted in online Appendix C.1, 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 because of pension cuts or to holdout for federal benefits. Indeed, the average claimant age in the 1936-1937 fiscal year was 70.3, while it was 67.5 for the 1940-1941 year (USRRB 1941, 212). Removing the average age is a rough attempt to isolate variation in claiming age across benefit amount. For post-RRA claimants, the figure would ideally use information on average benefits

percentage change within occupation, not individuals, and is in part comprised of new eligibility.

Panel B of Figure 4 shows the expected negative relationship, where marker size denotes the total claimant count (pre and post-RRA claimants) for each occupation. Superimposed is the result from a weighted least squares regression of the form: $\Delta(\text{Retirement Age})_o = \alpha + \beta \times (\text{Benefit percentage change})_o + \varepsilon_o$, where o indexes occupation group. The estimated effect of -1.86 years implies that a one standard deviation increase in benefit growth (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.

3. Data

This section describes the sample used for the first set of estimates engaging with nonparticipation in the railroad industry as a whole. This sample then serves as the basis for further restrictions used to estimate responsiveness to benefit changes, described in Section 6.

3.1. Linked Decennial Census Data

Researchers studying retirement during this period often turn to the decennial census (e.g., Ransom and Sutch 1986; Moen 1987; Costa 1995; Friedberg 1999; Fet-

and retirement ages for those who retired in 1940, but I have not found this information in later publications of the RRB annual report.

ter and Lockwood 2018). In principle, the recent public availability of full count censuses (Ruggles et al. 2021) allows an examination of retirement for the full population of railroad workers. However, it is not generally possible to identify a worker's previous primary industry of employment when they are observed out of the labor force.[25] I address this by leveraging recent developments in decennial census record linkage, which facilitate comparisons of labor force outcomes ten years after observing industry. In the 1920 and 1930 censuses, I keep men in the labor force aged 37-67, link to the following census year (1930 or 1940) using the publicly available linking algorithm provided by Helgertz et al. (2020), and measure LFP outcomes for individuals aged 50-74 in the later year.[26] I then stack the 1920-1930 and 1930-1940 linked samples generated from these links.

Treatment Group: Railroad Workers — I use the RRA rules to define eligibility for railroad retirement benefits based on industry and to a lesser extent occupation (see online Appendix B). To verify accuracy, I compare administrative information on covered employment (USRRB 1941, 162) – workers who paid railroad retirement

[25] Among the approximately 2.8 million men aged 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. The 1940 census asked about an individual's "usual industry" over the previous decade (irrespective of their current LFP). I use this question to rule out that results are driven by error due to linkage or temporary employment in Section 5.5.1.

[26] 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 (Helgertz et al. 2022). However, I show in online Appendix A.3 that results are not sensitive to using any of the algorithms provided by Abramitzky et al. (2020). See Helgertz et al. (2022) for a full description of the linkage methodology, as well as Bailey et al. (2020) and Abramitzky et al. (2021) for discussions of the tradeoffs and frontiers in historical census record linkage.

payroll taxes – to employment totals in the 1940 full count census. 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 codes to classify railroad workers in 1920 and 1930; however, as noted above, many of these workers were previously ineligible for private railroad pensions, which motivates further sample restrictions in Section 6.

Control Group: Workers Covered by Other Industrial Pensions — The 1930 share of U.S. non-agricultural employment on railroads declined markedly at ages 65 and 70 (see online Appendix Figure A.4). Extensive pension coverage explains why pre-RRA retirement of railroad workers differed from that of the average U.S. worker and suggests a natural control group – other industrial workers covered by private pensions. I detail my procedure for classifying workers as likely covered by pensions in online Appendix B. Given the use of full count censuses, I focus on minimizing the probability of false classification rather than include as many covered workers as possible. I calculate 1930 census-recorded employment for non-railroad industries with pensions and compare these with 1929 pension covered employment by industry provided by Latimer (1932), the most extensive survey of U.S. industrial pensions at the time. If census employment is much larger (ratio >3) or much smaller (ratio <1/3), I omit that industry. Utilities and a subset of manufacturing industries comprise the final comparison group (in practice, the specific choice of included industries does not impact any results). These pensions contained similar rules to those of railroad plans.[27]

3.2. Representativeness and Balance

I keep those individuals in the 1920-1930 and 1930-1940 linked samples who were working in railroad or control industries in the base year, resulting in 956,391 men aged 50-74 when LFP outcomes are measured (177,031 are aged 65-74). Following the literature standards (Abramitzky et al. 2020; Bailey et al. 2020), I estimate weights to make the sample representative of the population at risk of being linked.[28]

Linked Sample Representativeness —Online Appendix Table A.2 presents balance tests between the linked sample and population at risk of being linked for 1920, 1930, and pooled. Columns 1-3 show that linked individuals are more likely to be married, to have children, and to have more children. They are slightly positively selected on socioeconomic characteristics, with higher occupational income scores (median income by occupation according to 1950 wages) and a higher probability of

[27] Among firms surveyed, the 1927 average annual railroad pension of \$584 was quite similar to that of utilities (\$630) and closer still to that of manufacturing (\$602) (Latimer 1932, 223). Of 15 public utilities plans featuring 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, 77-78).

[28] 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 (i.e., men aged 37-67 in the base year in railroad or control industries) where the dichotomous outcome variable y_i is equal to 1 if the individual is linked forward. 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, 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 $w_i = \left(\frac{1-\widehat{p}_i}{\widehat{p}_i}\right) \times \left(\frac{\bar{y}}{1-\bar{y}}\right)$.

home ownership.[29] Columns 4-6 provide re-weighted tests. While some differences stay statistically significant, the size of all differences are small and economically insignificant (the largest is home ownership, wherein unlinked individuals are just 3.6 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 (no results are sensitive to weighting; see online Appendix A.2).

Balance Across Industry, Age, and Year—The literature suggests the importance of assessing whether measures of income, wealth, and family structure may potentially confound interpretations of the RRA as driving changing retirement behavior.[30] The empirical analysis compares LFP across industries, 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 analysis sample. Column 2 shows the results from a series of descriptive triple-differences specifications that test for relative covariate differences in the base year across industry, period, and age (as measured in the later year).[31] Only two

[29]Online Appendix Figure A.18 shows no evidence of age-specific differences or changes in the probability of linkage between railroad and control workers.

[30]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 may make saving more attractive because of bequest motives (e.g., Kopczuk and Lupton 2007). The level of non-pension wealth may also impact decisions over retirement (Imbens et al. 2001; Brown et al. 2010).

[31]Specifically, for each covariate $x_{i,t}$, $t \in \{1920, 1930\}$, I estimate the following specification via weighted least squares (see footnote 28 for weight calculations):
$x_{i,t} = \beta_0 + \beta_1 \times \mathrm{RR}_{i,t} + \beta_2 \times \mathbf{1}\{t = 1930\} + \beta_3 \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_4 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} + \beta_5 \mathrm{RR}_{i,t} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_6 \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_7 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq$

are significant (race and occupation score) and the estimates are negligible relative to the means, indicating no evidence that potential confounders vary with railroad status at margins not differenced out in the analysis below.[32]

Geographic Distribution — Railroads reached peak mileage by the 1930s (Carter et al. 2006, Series Df927-955) and had been national in scope for over two decades (Atack 2013). Panels A and B of online Appendix Figure A.5 show 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). These patterns suggest comparisons are not likely to come from 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 online Appendices A.6 and A.7.

$65\} + \varepsilon_{i,t}$

[32]Online Appendix Table A.3 shows traditional mean comparisons between railroad and control workers, broken down by census year and 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 ages 65 and older (in 1930 or 1940), 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 a dummy for 1930 (relative to 1920) and a dummy for age 65 and older is 0.21 (p-value=0.97). The covariates do not predict railroad status along potentially confounding margins.

4. Research Design: Comparing Nonparticipation by Railroad Status, Decade, and Age-Eligibility

Figure 5 plots LFP by age in 1930 and 1940 for railroad and control workers.[33] 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.[34] Second, the gap in 1940 begins at age 65, the initial age of eligibility under RRA benefits. Third, control workers also exit 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 2 and patterns in Figure 5 provide evidence of the link between pensions and retirement behavior as measured in the census. The decline in LFP 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 the selection of control industries. The following specification estimates the magnitude of these gaps at every

[33]See online Appendix A.1 for a discussion of changes to LFP measurement between 1930 and 1940. Gaps are slightly smaller when using a consistently defined (but poorer) measure of LFP, but the patterns are quite similar (online Appendix Figure A.13).

[34]Because of greater coverage in the railroad industry and broad availability of benefits at age 65, the reader might expect an existing gap at ages 65 and older in 1930. However, another conclusion to come from the model simulation in online Appendix C.2 is that replacement rates were too low before the RRA to rationalize retirement before age 70 at reasonable levels of disutility. As some supporting evidence, online Appendix Figure A.16 shows that when compared with 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 even larger).

age $a(i) \in [50, 74]$ relative to 64:

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

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, providing graphical evidence analogous to tests for differential pre-trends from event study specifications. The $\widehat{\pi}_{a(i)}$ and $\widehat{\rho}_{a(i)}$ indicate 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 the aforementioned county-level variation in Depression severity and alternative government old age support, I cluster standard errors at the county level (in $t-10$), but also present results clustered at the state level in online Appendix A.2.

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 1940 nonparticipation of railroad workers relative to control workers, relative to these differences in 1930, and relative to workers 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 5 and online Appendix A present evidence to rule out a host of alternative explanations.

5. Results: The Effect of the RRA on Labor Force Nonparticipation and Retirement Timing

Figure 6 plots the coefficient estimates from specification (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 solid gray 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 3.2.4 of sizable behavioral effects at voluntary ages, while many workers still find it optimal to delay retirement until mandatory exit at 70. Preexisting differences (solid black line) are flat until age 70, when private pension compulsory provisions bind many workers. Both sets of estimated coefficients are only statistically significant at these ages.[35] The magnitudes of $\widehat{\gamma}_{a(i)}$ rise sharply

[35]The dashed lines are pointwise confidence bands. In online Appendix Figure A.6 I show that

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 individuals turned 65 recently and may have retired after the reference period while still 65. I therefore view coefficients as partially including the previous age, which likely explains why $\widehat{\gamma}_{65}$ is roughly half the size of $\widehat{\gamma}_{66}$.

Interpreting these coefficients relative to 1930 railroad nonparticipation yields similar patterns by age, with the largest relative increases occurring between ages 65 and 69 (between 47.9 and 73.0 percent). For ages 65-74 the increase is 33.3 percent, but declines from 60.6 percent for ages 65-69 to 17.9 percent for ages 70-74 (both because nonparticipation was already high at ages 70 and above and because coefficient estimates are smaller). Comparing the estimates instead with the observed change in nonparticipation ages 65-74 indicates roughly 40 percent would have worked in the absence of the RRA. The RRA substantially accelerated retirement of railroad workers, with the biggest impact between ages 65-69.[36]

Retirement Timing — How well do these estimates reflect the prediction in Section 3.2.4 that retirement timing should be largely clustered at either end of

conclusions regarding statistical significance do not change when using Uniform $Sup - t$ confidence bands (Montiel Olea and Plagborg-Møller 2019). I estimate the bands using using the Stata user written command available here.

[36]To show the role of control workers in forming these magnitudes, in online Appendix Figure A.9, I plot estimates from a specification similar to (3) that omits control workers – comparing railroad workers before and after the RRA. 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 that 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 home values recovered by 1940, some individuals may have exited the labor force regardless of changing pension incentives.

voluntary eligibility ages; 65 and 70? In online Appendix Figure A.7 I plot F-statistics from tests of consecutive equality (by age) of the coefficients in Figure 6.[37] I plot these for the relative effects ($\widehat{\gamma}_{a(i)} = \widehat{\gamma}_{a-1(i)}$) as well as the total effects ($\widehat{\gamma}_{a(i)} + \widehat{\rho}_{a(i)} = \widehat{\gamma}_{a-1(i)} + \widehat{\rho}_{a-1(i)}$). The only ages between 65-74 for which coefficients are statistically different are 65, 66, and 70.

I also examine the effect of the RRA on the 1-year retirement hazard (i.e., probability of exiting in year t conditional on working in $t-1$), a commonly used measure in the literature (e.g., Hausman and Wise 1985; Coile 2015) also central to the estimation strategy in Section 6. The 1930 census does not allow determination of work in the previous year, so I restrict the analysis sample to the 1930-1940 links and include only those workers with positive weeks employed in 1939. I estimate the following, cross-sectional version of specification (3):

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

the results, displayed in online Appendix Figure A.8, show flat differences in the hazard through age 64, a discontinuous spikes at age 65, and a spike again at age 70.[38] In sum, the evidence suggests that railroad workers responded to the RRA by retiring predominantly at ages 65 and 70, consistent with the expected patterns.

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

[38] The patterns indicate high 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. As such, I do not place much stock in the estimates at those ages.

5.1. Additional Evidence

Relating Nonparticipation to Pension Receipt — I test the effect on the probability of non-wage income ≥$50 per year (nominal) in 1939 as a proxy for pension receipt (this question was only asked in 1940).[39] I estimate specification (4) and plot the coefficients for nonparticipation and non-wage income receipt together in online Appendix Figure A.10, which shows the patterns track one another quite closely, offering strong evidence of the link between nonparticipation and pensioned retirement.[40]

Linked Data are Not Driving Results — Railroad retirement benefits were more generous than most industrial pensions or public elderly transfers (see footnote 18). 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 benefits should attenuate results.[41] To provide evidence on

[39] 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 (USRRB 1940, 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 non-wage income question, so I assume the age of receipt is their 1940 age (and the patterns in online Appendix Figure A.10 bear this assumption out).

[40] Because the nonparticipation estimates are the vertical sum of the coefficients in Figure 6 (and non-wage income estimates are for effects that could be estimated if that question were asked in 1930), the effects continue to rise past age 70.

[41] Nonportability of pensions across firms is a primary reason why older employees would prefer to remain at the same company. Pensions were often used to "instill self-reliance in [employees'] corporate families" (Huibregtse 2010, 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 1921b, 835). Among those railroad workers in 1930 aged 64 in 1940 who reported employment in 1940 and who had worked

this point, I use the 1940 census question asking about "usual industry" and "usual occupation."[42] I estimate specification (4) on the set of individuals in 1940 aged 50-74 who reported usually being a railroad or control worker (time subscripts are now all 1940), matching railroad and control Integrated Public Use Microdata Series (IPUMS) usual industry and occupation codes to 1950 codes. Online Appendix Figure A.11 plots these estimates both for nonparticipation and non-wage income receipt. The smaller sample size leads to less precision, but the pattern confirms flat differences at pension-ineligible ages and show a somewhat *larger* increases in nonparticipation after age 64 (compare with online Appendix Figure A.10). Similar patterns indicate 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 constitute a plausible lower bound.

5.2. Robustness and Ruling out Alternative Explanations

This section summarizes a host of additional results in online Appendix A. The definition of LFP changed markedly in 1940, from the concept of "gainful employment" to measuring work during a specific reference period. In A.1 I show that using gainful employment to consistently define LFP does not meaningfully change results. In A.2 I next show that the patterns in Figure 6 are maintained across nine

positive weeks in 1939 (n=7,204), roughly 71 percent were still working for railroads.

[42] 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 (online Appendix Table A.4) indicate a relatively small difference in occupational income scores, constituting roughly 5 percent of the mean occupation score. They are balanced on home ownership, children, and marriage, implying usual industry is a reasonable test on the validity of census linkage.

variants of specification (3) that are unweighted, include covariates, include various sets of fixed effects for interactions between county, age, and railroad status, limit comparisons to be within occupational income score or occupation, or cluster standard errors at the state level. I then document that results are not sensitive to using any of the four linkage algorithms provided by Abramitzky et al. (2020) or their intersection (in A.3) or the particular choice of railroad or control industry workers included (in A.4).

Online Appendix A also presents evidence ruling out alternative explanations. I show that differential linkage probabilities proxy reasonably well for differential mortality and that there is no evidence of differential selection into linkage (mortality) over the age profile in A.5. I next show in A.6 that results are not driven by either of the other New Deal elderly transfer programs (Old Age Assistance and Social Security). Finally, I show in A.7 that patterns 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.

5.3. Explaining Aggregate Trends in Male Elderly LFP

Panel A of Figure 1 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 5. Table 2 lists the estimates of $\widehat{\gamma}_{a(i)}$ ($\times 100$) 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).[43] This exercise indicates the RRA explains between 2.5 and 6.4 percent of the LFP decline, depending on the age considered, with more of the decline explained at ages 65-69. The last row shows the aggregate change for ages 65-74 was almost 5 percent. Fetter and Lockwood (2018) show that around 60 percent of the aggregate decline ages 65-74 can be attributed to public assistance for the poor; these results indicate around 12 percent of the remainder can be directly attributed to the RRA.

