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GOVERNMENT OLD-AGE SUPPORT AND LABOR SUPPLY: EVIDENCE FROM THE OLD AGE ASSISTANCE PROGRAM

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ABSTRACT

Many major government programs transfer resources to older people and implicitly or explicitly tax their labor. In this paper, we shed new light on the labor supply effects of such programs by investigating the Old Age Assistance Program (OAA), a means-tested and state-administered pension program created by the Social Security Act of 1935. Using newly available Census data on the entire US population in 1940, we exploit the large differences in OAA programs across states to estimate the labor supply effects of OAA. Our estimates imply that OAA reduced the labor force participation rate among men aged 65-74 by 5.7 percentage points, nearly half of its 1930-40 decline. Estimating a structural model of labor supply, we find that the welfare costs to recipients of the high tax rates implicit in OAA's earnings test were quite small. Predictions based on our reduced-form estimates and our estimated model both suggest that Social Security could account for at least half of the large decline in late-life work from 1940 to 1960.

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1 Introduction

Many of the most important government programs—including Social Security and Medicare—transfer resources to older people and tax their labor relative to that of younger people.[1] Standard economic theory predicts that such programs reduce late-life labor supply. Understanding the size and nature of such effects on labor supply is an increasingly important issue, as demographic trends have increased both the potential labor supply of the elderly and its aggregate importance, while simultaneously increasing the need for reforms to government old-age support programs. This raises three important questions. What are the effects of government old-age support programs on late-life labor supply? What is the relative importance of the two key features of these programs—the transfers to older people and the taxation of their labor—in determining these effects? And to the extent that taxation of labor is important, how large are the associated welfare costs to recipients?

We address these questions by investigating Old Age Assistance (OAA), a means-tested program introduced in the 1930s alongside Social Security. OAA was large both in absolute terms—22 percent of people 65 and over received OAA in 1940—and relative to Social Security, which made no regular payments until 1940 and remained smaller than OAA until the 1950s. Like important social insurance programs of the present day, OAA both increased non-labor income and, through an earnings test, implicitly taxed work for older people. Yet unlike Social Security and other social insurance programs that are national in scope and near-universal in coverage, OAA was state-administered and exhibited considerable variation across states in eligibility and benefit levels. This provides empirical leverage that is seldom available in more recent periods, providing an unusual opportunity to learn about the effects of these programs.[2]

The particular setting we study is of special interest because it enables us to shed new light on the extent to which the introduction and expansions of OAA and Social Security contributed to the large decline in labor force participation among older men over the 20th century. Figure 1 illustrates these trends during the early expansions of these programs, from 1920 through 1970.[3] As we will discuss further, the striking time-series correlation

[1]Although Social Security has gradually reduced the extent to which it taxes late-life work, it imposed a strong earnings test for much of its history and continues to tax the late-life work of many people through its tax and benefits formulas today (see, e.g., Goda, Shoven and Slavov, 2009; Gelber, Jones and Sacks, 2013). Medicare's secondary payer status (Goda, Shoven and Slavov, 2007) and Medicaid's means-testing rules mean that they implicitly tax late-life work at significant rates as well.

[2]The difference in pension and disability programs between Quebec and the rest of Canada is a notable exception (Baker and Benjamin, 1999; Gruber, 2000).

[3]The entire increase in combined OAA and Social Security payments is due to OAA up to 1940, and all of the increase after 1950 is due to Social Security. Gruber (2013) shows a similar graph from 1959 to 2009 that exhibits the same inverse relationship between Social Security spending and labor force participation.

between the expansion of the Social Security program after 1950 and declining labor force participation is often noted in discussions of Social Security and retirement (for example, Feldstein and Liebman, 2002; Krueger and Meyer, 2002; Gruber, 2013; Coile, 2015). But as the same authors note, there is still significant uncertainty about the causal relationship between the two trends.

Our analysis takes advantage of recently-released data on the entire US population at this time from the 1940 US Census. Two advantages of this dataset over previously available data are its large sample size (over 6 million men aged 55–74) and its precise geographic information. The rare combination of large policy variation and a large dataset enables us to perform a wide range of empirical tests of the effects of OAA on labor supply. Our main empirical tests make use of two sources of variation. The first is the age eligibility requirement that existed in all states, almost always limiting eligibility for OAA to individuals 65 or older. Importantly, other modern-day programs that use age 65 as a cutoff, including Social Security, were either small or non-existent at the time. The second source of variation is cross-state variation in payment and eligibility levels of OAA programs. The empirical analysis tests whether there is a differentially large reduction in labor force participation after age 65 in states with larger OAA programs relative to states with smaller programs.[4]

Our estimates indicate that OAA significantly reduced labor force participation among older individuals. The basic patterns that we explore in the data are evident in Figure 2, which plots male labor force participation by age, separately for states with above- and below-median OAA payments per person 65 and older. Up to age 65, the age pattern of labor force participation was extremely similar in states with larger and smaller OAA programs. At age 65, however, there was a sharp divergence in labor force participation between states with larger OAA programs relative to those with smaller programs, and this divergence continued at older ages. Our regression results, which isolate variation in OAA program size due to state policy differences, imply that raising state OAA payments per person 65 and older by one standard deviation would have led to a roughly 3.3 percentage point decline in labor force participation among men aged 65–74. These results imply that OAA can explain close to half of the large