This paper is one of the first to note a focal retirement age before the New Deal of age 70 (Figure 1, Panel B). The large, existing spike in 1930 nonparticipation beginning at the compulsory retirement age of 70 provides one proximate explanation. At the same time, the continued use of compulsory retirement provisions after the RRA provides one explanation for the continuance of an age-70 spike in the hazard in 1940. Railroad pension recipiency continued to grow after 1940 (Figure 2). While beyond the scope of this paper, generous benefits and later expansions for disability, spousal support, and early retirement 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 after this period, which may be one reason why the age-70 spike is lower by 1950 and lower still by 1960.

6. Elasticities of Elderly Labor Force Nonparticipation

The results thus far represent a response to new eligibility for some and increased annuities of varying amounts for others. They are therefore of limited use for under-

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

standing 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* along the intensive margin that leverages the switch from a flat benefit formula pre-RRA to a progressive benefit formula under the RRA. The section concludes by comparing these estimates to those in the literature.

6.1. Analysis Sample

Restricting to 1940 — Recall from Section 3.2.4 that, for railroad workers who would have qualified for pensions under private plans, equation (2) shows the percentage 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 1940 nonparticipation for individuals working in 1939, leading to a natural interpretation of regression coefficients as changes in the 1-year retirement hazard (as in Section 5.5.1). Reassuringly, online Appendix Figure A.12 shows that the density of 1940 claiming 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.[44]

The predictions from Section 3.2.4 indicate that among pre-RRA eligibles, the observed spike in retirement at age 65 should be driven primarily by workers who

[44] As discussed in Section 5, the smaller spike at 65 is likely due to retiring (claiming) after being measured in the census while still 65. This is consistent with online Appendix Figure A.12, which shows a higher density at age 66 in the census-based sample relative to administrative records. The figure shows that the density formed by instead 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.

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 working past 64. Because of compulsory retirement, I limit the sample to cohorts under 70 in 1940, including those as young as 50 to show $\%\Delta B(\overline{w}_i)$ does not predict retirement at ineligible ages. Given that $\overline{w}_i$ (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 was perhaps the most important determinant of eligibility for pre-RRA private railroad pensions (online Appendix Table A.1). Tenure at the same firm was also required. I proceed by making sample restrictions that are intended to proxy for fulfilling these requirements, consistently attempting to make conservative choices for who was pre-RRA pension-eligible.

Few plans required more than 30 years of service.[45] I link the 1930-1940 component of the main analysis sample back to the 1910 census, 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.[46] Of the 1930 railroad workers linked, 43.2 percent were

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

[46]For the links between 1910 and 1930, I use the "exact-conservative" method from Abramitzky et al. (2020), 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 (online Appendix Figure A.15) suggests results would be similar using the same algorithm for both links. The match rate to 1910 is 52.7 percent for railroad workers and 52.2 percent for control

working for railroads in 1910, which is quite close to the estimated recipiency rate in the decades leading up to the RRA (online Appendix Figure A.3). Online Appendix Table A.7 compares railroad workers ages 55-64 in 1940 who worked in 1939 by whether they were working on railroads in 1910 or not (among those linked). Because service length was one key determinant of earnings (USRRB 1938, 102), wages should be higher among those on railroads in 1910. Indeed, they are between roughly 27-34 percent higher, depending on the wage measure.

Some of the above workers may have worked for separate firms, in which case they would generally have been ineligible or have faced reduced annuities (see footnote 41). As I propose that an individual observed living in the same county has a higher likelihood of working for the same company relative to someone who moved, I restrict the sample further to the roughly 72 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 (online Appendix Table A.8).[47]

Calculating the Benefit Percentage Change — This section provides a brief sum-

workers. I generate weights to make the sample representative in a similar fashion as the previous sample (see footnote 28). 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 estimated 1930-1940 weights. Online Appendix Table A.8 shows the elasticity results presented in this section are relatively stable across the various Abramitzky et al. (2020) algorithms (referred to as "ABE") as well as unweighted specifications.

[47] 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 2). Further, Panel A of Figure 4 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 3.2.4. There is limited scope for further restrictions to proxy for this margin of eligibility, but if age-65 eligibility is conditionally random after the above service and tenure restrictions, measurement error should indicate that the below estimates represent plausible lower bounds.

mary of how I calculate $\overline{w}_i$ and subsequently $\%\Delta B(\overline{w}_i)$, while online Appendix C.3 provides a detailed description. I include both full time workers and those who worked part of the year by linearly interpolating the latter's 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 profile of average railroad wages in the full count 1940 census to "back-cast" individual earnings by age to 1924 – the first year in which wages entered into annuity computations – and annual average wages from the Interstate Commerce Commission to adjust for nominal wage growth.[48] For simplicity, I apply the same procedure to control worker wages, but online Appendix Table A.8 shows that simply using their observed 1939 wage yields similar results. Finally, given the complexity of how benefits changed at both tails of the distribution (see online Appendix C.1), I omit workers with $\overline{w}_i \notin (\$125, \$300)$.[49] I then estimate $\%\Delta B(\overline{w}_i)$ via equation (2).

The final sample consists of 1,398 men aged 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.

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

[49] One additional reason for the lower restriction is that online Appendix 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 RRB administrative distribution of average wages above roughly \$125 but overestimates the density at lower wages. Another is because lower wage workers would be more likely to qualify for elderly public assistance. An additional 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. Online Appendix Table A.8 shows results are similar for various alternative restrictions on $\overline{w}_i$.

6.2. 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).[50] I estimate the following specification:

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

where the coefficient of interest (η) 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) average wage bins, which limit variation contributing to η to come only from comparisons between workers in railroad and control industries of similar earnings. In other words, η represents how the differences in retirement between railroad and control workers with similar wages varies by $\%\Delta B(\overline{w}_i)$.[51] 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 converge absent the

[50] 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 online Appendix C.1).

[51] $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).

RRA, $\widehat{\eta}$ identifies the semi-elasticity of the 1-year retirement hazard with respect to the percentage change in annual benefits. Section 3 described how pre-RRA average benefits were quite similar to other industrial pensions while zero existing differences in nonparticipation in 1930 between railroad and control workers at ages less than 70 (Figure 6) provides some evidence that cross-sectional comparisons should only represent changing incentives in the 1930s.[52] Because of the rough equivalence between $\%\Delta B(\overline{w}_i)$ and the percentage change to pension wealth (see online Appendix C.2), $\widehat{\eta}$ will also approximate elasticities with respect to pension wealth (Moffitt 1987).

6.3. Hazard Elasticity Estimates

Table 3 shows the semi-elasticity estimates for each age group (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 (omitting $w(i)$). Evaluated at the mean benefit percentage change ($\overline{\%\Delta B(\overline{w}_i)} = 53.1\%$), the estimate 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

[52]This assumes that pension incentives for control workers remained similar to 1930. For a consistent group of reporting firms, the number of manufacturing and utility pensioners rose from 33,373 in 1932 to 42,085 in 1937, while nominal payments remained fairly stable from \$753 to \$777 (Latimer and Tufel 1940, 76-77). As argued elsewhere in this paper, because public assistance and Social Security provided substantially lower benefits than private pensions, these programs should not have provided any differential incentive for workers who qualified for private pensions. In summarizing the principle developments of the private pension movement in the 1930s, Latimer and Tufel (1940) wrote, "It might have been thought that conditions during the years 1932-1938 were such as to have retarded the healthy growth and development of private pensions in the United States... Despite these conditions, no downward trend occurred in the pension movement." (Latimer and Tufel 1940, p. 42).

bin fixed effects instead of the linear term (column 3), add occupation fixed effects (column 4), and 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 to flexibly control for wages indicate that the bias is positive, consistent with the expected sign (see Section 3.2.4). Various additional robustness tests presented in online Appendix Table A.8 indicate a range of between roughly 50 and 70 percent of the observed hazard explained for 11 of the 12 robustness checks.[53]

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, I instead estimate the preferred version of specification (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=0.156).

For those who remain working past 64 (and to some extent 65), there is little

[53]These robustness checks can be broken into two main groups: those that test alternative linking algorithms or weighting schemes (the first five) and those that investigate using different wage measures or including different wage ranges (the next five). The other two checks don't limit to the same county (check 11), or include only those control workers who worked in the same industry code in 1910 and 1930. Of note, the estimate for this last test is sizeably larger, implying roughly 90% of the hazard is explained.

evidence of a retirement response, except perhaps at age 69 (although not statistically significant).[54] In sum, the evidence is consistent with the prediction in Section 3.2.4 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.

6.4. Elasticities of Nonparticipation

The semi-elasticity estimates are not directly comparable with those presented in Section 5, as the latter focus on nonparticipation (a stock) and the former on labor force exit (a flow). Further, much of the literature on Social Security and nearly all of the literature on elderly transfers and retirement before the 1970s focus on nonparticipation. There is no way to leverage wage information in 1939 to directly examine nonparticipation for workers aged over 65 in 1940, so I develop a measure of the implied cumulative effect of benefit increases on nonparticipation from the hazard semi-elasticities. Counterfactual nonparticipation then forms the basis for estimates of elasticities of nonparticipation.

A simple Bayes Rule argument implies the unconditional LFP at age a can be estimated by the product of the within-cohort hazard $h(a)$ and LFP at age $a-1$.[55] Estimates of LFP are computed iteratively, using LFP at age 64 as the baseline:

[54] Declining sample sizes should caution interpretation at older ages. In a regression pooling aged 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.

[55] This assumes the probability of exiting by age is stable across cohorts. This is not the case for the cohorts who had already exited (see discussion in footnote 24) but is plausible for cohorts turning 65 after the RRA.

$\text{LFP}(a) = \text{LFP}(64) \times \left(\prod_{t=65}^{a} (1 - h(t)) \right)$. 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 specification (5) for ages 65-69 jointly, fully interacting all independent variables with dummies for each age.[56] Let the hazard estimates be termed $\hat{\eta}(a)$, I formulate counterfactual LFP by iterating on counterfactual hazards, evaluated at the mean benefit percentage change $\overline{\%\Delta B(\overline{w}_i)}$: $\widehat{\text{CF\_LFP}}(a) = \text{LFP}(64) \times \left(\prod_{t=65}^{a} (1 - (h(t) - \hat{\eta}(t) \times \overline{\%\Delta B(\overline{w}_i)}) \right)$. 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 estimates in this section 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=0.022). The elasticity of nonparticipation evaluated at the mean benefit percentage 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 $\left(\frac{.1007}{.347}\right) \times \frac{1}{.531} = 0.55$ (p-value=0.033).[57] Following the same procedure for

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

[57] LFP is overstated relative to that observed in the sample because the hazard is underestimated at each age (individuals may retire later while the same age). Counterfactual LFP is overstated in the same way. Note that, because these terms enter into both the numerator and denominator of equation (6), it is not directly evident whether this leads to an under or overstated estimate.

ages 65-74 (and assuming the "effect" of benefit increases on exit at ages 70 and above is 0) implies an elasticity of 0.45 (p-value=0.006). As expected, the elasticity declines as ages above 69 are considered; the compulsory retirement provisions, not the benefit increases, cause exit at those ages.

The elasticity of nonparticipation found is much larger than those found in modern settings from Social Security (e.g., Krueger and Pischke 1992; Coile and Gruber 2007), teachers pensions (Brown 2013), or disability insurance (Bound 1989). At the same time, the estimates are within the range of those found by Costa (1995) from Union Army pensions (0.73 in 1900; 0.47 in 1910) and close to those found by Friedberg (1999) for elderly public assistance (0.25-0.42 in the 1940s, as calculated in Costa 1998b, Table 1). My estimates complement those from earlier periods; *a-priori*, marked differences in the incentive structure and eligible population between these transfer programs and Social Security may limit the applicability of estimates based on the former to infer the consequences of changes to benefits under the latter.[58] Yet, the large elasticities found indicate the conclusion of declining male retirement

Online Appendix 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 footnote 44). If I adjust $h(a)$ by the factor of the moving average to that of each age and recalculate the elasticity, the estimate is 0.52 (p-value=0.027). If I also adjust $\hat{\eta}(a)$ by the same factor, the estimate is 0.63 (p-value=0.016), both within the confidence interval of the main estimate.

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

responsiveness over the 20th century by Costa (1998a) survives comparisons across only Social Security-type programs. The magnitude of response is consistent with the unexpected nature of the RRA and its occurrence late in life for the cohorts under study (Moffitt 1987; Krueger and Meyer 2002) and suggests one reason for the decline in responsiveness has been marked increases in how long reforms are typically anticipated.

7. Discussion: 1950s Social Security Benefit Increases and Retirement

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 percent of men 65 and older who were eligible for Social Security increased from 14.7 to 35.2, while real benefit levels among recipients increased by roughly 94 percent (USSSA 1959, 18; Haines et al. 2010).[59] In turn, male LFP 65 and older declined by roughly 11 p.p. (Figure 1, Panel A). As with railroad retirement benefits, Panel B of Figure 1 shows much of the decline is attributed to a 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 p.p., the largest historical decadal decline for this age group.

Unsurprisingly given their intertwined history, railroad retirement and Social Security were structured quite similarly during this period, with both providing progressive benefits and having the same eligibility age of 65. The elasticity estimates

[59] I focus here on male retirement to keep results consistent with earlier sections. Average 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.

in this paper speak best to the impact of *benefit increases* in the 1950s in explaining increased (and earlier) claiming. Similar to the RRA, these increases were unexpected (Moffitt 1987) and, for many, occurred close to or at pension eligible ages. The change in the share *of those eligible* who claimed benefits at ages 65-69 – from 44 percent in 1950 to 70 percent in 1960 – is suggestive that increased benefits led to earlier claiming and retirement.[60]

I focus on those individuals who would have been eligible for benefits absent amendments in the 1950s (so called "1939 eligibles"). I focus on claiming, rather than LFP, because the latter is unobservable among only those who would have been eligible under the original amendments.[61] Online Appendix A.8 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 the 1950s for men aged 65-69 who were previously eligible. This is a large effect, but consistent with two recent attempts to re-engage with

[60]Some evidence to support this interpretation may be found in surveys asking reason for 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 1940-1950 change in claiming among men 75 and older was less (17 p.p.), indicating a shift towards earlier retirement.

[61]As described earlier, the stringent earnings test of railroad retirement benefits should lead to a close link between retirement and pension recipiency. The similarity between measures of non-income wage and retirement (online Appendix Figure A.10) and claiming and retirement densities (online Appendix Figure A.12) support this assertion. The earnings test for Social Security in 1950 was also quite stringent, although mild relaxation in the 1950s may have weakened the link between nonparticipation and pension receipt 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 $1,200 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.

Social Security expansions and retirement during this period.[62]

8. Conclusion

This paper uses the introduction of railroad retirement benefits in 1937 to estimate how retirement timing responds to changes in public pension benefits. Key elements facilitating the analysis are the recent public availability of full count decennial censuses and developments in census record linkage, which allow comparisons of LFP by prior industry. The RRA explains a disproportionate share of the 1930-1940 elderly male LFP decline and retirement timing tracks theoretical predictions. Using the switch to progressive benefits, I estimate an elasticity of nonparticipation for ages 65-69 of roughly 0.55, which is larger than those typically found in modern settings but consistent with findings from contemporary transfer programs structured quite distinctly. These estimates suggest that historic Social Security benefit expansions in the 1950s explain much of the increased retirement among men in that decade, driven largely by earlier retirement at ages 65-69.

Current increases to the Social Security full retirement age occur annually and effectively lower monthly benefits if claimed at younger ages. These changes were

[62] The current exercise provides a bridge between Fetter and Lockwood (2018), who apply estimates from public assistance to estimate between 50 and 90 percent of the decline in male LFP ages 65-74 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 decline 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 RRA benefits in 1940 – especially the earnings test – are more similar to those of the Social Security program in the 1950s than those of the program circa the mid-1970s.

legislated nearly forty years ago and are scheduled to stop in 2027. The lack of subsequent congressional action and impending depletion of the trust fund indicates that, for many workers, there will likely be a much shorter window between future announcements of benefit reductions and attainment of retirement age. The results in this paper indicate that these reductions may have much larger effects on retirement behavior relative to current changes that were announced earlier in the life-cycle.

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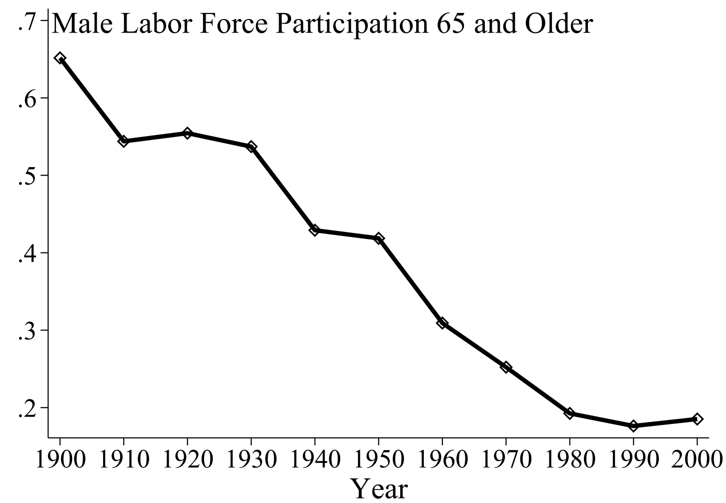
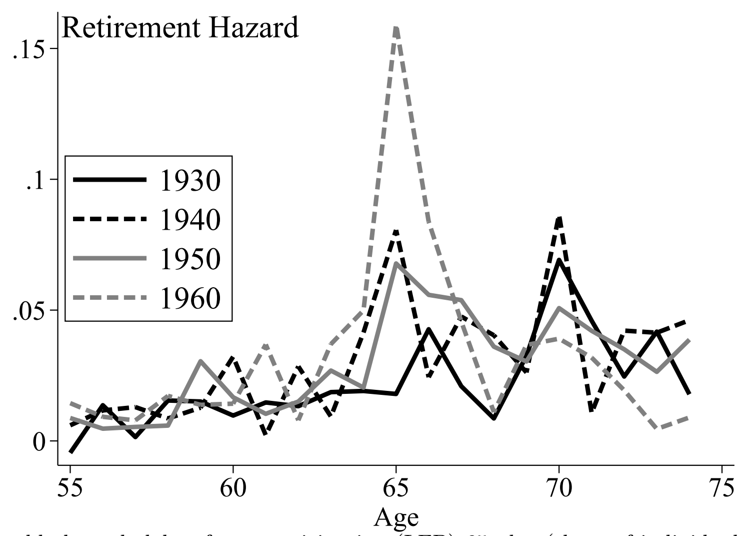
Figures and Tables

Figure 1: Trends in Elderly Male Labor Force Participation

Panel A. 20th Century Decline



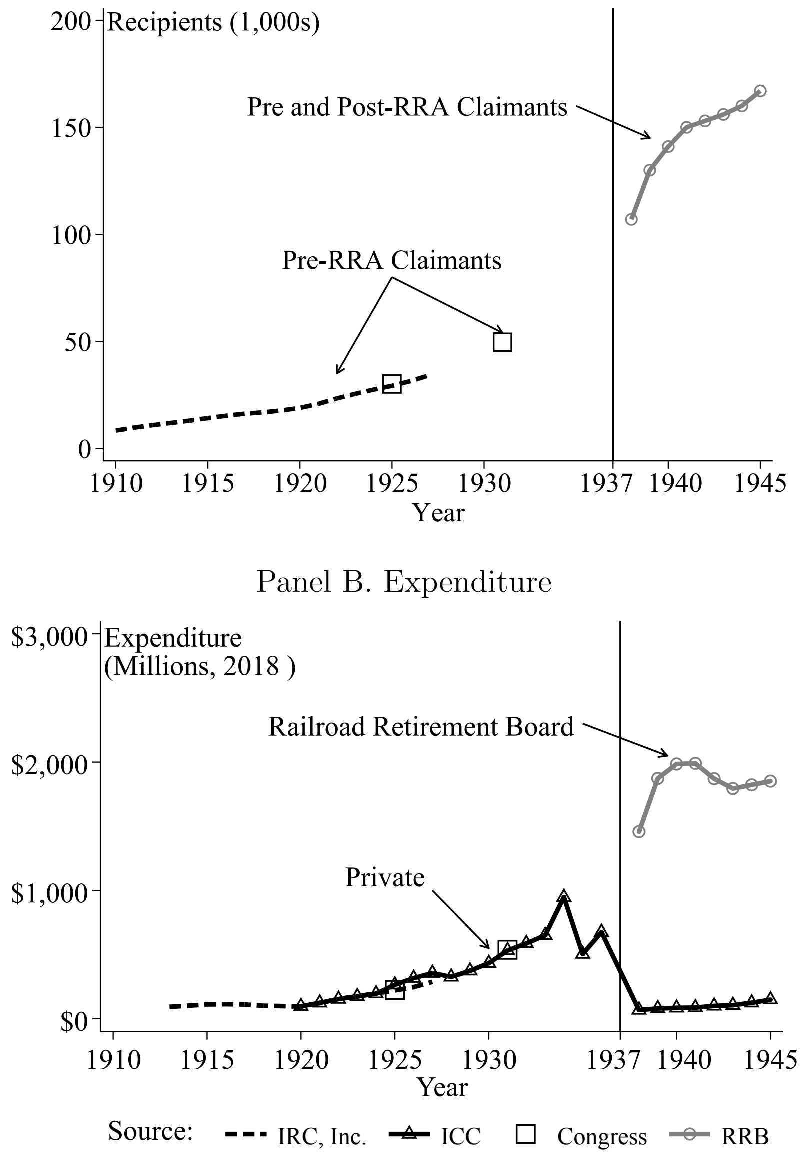
Panel B. Retirement Timing: 1930-1960



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. 2022). 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." This measure overstates LFP relative to the new definition in 1940 (see online Appendix A.1). I take adjustment factors provided by Durand (1948, 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 $a-1$, from 1930-1960 (also estimated from 1 percent decennial census samples).

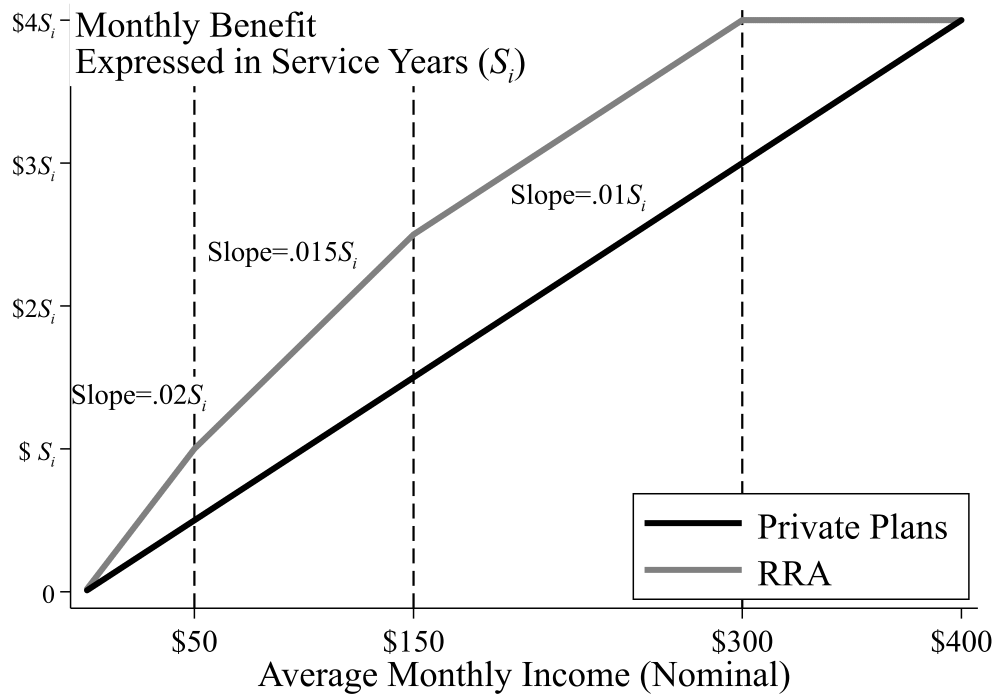
Figure 2: Growth in Railroad Pensions, 1910-1945

Panel A. Recipiency



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. Pre-RRA claimants refers to recipients who claimed prior to the RRA, whereas pre and post-RRA refers to the sum of rolled over and new pensions. Data for Panel A come from Latimer (1932, 161) for 1910-1927 (dashed black line); US Senate (1934, 92) for 1925 and 1931 (black boxes), and Carter et al. (2006, Series Bf746-761) for 1938 and later (gray line with circles). Sources for Panel B are the same, with the addition of annual pension expenditure from various editions of the Interstate Commerce Commission *Annual Report on the Statistics of Railways in the United States* covering years 1920-1945 (black line with diamonds; see online Appendix B for further source details). Recipiency and expenditure data for 1910-1927 are from a non-exhaustive set of reporting railroads (see discussion in footnote 10).

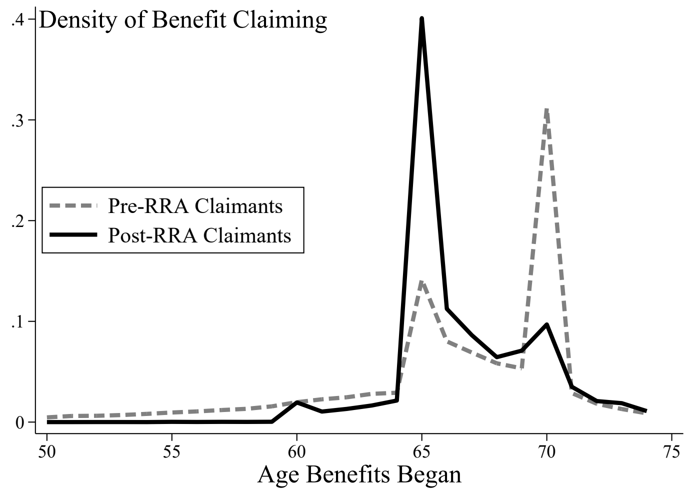
Figure 3: Pre and Post-RRA Railroad Pension Benefit Formulae



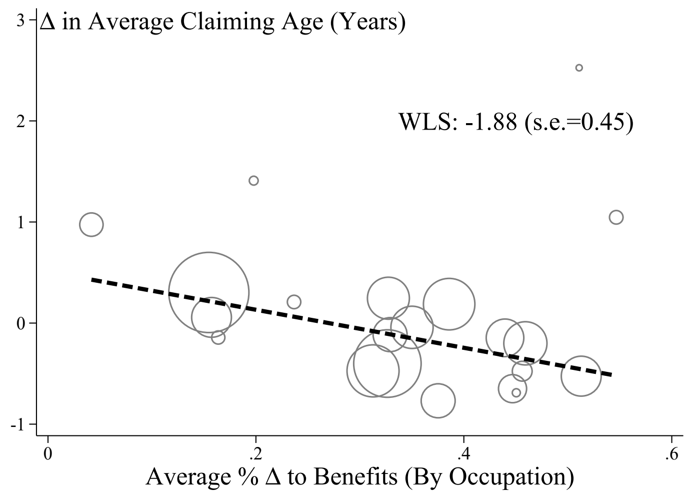
Notes: This figure plots a typical pre-RRA private railroad pension benefit formula (see online Appendix Table A.1) in black, and the formula common to all railroad workers covered by the RRA in gray. The y-axis is in terms of service years S_i. The slope values represent the slope of the schedule in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

Figure 4: Railroad Pension Claiming Ages and Benefit Changes

Panel A. Pre and Post-RRA Density of Claims, by Age

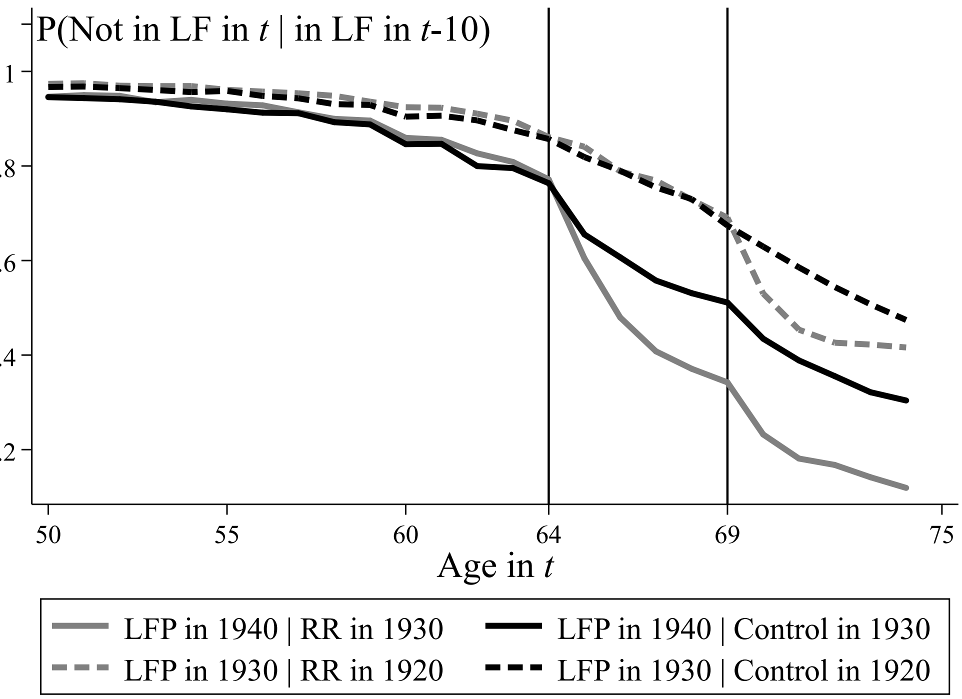


Panel B. Benefit Growth and Retirement Timing



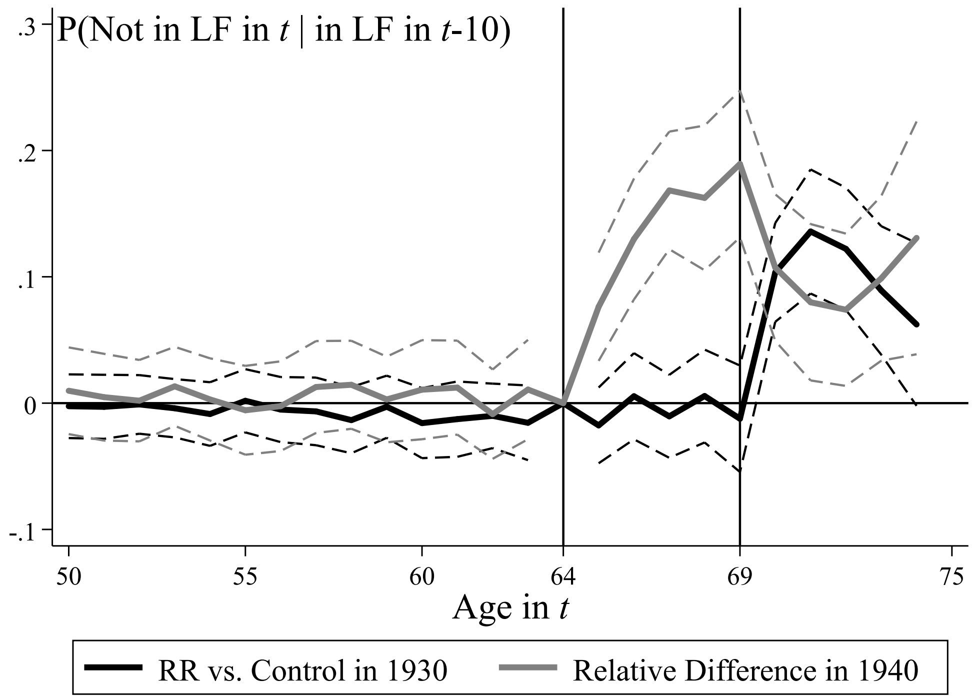
Notes: Panel A shows retirement densities by age before and after the RRA. The dashed gray line is the density of first benefit claiming age for pre-RRA private railroad pensioners, 90 percent of whom claimed between 1926-1935. I include retirees under "age" and "disability" annuities (see online Appendix Figure A.2 for each type separately). The solid black line is the density for post-RRA claimants who claimed in fiscal year 1940 (between July, 1939 and June, 1940). Roughly 2.3 percent of pre-RRA claimants and 4.1 percent of post-RRA claimants claimed at ages outside of 50-74. See main text and footnote 23 for more details. Data for Panel A come from the USRRB 1938, 99; 1941, 211, 212). Panel B Shows the difference in average claiming age (y-axis) against the percentage 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 footnote 24 for further details). Superimposed is the slope from a weighted least squares regression of the form: $\Delta(\text{Retirement Age})_o = \alpha + \beta \times (\text{Benefit percentage change})_o + \varepsilon_o$, where o indexes FCT occupation group.

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



Notes: Plots 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 footnote 28). Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and online Appendix B for more details).

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



Notes: Plots estimates from specification (3) where the outcome indicates whether the individual was not in the labor force (N=956,391), reweighted using weights generated to match the population at risk of being linked (see footnote 28). The solid black 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 $t-10$). Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and online Appendix B for more details).

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: Column 1 presents the sample mean while column 2 lists the estimates of β_7 (p-value in column 3) from a series of descriptive triple-differences specifications that test for relative covariate ($x_{i,t}$) differences (measured in the base year) across industry, period, and age of the form: $x_{i,t} = \beta_0 + \beta_1 \times \mathrm{RR}_{i,t} + \beta_2 \times \mathbf{1}\{t = 1930\} + \beta_3 \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_4 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} + \beta_5 \mathrm{RR}_{i,t} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_6 \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \beta_7 \mathrm{RR}_{i,t} \times \mathbf{1}\{t = 1930\} \times \mathbf{1}\{a(i, t+10) \geq 65\} + \varepsilon_{i,t}$. All regressions are reweighted using weights generated to match the 1940 population at risk of being linked (see footnote 28). Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and online Appendix B for more details).

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 specification (3) are presented in column 2 (see notes in Figure 6 for more details). 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 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. Data in columns 3-5 are from full count decennial censuses (Ruggles et al. 2021)

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: Estimates in columns 1-5 are from variants of specification (5) where the outcome indicates whether the individual was not in the labor force, reweighted using weights generated by the procedure described in footnote 46). Standard errors are clustered at the level of wage bin. The percentage 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 (online Appendix C.3 provides a detailed description of the procedure). Sample is comprised of all male individuals in the relevant age group in the 1940 full 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 3 and online 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. (2020); and, for railroad workers, worked for railroads in 1910 and lived in the same county as in 1930.

A. Additional Results

A.1. 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 1998; 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 little reason to expect changes in LFP measurement will lead to differential measurement error in the dependent variable between 1930-1940, across industry, and in an age window around 65, I explore sensitivity to defining LFP to instead indicate if an individual was not GE in both years (is not in the labor force in 1930 or has a "non-occupational response" in 1940).

Figure A.13 plots the results from specification (3) using not GE as the nonpar-

[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.

ticipation outcome. 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.2. 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 that 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 from specification (3) for nine variants as well as the results from the baseline model reproduced 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 $t-10$); 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 adding occupation score fixed effects; 8 the same as 7 but including occupation score-

[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.

by-county fixed effects instead; 9 the same as 8 but including occupation-by-county fixed effects instead. 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.[4]

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; variant 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 3) does not change conclusions; 3, 4, and 5 show that controlling arbitrarily for unobserved variables correlated with geography, time, industry, and age 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 occupations does not change results.

Applying the procedure of Section 5 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 for variant 2 (without weights), which places the share slightly lower at 9 percent.

[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 quite close to zero). All confidence intervals don't contain zero for the differential effects at newly eligible ages.

A.3. 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 6 that requires much more stringent sample restrictions. As described in Section 6, the use of the "exact-conservative" method from Abramitzky et al. (2020) for further linkages back to 1910 is chiefly due to public availability of the algorithm for non-consecutive census years.

I first show that results based instead on a sample using the algorithms provided by Abramitzky et al. (2020) are similar. These authors provide four algorithms, so for the sake of completeness I present results for each one, as well as the links implied by their intersection, which is the most conservative procedure (Bailey et al. 2020). 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 reproduced in bold. The results show the patterns and magnitudes are all maintained across linkage type (as with Figure A.14, I omit confidence intervals for expositional purposes). Applying the procedure of Section 5 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 five linkage algorithms.

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. (2020), as well as the intersection. The specific linkage technique does not meaningfully impact any of the results.

A.4. Choice of Treatment and Control Groups

As summarized in Section 3 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 full count census. I select control industries based on comparisons between the number of workers by industry covered by pensions provided by Latimer (1932) and industry workforce counts from the 1930 full count census. I first test sensitivity to control industries by re-running specification (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 2 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, preexisting differences at ages between 65-70 are expected because of pre-RRA pension incentives at those ages. 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). On the other hand, the non-zero existing and relative differences at pension-ineligible ages lends further support to my preferred 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 "triple-differences" summary 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.[5] 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.

A.5. Differential Mortality

If differential mortality among railroad and control workers led to differential 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

[5] 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.

probabilities to other industries (Aldrich 1997).[6] Indeed, while proponents of the RRA argued it would further the goal of safety of older railroad 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 the courts.[7]

Robustness to occupation fixed effects (see Appendix A.2) lends some support to the idea that specific occupational hazards are not driving labor force nonparticipation. 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 at risk of being matched 1920 and 1930 (those aged 40-64 working

[6] 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).

[7] In the majority opinion of *Railroad Retirement Board V. Alton Railroad Company* (1935), Justice Owen Roberts wrote 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." (US Supreme Court 1935, 12-13).

in railroad and control industries), using the same algorithm as in the main analysis (see Section 3). 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 $\ln(\text{Probability Linked})_a$ and $\ln(\text{Probability Survive 10 Years})_a$ 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 specification (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 may be viewed as contemporaneous). 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.[8] 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 age in the sample above 44 save for ages 57 and 62, none of which correspond to focal retirement ages

[8]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.

in the following decade. Reassuringly, there does not appear to be any evidence of differential selection due to mortality.

A.6. Elderly Public Assistance and Social Security

A.6.1. 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 states and 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.2 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 in 1939 to test whether measures of OAA eligibility and generosity are related to the share of county employment ages 40-64 on railroads in the 1930 full 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 a "fiscal externality" (Hendren 2016).[9] On the other hand, if OAA programs are differentially generous in higher railroad employment counties, the differential attractiveness of alternative pensions

[9]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.

could complicate interpretation of estimates as counterfactual railroad labor supply, especially for low wage railroad workers

A regression of the 1939 county share 65 plus receiving OAA on the railroad share of 1930 employment controlling for state 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 the OAA share. Given that the mean railroad employment share was 3.48 percent and the mean 1939 OAA share was 27.0 percent, this is a quite small association, but may be indicative of a positive fiscal externality on federal and state OAA savings. A similar regression with nominal payments per recipient as the outcome indicates that a 1 percent increase in the railroad employment 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 OAA eligibility and generosity did not vary in a systematic way that would bias the main estimated effects of the RRA, and provide some suggestive evidence that the RRA may have crowded out a small share of OAA expenditure.

A.6.2. 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 footnote 18), so although control workers may have had access to Social Security, their private pensions were more attractive (if they were eligible to receive them). Further, since the counterfactual of the RRA was likely coverage under both private and public pensions, including any impact of Social Security for control workers seems like a reasonable counterfactual. In other words, 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.7. The Depression and Industry-Specific Trends in Employment

The Depression exacerbated a previously mounting secular decline in railroad revenue (see discussion in Section 3.2.1). While flat existing differences and changes 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 5, 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 using labor market slack as 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.8. Using Elasticities to Estimate the Effect of 1950s Social Security Benefit Increases on Retirement

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 ("1939 eligibles"). 44 percent of eligible men in this age group were receiving (had claimed) benefits in January, 1950 (USSSA 1959, 18). Nominal benefit levels had not been adjusted since 1939 and high inflation during the 1940s led to stark declines in real benefits (benefits were not pegged to inflation until 1975). By the late 1940s, replacement rates (benefits as a percentage of average wages) had declined to less than 20 percent for a worker with "medium" earnings (Clingman et al. 2014).

The goal is to use the elasticity estimates to infer how much of the increased claiming is due to higher benefits. I relate decadal benefit and claiming changes– rather than annual changes – chiefly because adjustment frictions may lead to some delay between when benefit increases go into effect and currently eligible or near-eligible cohorts retirement behavior responds, particularly if increases are announced late in the year (Manoli and Weber 2016; Gelber et al. 2020). Further, year over year changes in Social Security 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 aged 65-69 in 1960 faced broadly higher benefits that were unexpectedly increased at some point after they were aged 55-59, and that the similar timing in the life cycle and large magnitude of these increases indicate the elasticity estimates found in this paper may be used to gauge the likely impact of these benefit increases in explaining increased retirement.

The observed change in claiming among men aged 65-69 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 who were not.[10]

I proceed via two strategies: I first use only the hazard elasticity estimate at age 65 of 0.18, 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

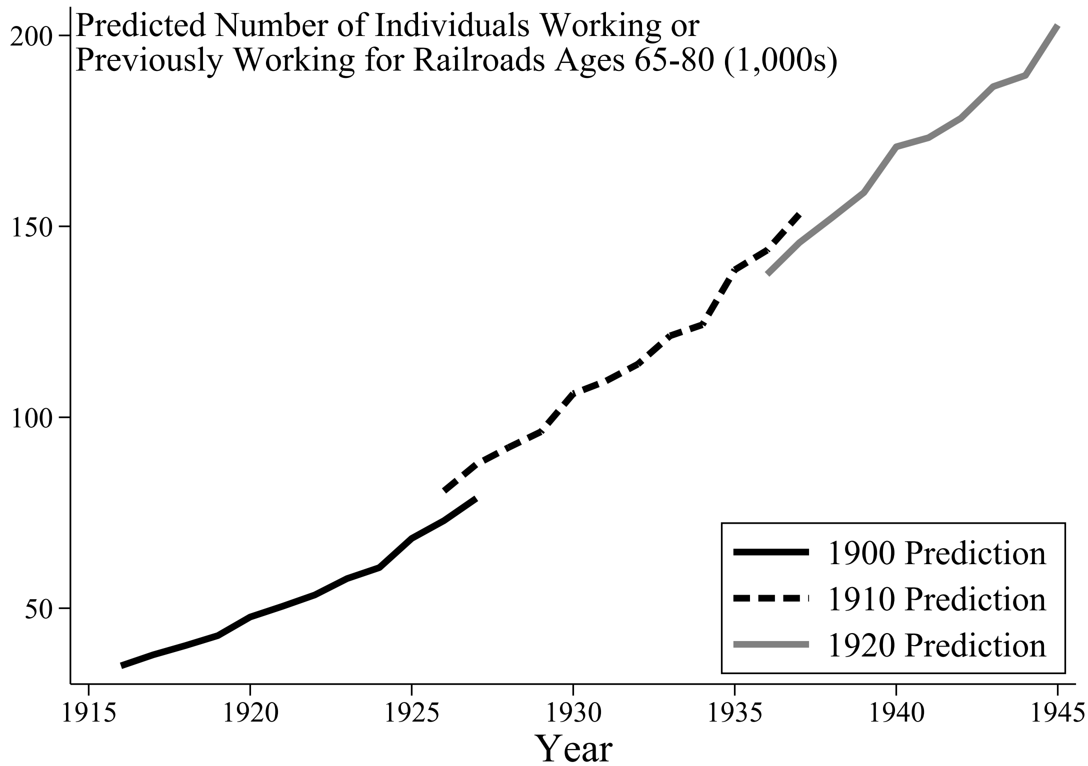
[10]Earlier covered workers had access to benefits either under the old or "new start" formula – whichever was higher (Cohen and Myers 1950) – which may imply more incentive to retire because of higher replacement rates, implying this claiming rate underestimates the claiming of 1939-eligibles. On the other hand, new eligibility for high benefits occurring late in life for previously ineligible workers may indicate a larger retirement response, which would go in the opposite direction. With these data it is not possible to assess the magnitude of these opposing effects.

16.9/26 or roughly 65 percent of the increase. I next use the preferred elasticity of nonparticipation estimate for ages 65-69 of 0.55. Applied to the 1950 claiming share of 44 percent, the elasticity estimate implies a 1960 claiming share attributable to the benefit expansions of $1.55 \times .94 \times .44$ or 64 percent, explaining (.64-.44)/(.7-.44) or roughly 77 percent of the increased claiming. Most of the robustness checks place the elasticity of nonparticipation somewhat larger, indicating an even higher percent of increased claiming explained.

Note that, due in large part to the end of WWII, an existing trend had been towards more claiming in the second half of the 1940s (USSSA 1959, 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 in 1949. Any increase in claiming that would have taken place, however, reduces the counterfactual increase to be explained, thus increasing the share explained by benefit expansions.

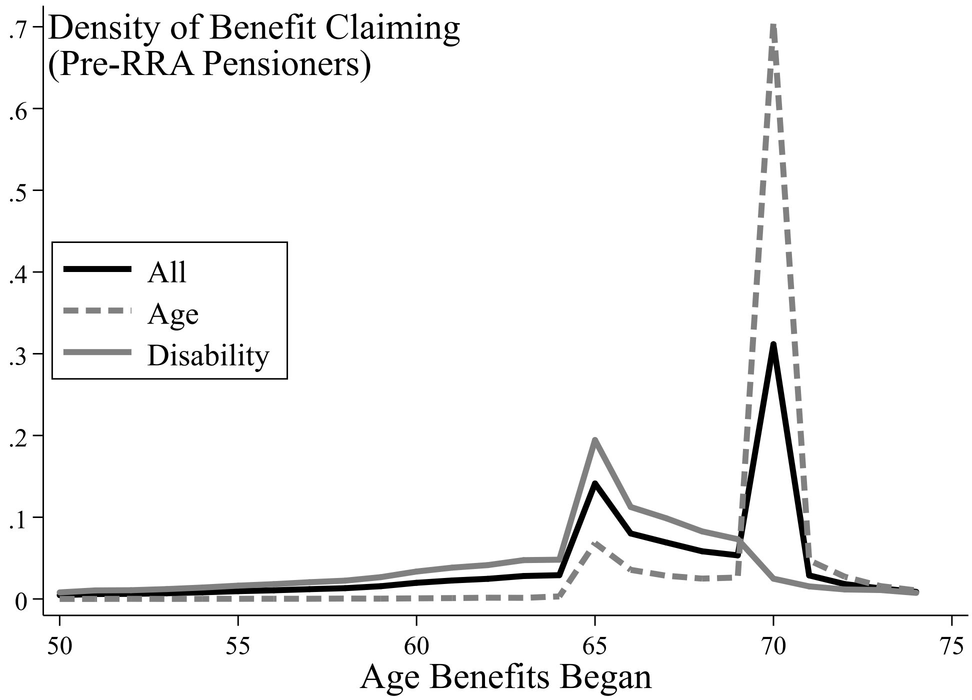
Appendix A Figures and Tables

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



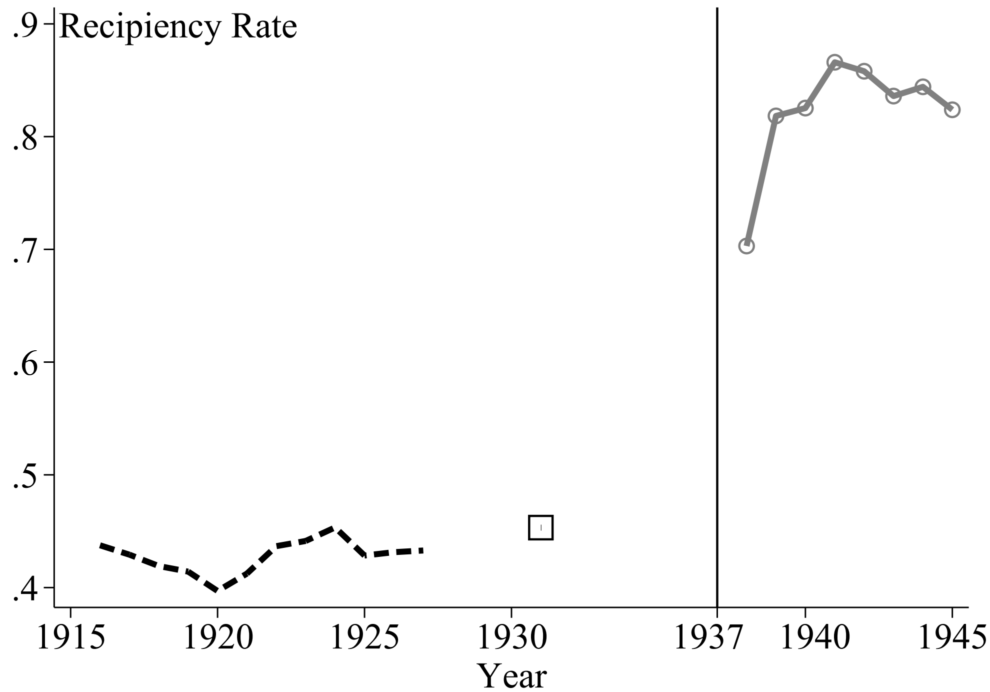
Notes: Plots the predicted number of individuals 65-80 who had worked for railroads between 1916 and 1945. Using the 1900, 1910, and 1920 full count censuses (Ruggles et al. 2021), I take the population of male railroad workers by age 49-64 and estimate populations in ensuing years using age-specific 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 (solid black line) when these first cohorts are 65-80, 1926 (dashed black line) for the 1920 prediction, and 1936 (solid gray line) for the 1930 prediction.

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



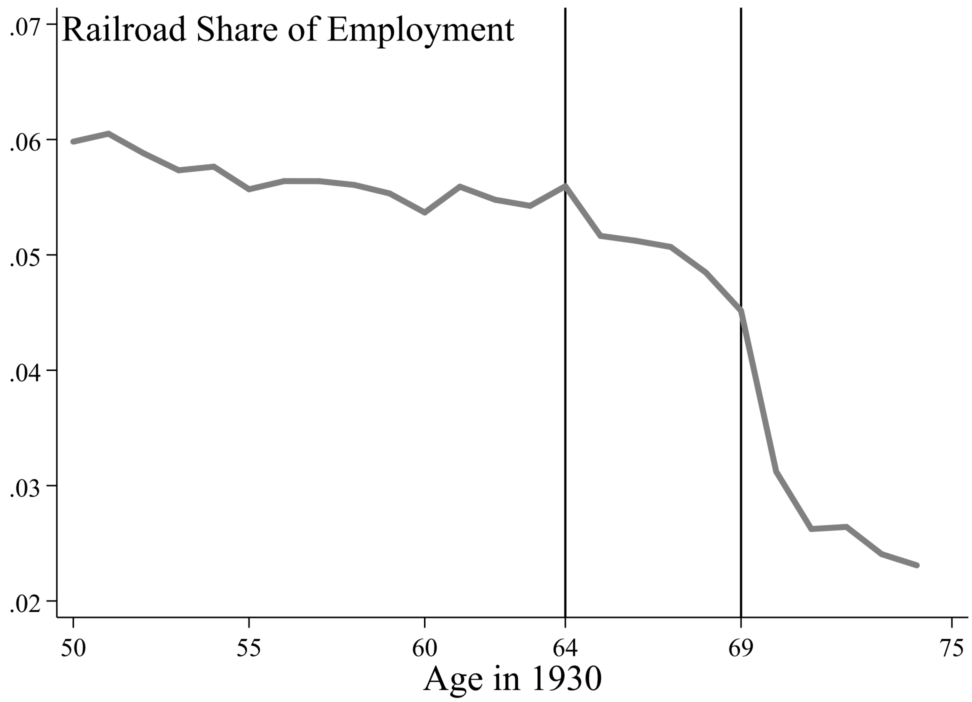
Notes: Plots the empirical probability density of retirement by age among all current "pensioners" in 1938 (solid black line) and decomposes the density into those retired under "age" provisions (dashed gray line) and under "disability" provisions (solid gray line); see footnote 23 for further details. Roughly 4.1 percent of pensioners claimed at ages outside of 50-74. Also see the notes to Figure 4 for more details. Data come from USRRB (1938, 99).

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 aged 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 for 1910-1927 is from a non-exhaustive set of reporting railroads (see notes to Figure 2 and Appendix B for recipiency sources and further details). 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 full 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.

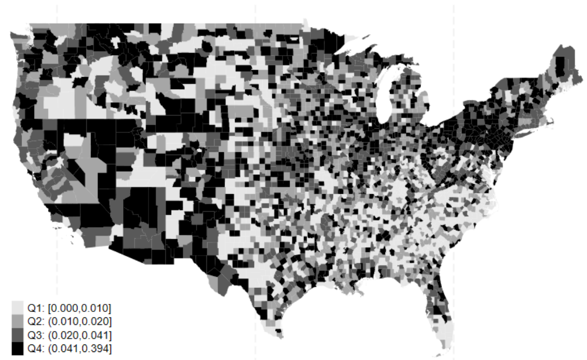
Figure A.4: Railroad Share of 1930 Employment



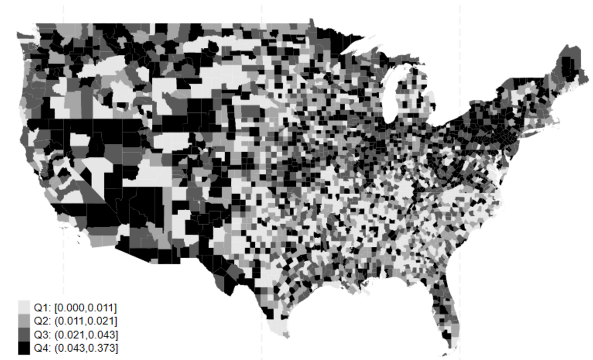
Notes: Plots the 1930 railroad share of all non-agricultural employment (all 1950 industry codes other than 105) by age, defining railroad employment as in Section 3 and Appendix B. Data are from the 1930 full count decennial census (Ruggles et al. 2021).

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

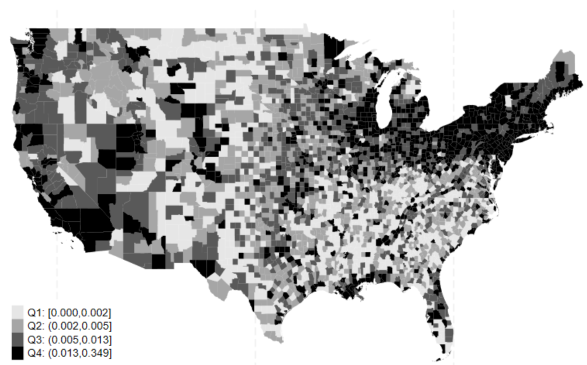
Panel A. Railroad 1920



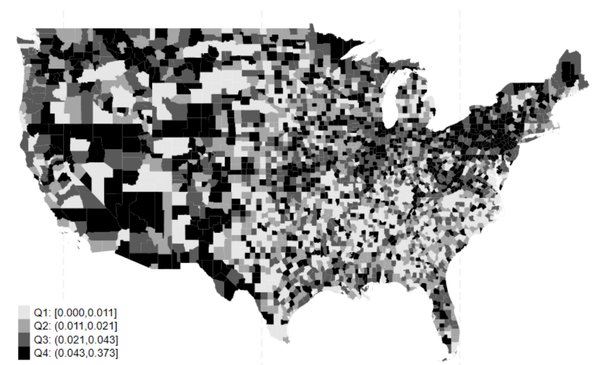
Panel B. Railroad 1930



Panel C. Control 1920

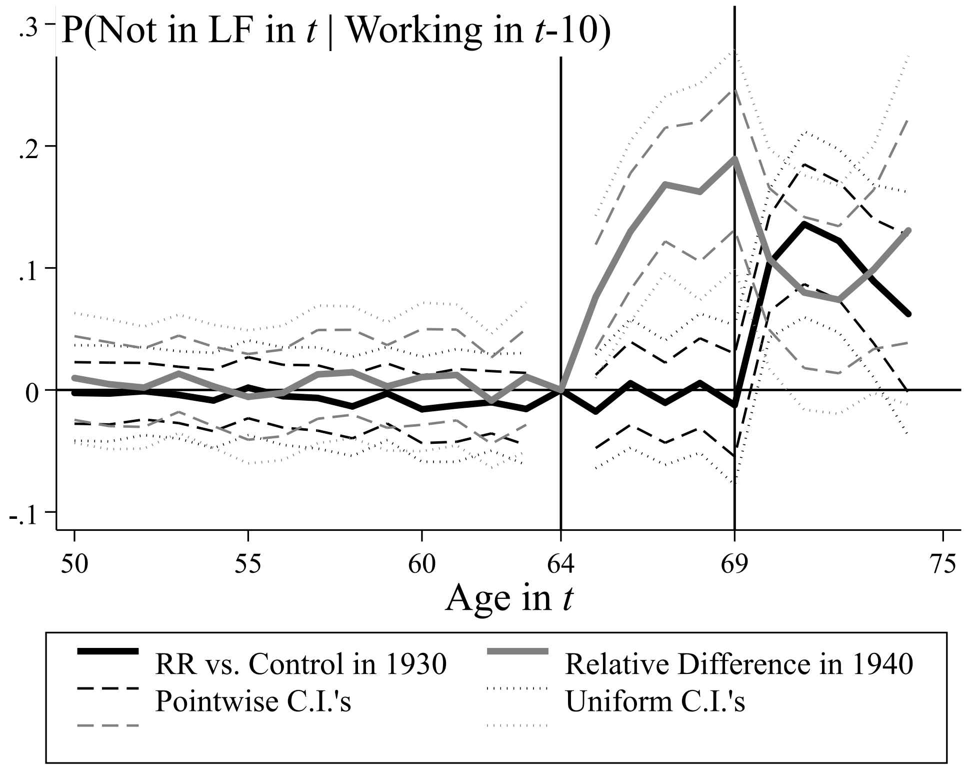


Panel D. Control 1930



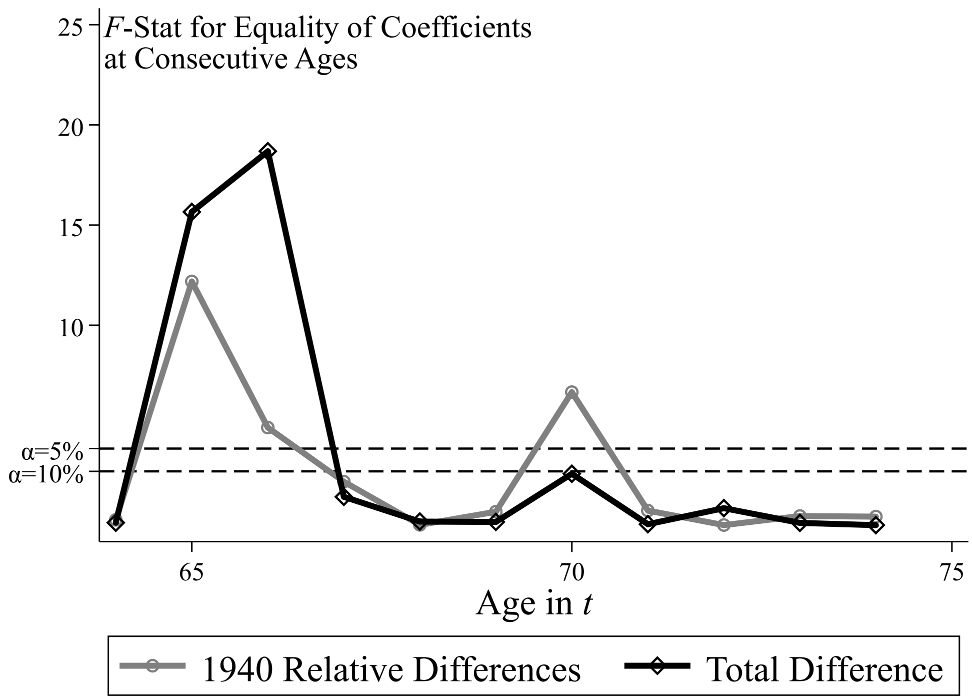
Notes: Plots the county share of employment ages 40-64 who were working on railroads (panels A and B) or industries I classified as covered by pensions (panels C and D) (see Section 3 and Appendix B for more details). Data are from the full count decennial censuses (Ruggles et al. 2021) for 1920 (Panels A and C) and 1930 (Panels B and D).

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



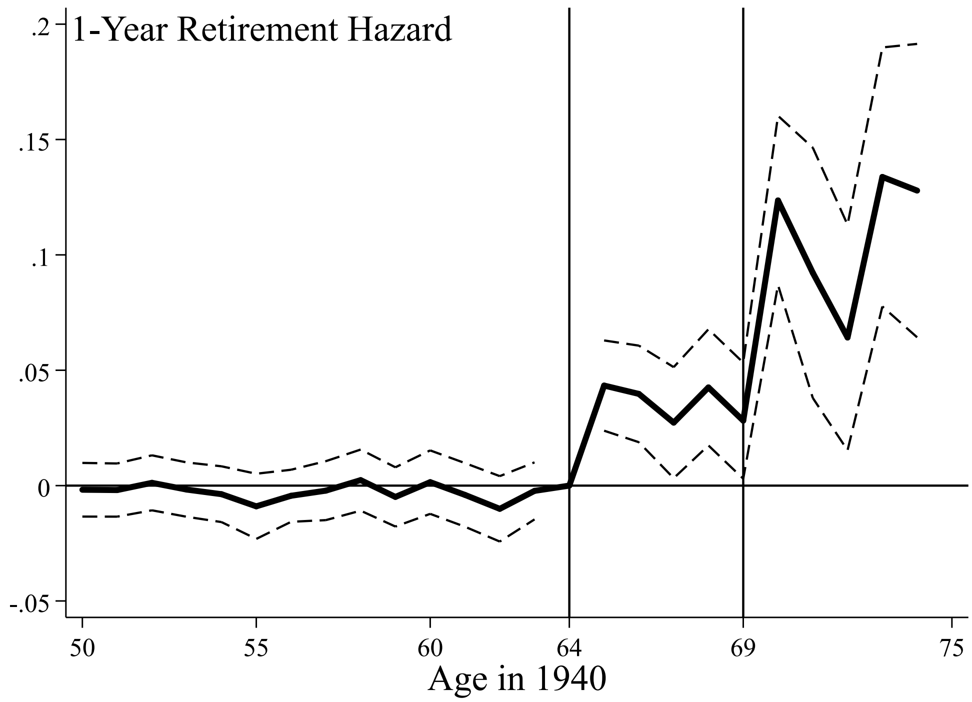
Notes: Plots estimates from specification (3) where the outcome indicates whether the individual was not in the labor force reweighted using weights generated to match the population at risk of being linked (see footnote 28). The solid black line plots the differences in labor force nonparticipation 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), estimated using the Stata user written command available here. Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

Figure A.7: F-tests of Equality for Consecutive Nonparticipation Coefficients by Age



Notes: Plots a series of F-statistics from tests of equality of consecutive coefficients for the point estimates from specification (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 Effect of the RRA on the Retirement Hazard

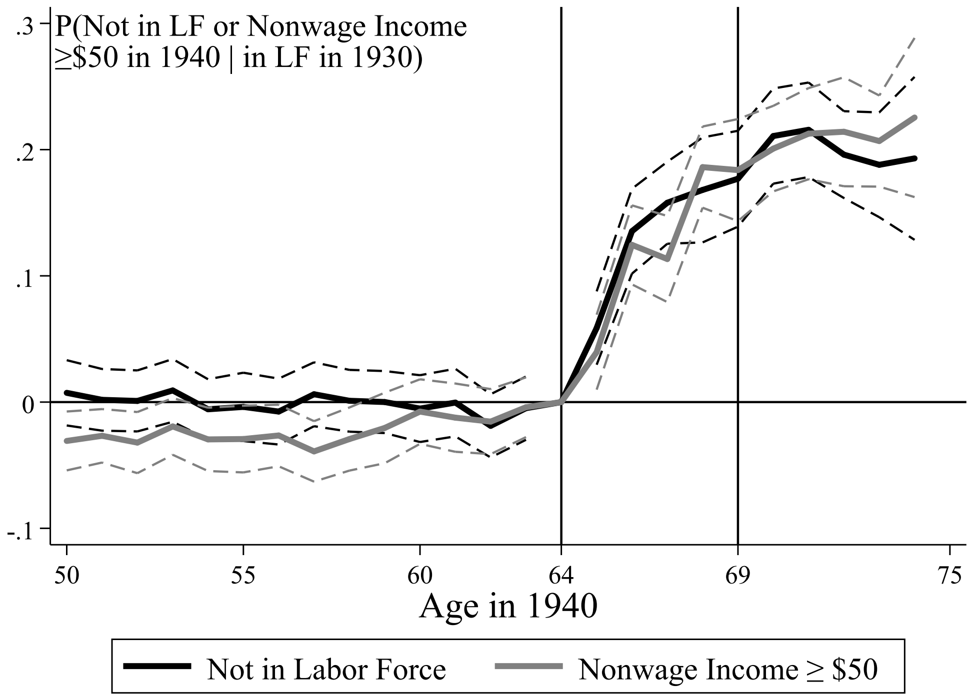


Notes: Plots estimates from specification (4) where the outcome indicates whether the individual was not in the labor force in 1940 but worked in 1939, so that estimates represent differences in the retirement hazard, or probability of retiring conditional on working last year. Estimates are reweighted using weights generated to match the population at risk of being linked (see footnote 28). Dashed lines are point-wise 95 percent confidence intervals, and standard errors are clustered at the 1930 county level. Sample is comprised of all male individuals aged 50-74 in the 1940 full 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 $t-10$), using the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details). The sample is further restricted to those who had positive weeks of employment in 1939.

Figure A.9: The Effect of the RRA Using Only Railroad Comparisons Over Time

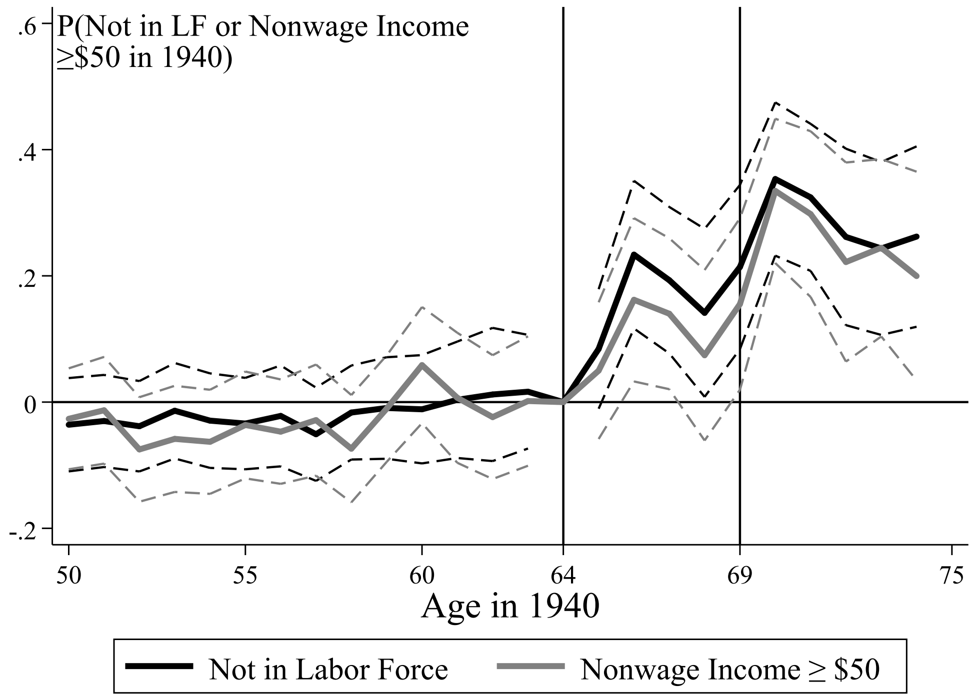
Notes: Plots estimates from a restricted version of specification (3) where the outcome indicates whether the individual was not in the labor force reweighted using weights generated to match the population at risk of being linked (see footnote 28). The solid black line plots the differences in labor 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. Sample is comprised of all male individuals aged 50-74 in the 1940 full count census (Ruggles et al. 2021) who were linked to the 1930 census, and who were working on railroads (in $t-10$), using the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

Figure A.10: The Effect of the RRA on Nonparticipation and Non-wage Income Receipt



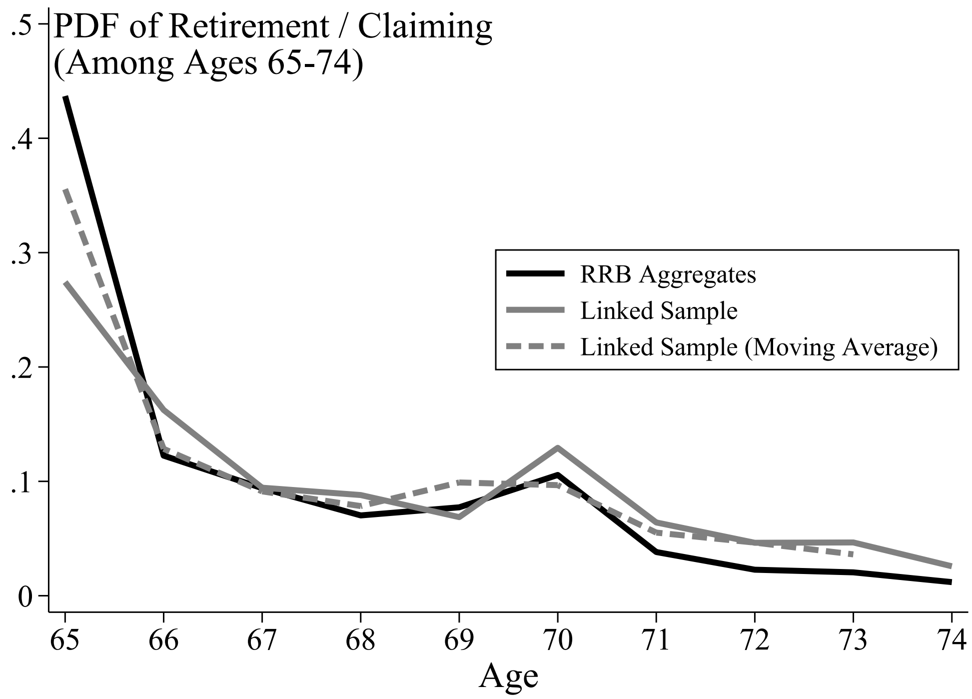
Notes: Plots estimates from specification (4) where the outcome indicates whether the individual was not in the labor force (black line) or an indicator for whether the individual received non-wage income in excess of $50 (nominal) in 1939 (gray line). Estimates are reweighted using weights generated to match the population at risk of being linked (see footnote 28). Dashed lines are point-wise 95 percent confidence intervals, and standard errors are clustered at the 1930 county level. Sample is comprised of all male individuals aged 50-74 in the 1940 full 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 $t-10$), using the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

Figure A.11: The Effect of the RRA on Nonparticipation Using Usual Industry and Occupation in 1940



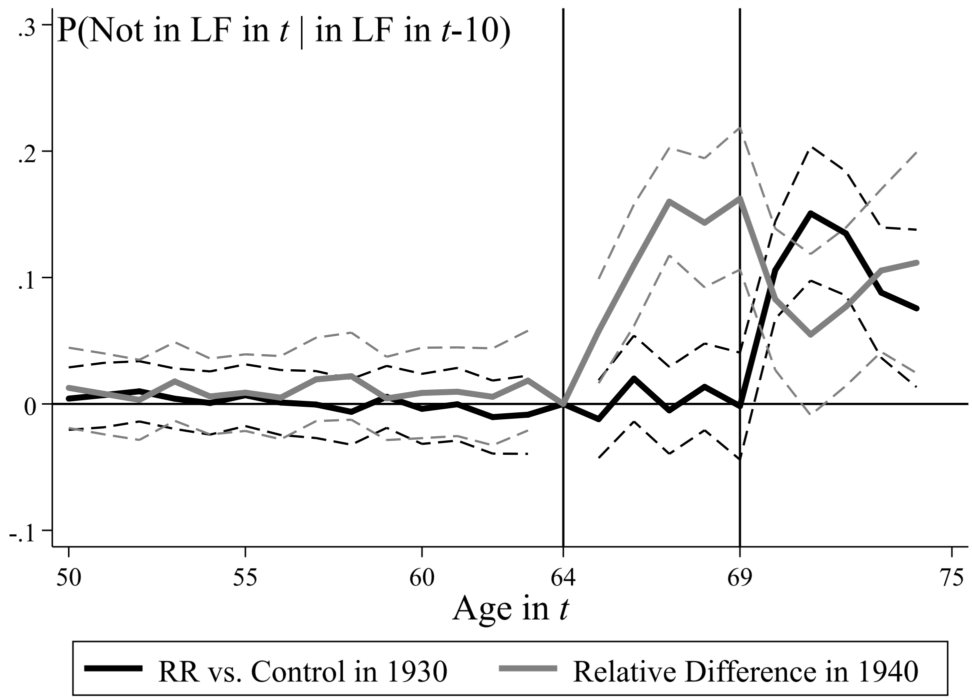
Notes: Plots estimates from specification (4) where the outcome indicates whether the individual was not in the labor force (black line) or an indicator for whether the individual received non-wage income in excess of $50 (nominal) in 1939 (gray line). Dashed lines are point-wise 95 percent confidence intervals, and standard errors are clustered at the 1930 county level. Sample is comprised of all men in the 1940 full count decennial census (Ruggles et al. 2021) aged 50-74 and either working on railroads or "control industries" as defined by their "usual" occupation and industry (see Section 3 and Appendix B for railroad and control definitions; see Section 5 for a discussion of usual occupation).

Figure A.12: 1940 Density of Railroad Pension Claims and Labor Force Exit



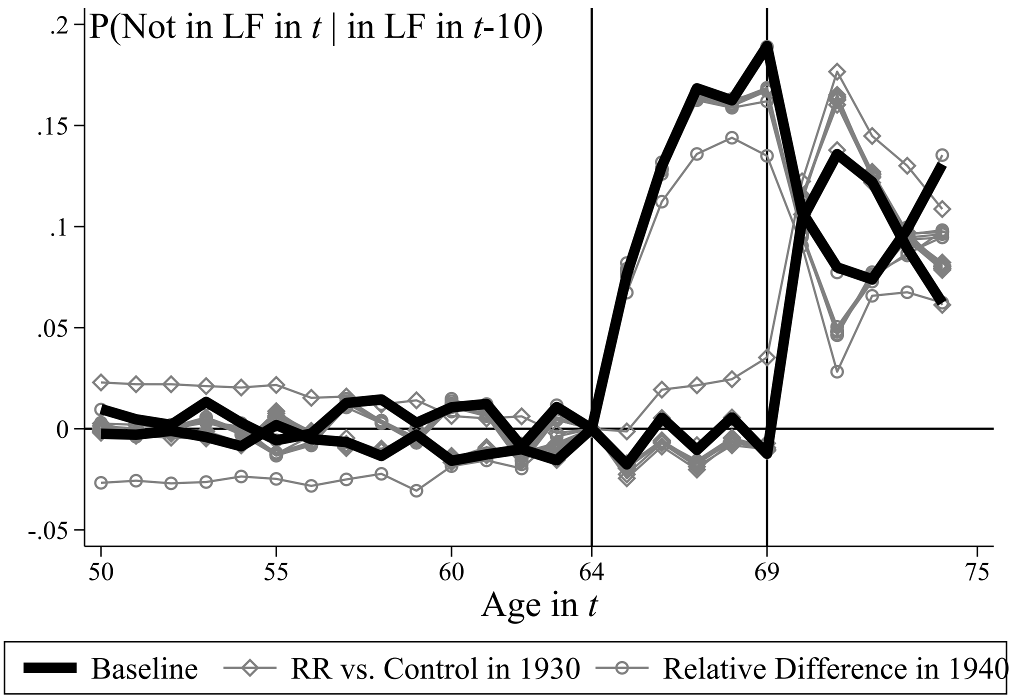
Notes: Plots the empirical probability density function of first benefit claiming age (solid black line) for post-RRA claimants in fiscal year 1940 conditional on ages 65-74(see notes to Figure 4 for further details). The solid gray line is the empirical probability density of the count by age reporting not being in the labor force for all railroad workers in the main analysis sample (see notes to Figure 6) aged 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 footnote 46). The dashed gray 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: The Effect of the RRA on Nonparticipation Measured Using Gainful Employment



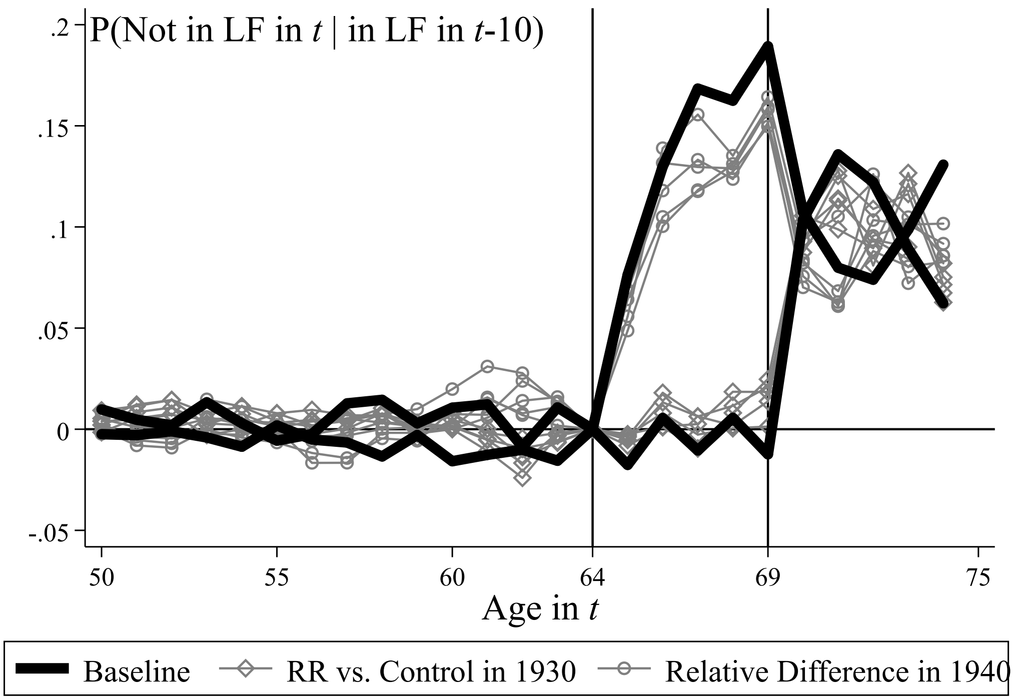
Notes: Plots estimates from specification (3) where the outcome is a consistent measure of no gainful employment between 1930-1940. This corresponds to 1950 occupation codes $\geq$ 980, excluding 985 and 990. Estimates are reweighted using weights generated to match the population at risk of being linked (see footnote 28). The solid black 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 $t-10$). Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

Figure A.14: The Effect of the RRA on Nonparticipation Using Various Specification Checks



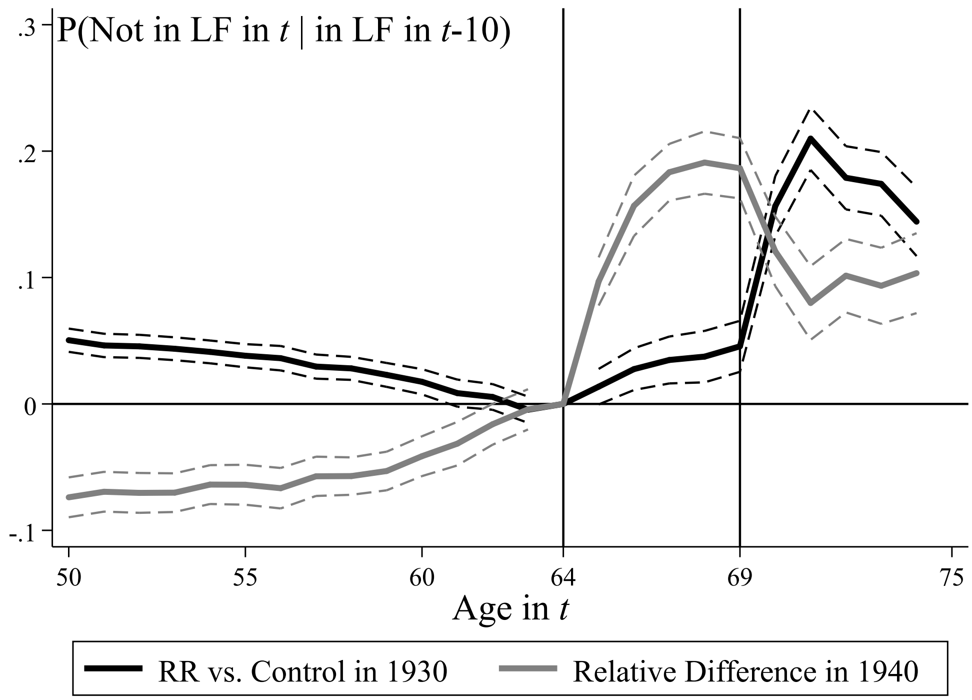
Notes: Plots estimates from nine variants of specification (3) where the outcome indicates whether the individual was not in the labor force. The variants are: 1 the baseline specification without the use of weights; 2 the baseline specification plus controls for race, number of children, and marital status (all in $t-10$); 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 solid black 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. Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

Figure A.15: The Effect of the RRA on Nonparticipation Using Various Linkage Algorithms



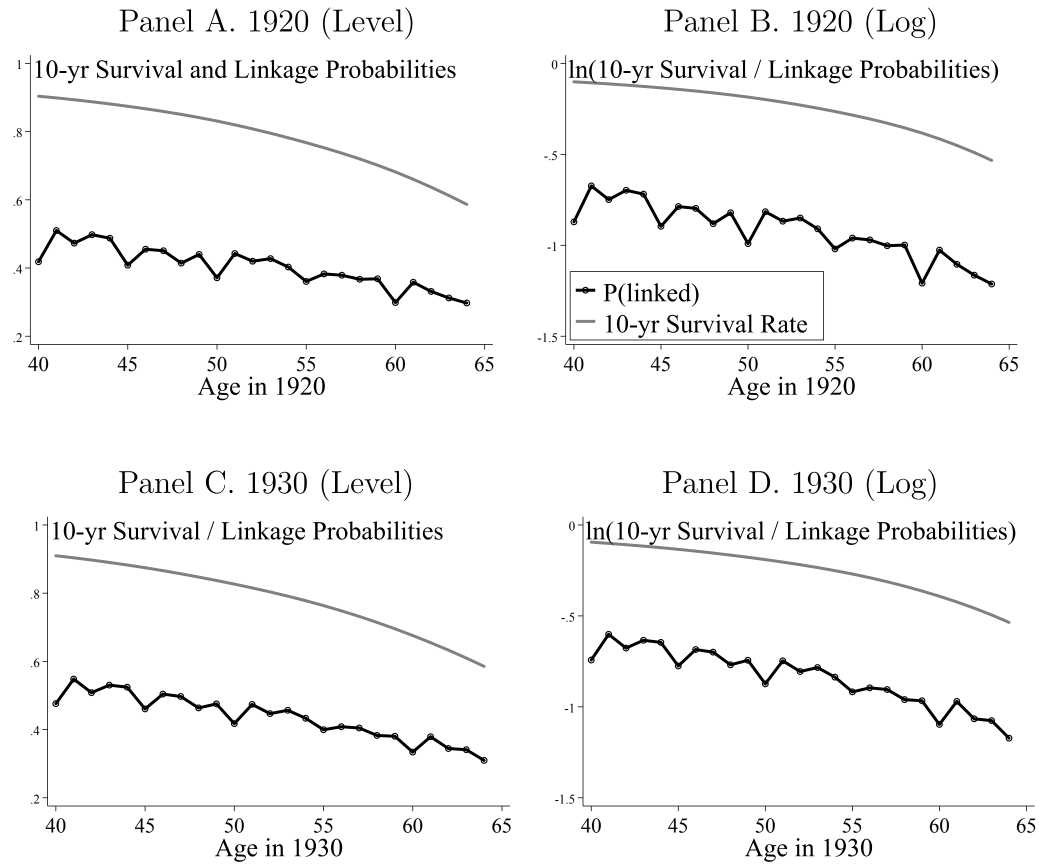
Notes: Plots five sets of estimates from specification (3) where the outcome indicates whether the individual was not in the labor force. The primary results using the sample formed from the linking algorithm provided by Helgertz et al. (2020) (Figure 6) are reproduced in black, with the gray lines representing estimates based on samples formed instead from the four available linkage techniques from (Abramitzky et al. 2020), as well as the links implied by their intersection. 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. Samples are each comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full count censuses (Ruggles et al. 2021), who were linked to the previous census (period $t-10$) using the above-described linkage algorithms, and who were working on railroads or industries I classified as covered by pensions (in $t-10$).

Figure A.16: The Effect of the RRA on Nonparticipation Using All Linked Non-Agricultural Workers as the Control Group



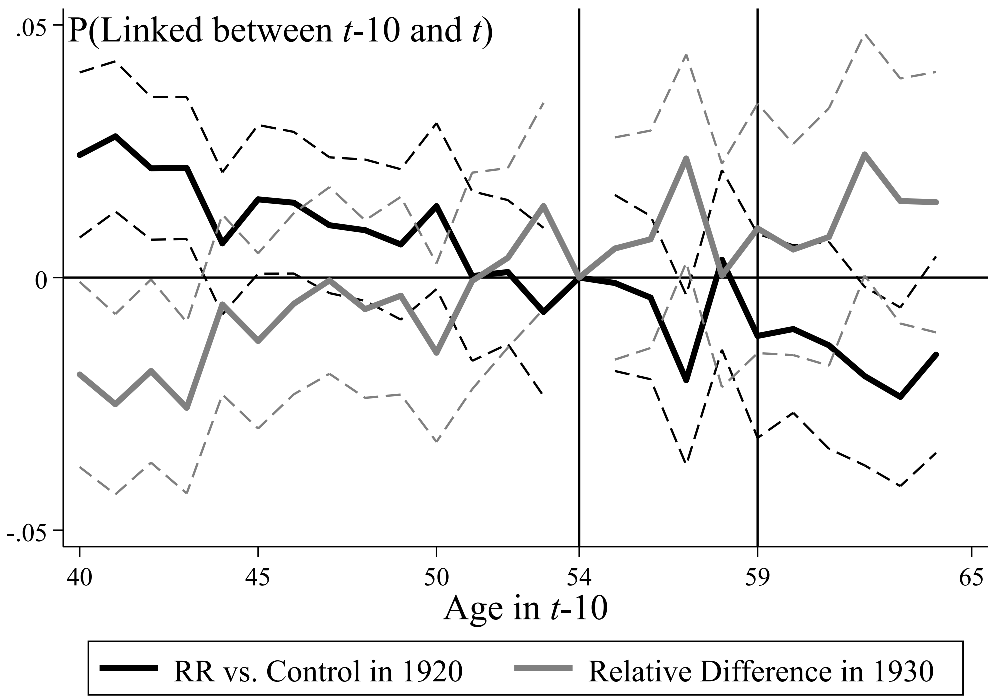
Notes: Plots estimates from specification (3) where the outcome indicates whether the individual was not in the labor force. The solid black line plots the differences in labor force nonparticipation between railroad and all other nonagricultural workers in 1930 ($\widehat{\pi}_{a(i)}$ under age 65; $\widehat{\rho}_{a(i)}$ 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. Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

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



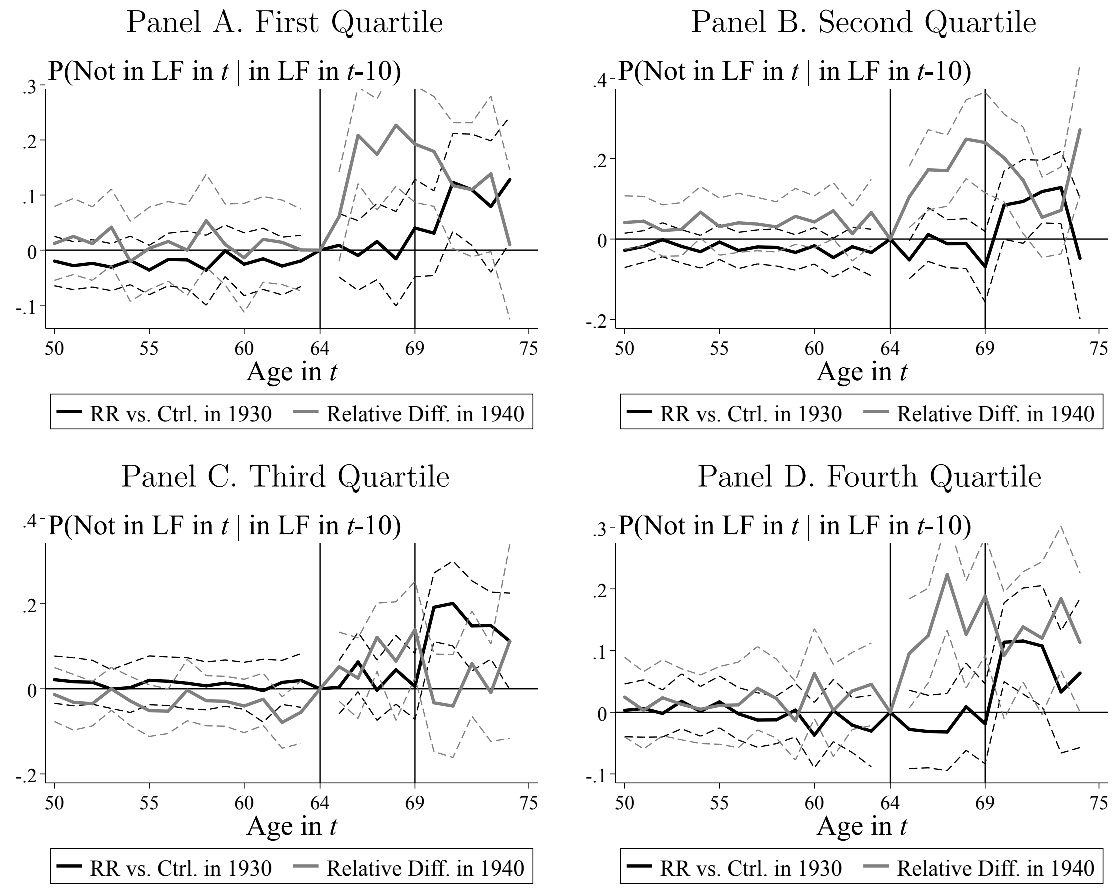
Notes: Plots age-specific 10-year survival probabilities (in gray) and probabilities of linkage to the next census (in black) for men ages 40-64 in 1920 (panels A and B) and in 1930 (panels C and D). These rates are presented in levels (panels A and C) and natural logs (panels B and D). Age-specific 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 full count censuses (Ruggles et al. 2021) who were working on railroads or industries I classified as covered by pensions. Linkage probabilities are derived using the linking algorithm provided by Helgertz et al. (2020).

Figure A.18: The Relationship Between Railroad Status, Year, Age, and Probability of Linkage



Notes: Plots estimates from a version of specification (3) where the outcome is contemporaneously measured and indicates being linked to the following census year, using the linking algorithm provided by Helgertz et al. (2020). Ages are given for the base-year with the omitted age being 54. The solid black 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. Sample is comprised of all male individuals ages 40-64 in the 1920 and 1930 full count censuses (Ruggles et al. 2021) who were working on railroads or industries I classified as covered by pensions.

Figure A.19: The Effect of the RRA on Nonparticipation by Quartile of the 1930 Unemployment Rate



Notes: Plots estimates from specification (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 footnote 28). Estimates are presented separately by quartile of the 1930 county-level unemployment rate for non-railroad male workers ages 40-64. The solid black 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 $t-10$). Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

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)

(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
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)

							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
Ann Arbor R.R.	1924	1,585	.001	.992	D						.01
Bessemer & Lake Erie RR.	1911	1,364	.001	.993	A,D,S	70		25		25	.01
Staten Island Rapid Transit Ry.	1917	1,364	.001	.994	A,D		65	35		10	.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							
Missouri-Illinois RR	1915	-	-	-	A,D	70		25		25	.01
Railway Express Agency, Inc.	1929	-	-	-	A,D,S	70		20	45	20	.01

Notes: Displays a digitized version of a Bureau of Railway Economics study in 1934 containing a subset of pension parameters for all private railroad pensions as of 1932 (reproduced in Railway Age 1934, 144-146), ordered according to 1928 employment (column 3). Column 1 gives the name of railroad firm or system (depending on the level for which the pension plan is specified); column 2 gives the year the plan was established; column 4 gives the 1928 employment density; column 5 gives the 1928 cumulative employment density; column 6 gives the type of pension plan provided: age (A), disability (D), and service (S); column 7 gives the compulsory retirement age (if the plan contained one); column 8 gives eligibility age for age-based pensions and column 9 the associated required minimum service; column 10 gives the eligibility age for disability-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. See Appendix B for further source details.

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: 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 footnote 28). See sample notes in Table 1.

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: 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 footnote 28. Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

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

	(1) Not Usually in Same Industry	(2) Difference Among Those Usually in Same Industry	(3) p-value
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: 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 footnote 28). Sample is comprised of all male individuals aged 50-74 in the 1940 full 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 $t-10$), using the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

Table A.5: The Effect of the RRA on Nonparticipation 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: Displays estimates from a summary version of specification (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 footnote 28). Every row drops an individual industry in the main control sample. Sample is comprised of all male individuals aged 50-74 in the 1930 and 1940 (period t) full 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 the linking algorithm provided by Helgertz et al. (2020) (See Section 3 and Appendix B for more details).

Table A.6: The Effect of the RRA on Nonparticipation 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: Displays estimates from a summary version of specification (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 footnote 28). See notes to Table A.5 with the alteration 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 by Whether they were Working 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)	150.8	64.7	8,887	191.1	67.8	7,119
1939 Monthly Wages: Full Time and Imputed (w_i)	143.4	67.2	1,376	190.1	71	1,040
Estimated Average Monthly Wage ($\overline{w}_i$)	122.5	60.2	1,376	164.7	63.8	1,040
Occupation Score	30.8	8.4	1,231	37	7.9	9,509
Own House	.64	.48	1,370	.7	.45	1,035
ln(House Value) | Own House	8.1	.87	8,273	8.3	.75	6,992
Demographic						
# of Children | Have Children	3	1.8	1,056	2.8	1.7	7,728
Have Children	.78	.42	1,376	.76	.44	1,040
White	.93	.17	1,376	.97	.11	1,040
Marital Status	.94	.24	1,376	.94	.23	1,040

Notes: Columns 1-3 provide statistics for individuals who reported working in non-railroad industries in 1910, while columns 4-6 present the same for those who did. The procedure for construction of 1939 wage variables is described in detail in Appendix C.3. Other economic variables, and all demographic variables, are from 1930. Averages are reweighted using weights generated by the procedure described in footnote 46. Sample is comprised of all male individuals 55-64 in the 1940 full 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 3 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 also successfully linked to the 1910 census using the "exact-conservative" algorithm provided by Abramitzky et al. (2020).

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)		
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)		
Any 1910 County	.135	.013	683
	(.049)		
Control Same Industry (1910 & 1930)	.239	.015	311
	(.09)		

Notes: Displays estimates from specification (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 first row reproduces the main estimate from Table 3; the second provides the estimates without weights; the next four rows use the other 3 linking algorithms provided by Abramitzky et al. (2020) as well as the links implied by their intersection; the next five rows present checks on the wage measure used to construct $\%\Delta B(\overline{w}_i)$, first using observed (and imputed) annual wages, then restricting to estimated average wages above \$100, above \$125, between \$125 and \$275, and between \$100 and \$300; the next row does not limit the sample to those railroad workers in the same county in 1910 and 1930; and the final row restricts the control group to those who also worked in the same industry code in 1910 as in 1930. See notes to Table 3 for sample construction.

B. Data Appendix

B.1. Matching 1928 Railroad Employment to Pension Plans

To order pension plans according to 1928 employment size, I use information from the 1928 Interstate Commerce Commission (ICC) Annual Report (USICC 1928) listed on pages CIII, 18, 19, 36, 37, 54, 55, 72, 73, 90, 91, 108, 109, 126, 127, 144, 145, 162, and 163, containing 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 of multiple firms 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. In these cases, I aggregate employment across firms within system.

Note that employment of the Pullman Co. is for the end of the 1928 calendar 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 a separate employment level provided for it (although it is reported as a separate pension). Hence, employment is omitted for these last two companies.

B.2. Constructing Railroad Pension Recipiency and Payment Time Series'

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

- Source: *Annual Report on the Statistics of Railways in the United States* (US-ICC, Various Years) for the following years and page numbers: 1930, S-31; 1931, S-63; 1932, S-69; 1933, S-76; 1934, S-82; 1935, S-84; 1936, S-76; 1937, S-91; 1938, S-83; 1939, 164; 1950, 94.

  Contains: Annual railroad pension expenditure from 1920-1945.

- Source: *Industrial Pension Systems in the United States and Canada* (Latimer 1932, 161).

  Contains: Annual private railroad pension recipiency from 1910-1929.

- Source: *Congressional Report* (US Senate 1934, 92).

  Contains: Private railroad pension recipiency and expenditure in 1925 and 1931.

- Source: *Historical Statistics of the United States Millenial Edition* (Carter et al. 2006, Series Bf746-761).

  Contains: Railroad retirement public pension recipiency and benefits paid from 1938-1945.

B.3. 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 circa 1930.

B.3.1. Workers Covered by the RRA

- Coverage Rules

Covered workers were those working for employers "constituting the national system of railroad transportation" (USRRB 1938, 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).

- Classification of RRA 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 full count census. I base classifications predominantly on industry codes, with use of occupation codes as a secondary method that ends up contributing a relatively insignificant share of the total number of included workers.

Table B.1 reproduces a table from USRRB (1941, 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 IPUMS 1950 harmonized set of codes) for railroads and discuss 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 during the 1940 census reference period.

a. Industry Code 506: Railroads and railway express service

The primary 1950 IPUMS industry code for railroad workers is 506. Employment in the 1940 full 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 this industry code 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 (USRRB 1938, 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 coveragey, 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.

- *Summary*

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. I use these codes to classify railroad workers in both linked sample base years (1920 and 1930).

B.3.2. 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 full count census by industry. I then use these codes when including workers in the control group in both linked sample base year (1920 and 1930).

- *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 which, given my use of full count censuses, is unimportant. I show results are not sensitive to broadening or restricting the set of control industries in Section 5.

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 as universal in any other industry as it was in the railroad industry, 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 the Latimer (1932) study.

I adopt the following procedure to choose which industry codes to include: First, I match the pension definitions in Table B.2 to comparable industries in the 1930 census; Second, I 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; Third, I choose a tolerance for the ratio of at most 3 or at least 1/3, and keep those industries satisfying this criteria. Table B.3 shows that this leaves utility industries and a subset of manufacturing.

- Summarizing Included Industries as Controls

a. Utilities

This procedure matches public utilities particularly 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 all Non-Railroad Pension-Covered 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 covered 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 non-railroad 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 USRRB (1941, 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 among firms that had pensions. Data are from Latimer (1932, 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 and greater than 1/3.

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

C.1. Details of Railroad Retirement Benefits

This section adds to Section 2 further details of railroad retirement benefits under the 1937 RRA. This section draws heavily from USRRB (1937) Section III.A.

- *Eligibility*

Appendix B describes which types of employers and their workers were covered under the RRA. To receive credit for work prior to the RRA in determination of pension benefit amount, 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 in 1940 (Figure 4, Panel A)– more in Appendix C.2 below.

[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 1910 for the elasticity analysis).

- *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 an 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 for workers from each railroad firm. They recognized that unions had negotiated with railroads to cut wages by 10 percent beginning in 1932 with only "promises [to unions] to provide new jobs and preserve old ones" (Graebner 1980, 156). Possibly 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 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 the computation if it increased the average wage.

- *Benefit Formula*

Most of the important rules governing how benefits were computed are described in Section 3.2.2. 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 any month in which individuals performed "compensated service to any person or company whether or not an employer under the [RRA]" (USRRB 1937, 18). While restrictive, this type of earnings test was used on most private railroad pensions (see USRRB 1937, 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 nonparticipation (zero wages) as a condition for pension recipiency.

[2]The minimum benefit rule is as follows: If $\overline{w}_i \geq \$50$, the benefit is \$40 unless the normal annuity computation would have been more; if $\overline{w}_i \in [\$25, \$50)$, the benefit is 80 percent of $\overline{w}_i$; if $\overline{w}_i \in [\$20, \$25)$, the benefit is \$20; and if $\overline{w}_i < \$20$, the benefit is $\overline{w}_i$. Further, the minimum was always at least as high as the minimum legislated Social Security 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 (USRRB 1938, 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.

C.2. Forward Looking Models of Pension Incentives and Retirement

This 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 percentage change. 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 of Section 6, the focus of this exposition is for those who would have been eligible at 65 under private plans. I first discuss a set of simplifying assumptions that are in part motivated by the structure of benefits and patterns of wage growth over the age-profile. I then show that under these assumptions, the percentage change to annual benefits is equivalent to the percentage change to pension wealth. The last part of the section describes and simulates an Option Value model to develop a set of predictions for how the RRA should impact retirement timing.

C.2.1. 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 focal retirement age (e.g., the Social Security early retirement age), and also have benefit factors $k(\overline{w}_i)$ that depend on retirement age (e.g., reduced Social Security benefits for claiming before the full retirement age or delayed retirement credits if claiming after the full retirement age). Hence, $\overline{w}_i, S_i$ and $k(\overline{w}_i)$ are each functions of retirement date.[4] In contrast, railroad retirement benefits featured no actuarial adjustments increasing benefits for delaying retirement past 65, as well as no further credited service after 65 (see Appendix C.1 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.*

I assume this case away for the below simulation. To provide evidence that wages are, on average, declining at these ages, I conduct a series of regressions of 1939 wages (among railroad workers in the 1940 census) on age, age-squared, and in some specifications occupation fixed effects, which all place the predicted peak wage between ages 57-60.[5] 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.3 for further details). Predicted average wages are somewhat lower than earnings at age 65 or ages slightly above, but the difference, and how it would affect the average wage and benefit amount, is negligible.[6] Further, recent work shows that the these types of intertemporal substitution incentives induce very little retirement response, even in

[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]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. Since potential future wages are never observed if an individual retires, studies typically make assumptions of constant positive wage growth (e.g., Coile and Gruber 2007), which does not appear to match this setting well.

[6]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.

contexts in which changes to substitution incentives are quite large (Gelber et al. 2016).

Assumption 2: *No Early Retirement*

As noted above, early retirement was allowed on a reduced basis at 60, but it appears to not have been taken up by many. Further, the cohorts studied in the elasticity analysis of Section 6 are 65 and older in 1940 or already 62 and above when benefits became broadly available in 1937, indicating they had at best a constrained choice over retiring early. I therefore abstract away from considering early retirement.[7]

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

Almost all of the pre-RRA private 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.[8] Rather than introduce further imputations than I do below in Appendix C.3, my preference is to assume that average wages would have been the same as that calculated under the RRA. What matters is what individuals expected their average wage to be; on the one hand, had pensions continued to be private, individuals who turned 65 in 1940 would have had reduced average wages due to the Depression (see Figure C.3 below). On the other-hand, the decade

[7] While inherently speculative, the absence of much early retirement density may be because this paper focuses on workers with good earnings prospects, as modern evidence indicates negative selection into early retirement (Li et al. 2008).

[8] I thank George Alter and Sam Williamson for assuring me this was indeed the case for the Pennsylvania Railroad pension system.

preceding age 65 surrounds peak-wage ages, which may indicate they expected somewhat higher average wages relative to what they earned between 1924-1931. Either way, it is likely that average wages as calculated under private plans and under the RRA would be highly correlated.

C.2.2. Equivalence of Growth in Annual Benefits and 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 (e.g., 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 $B(\overline{w}_i, S_i)$ 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 and can be factored out of the sum. Therefore, assuming that $\delta_{s|t}$ and r are constant – a reasonable assumption given the unexpected and quick nature of the RRA – the percentage change to monthly benefits $\%\Delta B(\overline{w}_i)$ defined in Section 3.2.4 is equivalent to the percentage change in pension wealth.[9]

[9] 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).

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 $\text{PVPW}_t(\overline{w}_i, S_i)$ is maximized for the great majority of workers under the RRA at age 65 (under the above assumptions $\text{PVPW}_t(\overline{w}_i, S_i)$ is maximized at age 65 for *all* workers). 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 measure 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 the second prediction is likely true, but the remaining density at ages above 65 indicates the first cannot be. Perhaps more importantly, since fixed benefit levels at age 65 imply the Peak Value is highest at age 65 regardless of $\overline{w}_i$, the framework leaves little scope for retirement date to depend on benefit levels.

C.2.3. Option Value Model

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 commonly studied case of Constant Relative Risk Aversion, 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 ($\kappa_i > 1$) which is allowed to vary arbitrarily across individuals. The optimal retirement date is the date R^* that maximizes $V(R)$ subject to $R \leq 70$: $R^* = \max \left\{ \underset{R}{\operatorname{argmax}}\{V(R)\}, 70 \right\}$, 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 ($w_i = 1.11\overline{w}_i$).[10] 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 for an individual who was eligible for pre-RRA benefits at age 65. The representative individual who is 62 in 1937 is considering whether to retire at 65 in 1940 or to keep working past 1940, which is the primary cohort of interest studied in Section 6.[11]

Panel A of Figure C.1 plots R^* according to the pre-RRA benefit formula (Figure 3), with the darker shade indicating $R^* = 65$ and lighter shade $R^* = 70$. The

[10] This is the ratio of wages in 1939 to predicted average wages for an individual who is 65 in 1940 (see Appendix C.3 below). 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 earlier, but in practice is quite similar.

[11] None of the conclusions presented in this section change if I instead assume the shock occurs when the original RRA was passed, or when individuals aged 65 in 1940 were age 59.

conclusion regarding timing of pre-RRA 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 predicts no effect of the level of wage on retirement timing before the RRA (holding κ_i fixed). In other words, because benefits are not progressive, the replacement rate is constant across the wage distribution (see footnote 16), and so timing does not vary by $\overline{w}_i$ conditional on κ_i.

In contrast, Panel B shows that optimal retirement timing under newly progressive benefits is highly variable in $\overline{w}_i$; 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 will produce 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 (see discussion in footnote 22), comparisons of how individuals respond across wages will be biased upward. This illustrates the econometric issue in estimating the relationship between 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 factors affecting labor supply (Moffitt 1987; Krueger and Meyer 2002; Coile 2015).

A further conclusion to come from the model, untestable with the data at hand, is that replacement rates under private pensions appear to have been too low to rationalize retirement before age 70 at reasonable levels of disutility. Also, under the simplifying assumptions, the Option Value does not predict any of the *observed* density at ages 66-69 after the RRA (Figure 4, Panel A). For a worker to find it optimal to retire at these ages, 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 footnote 20), adjustment frictions (Manoli and Weber 2016), and the constrained choice set over retirement age available to older cohorts when the RRA went into effect.

C.3. Constructing Average Wages

I construct $\overline{w}_i$ using three sources of information: direct observation of 1939 wages in the elasticity 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 full count census.

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

The use of individual level wages is crucial to define the percentage 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 also include workers who did not work the full year and interpolate their annual wage by simply multiplying the wages they earned 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 occupational income 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 wages by dividing by 12.

- Imputing Previous Earnings

I next use the 1939 age-profile of railroad wages– derived from the full count 1940 census – to back-cast wages to earlier ages. Figure C.2 plots average wages for ages 18-64 (black solid 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, the predicted maximum wage is achieved at age

60.7. As described above in Appendix C.1, 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 and older 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.

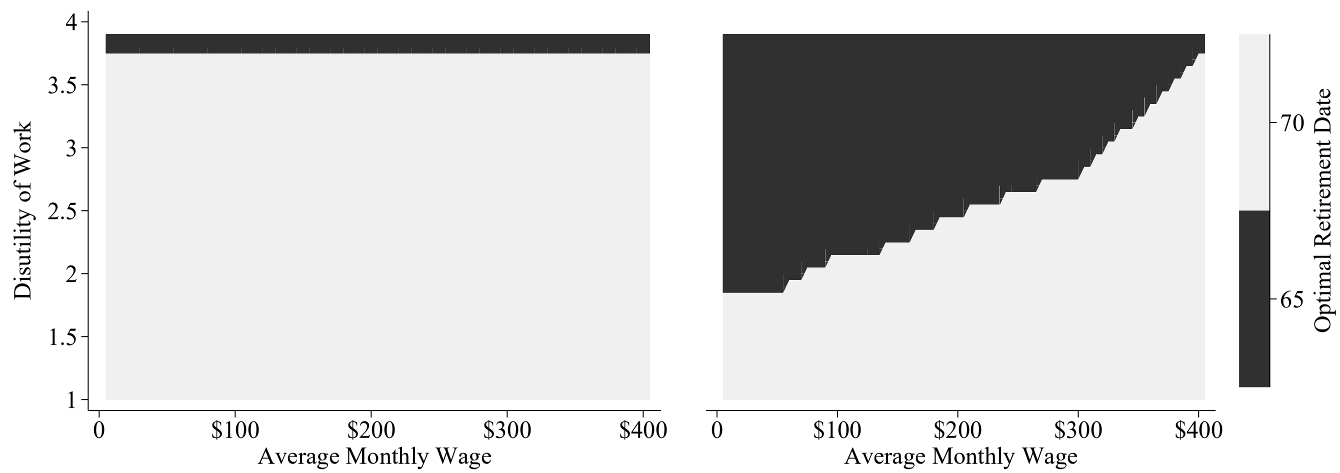
- Nominal Wage Growth

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 annual railroad wages (USICC 1930, 24; 1940, 74).[12] 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 the ratio of average wages in each year relative to 1939 to adjust down the age-adjusted previous wages described above. Finally, because I restrict the sample 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 wages for 1937, 1938, and 1939, and dividing by 30.

[12] There are more details on 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 measure of annual nominal railroad wage growth drawn from a consistent sample definition, rather than use more detailed information in some years drawn from different samples over time.

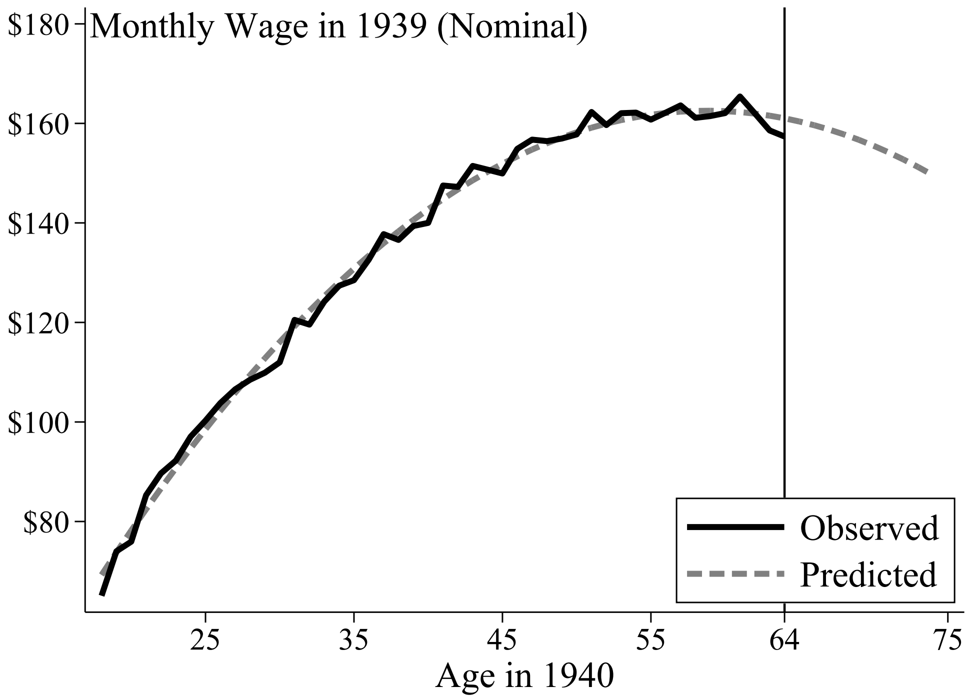
Figure C.1: Optimal Retirement Timing Pre and Post-RRA

Panel A. Pre-RRA Optimal Retirement Ages   Panel B. Post-RRA Optimal Retirement Ages



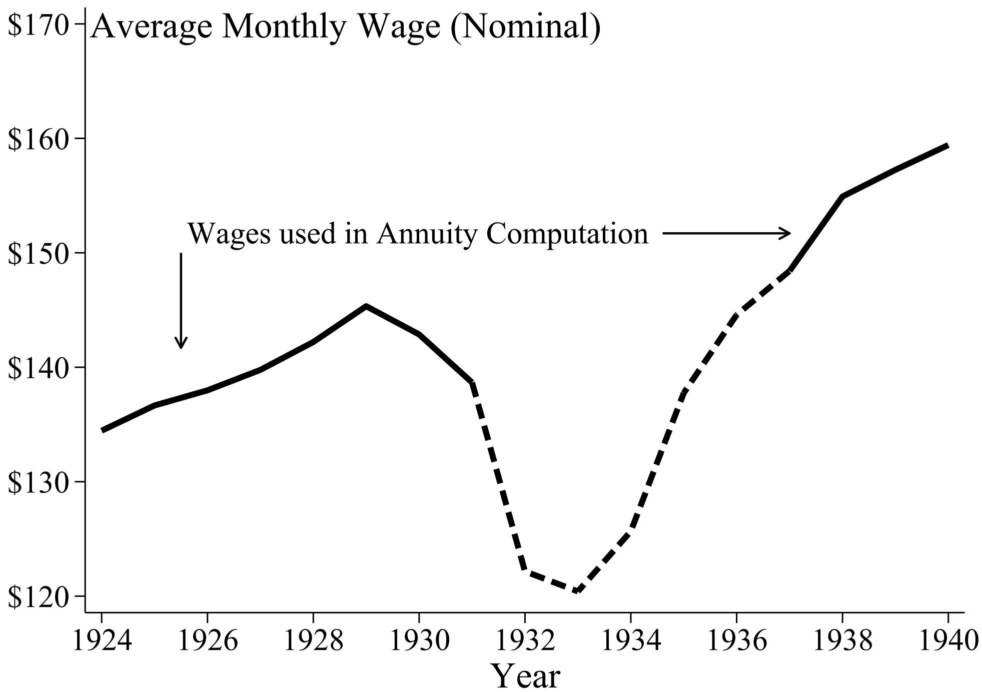
Notes: Panel A plots the results from simulating the Option Value model given various average wage ($\overline{w}_i$; x-axis) and disutility of work (κ_i; y-axis) pairs for workers under a typical pre-RRA private railroad pension benefit formula (see online Appendix Table A.1) who would have been eligible for benefits at age 65. The lighter shade refers to an optimal retirement age of 70, while the darker shade refers to an optimal retirement age of 65. Panel B plots the same for benefits under the RRA.

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



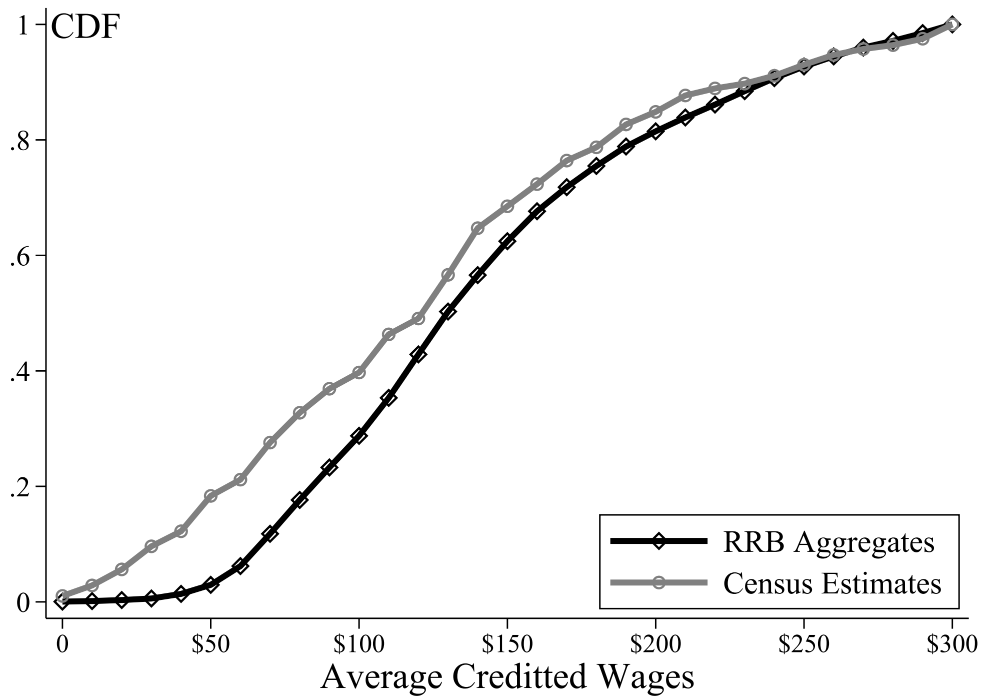
Notes: Plots average wages by age (solid black line) and predicted wages (dashed gray line) from a regression of wages on age and age squared, predicted out of sample for ages 65-74. Sample is all male individuals ages 18-64 in the 1940 full count decennial census (Ruggles et al. 2021) working for railroads according to the classification described in Section 3 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.

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 (USICC 1930, 24; USICC 1940, 74).

Figure C.4: Comparing the Distribution of Average Wages Between the Census and 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 are estimated from taking the difference of claimant counts from RRB information on average wages among recipients in force as of fiscal year 1940 (USRRB 1940, 267) and as of fiscal year 1939 (USRRB 1939, 101). Average wages in the census are calculated according to the procedure described in Appendix C.3 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.

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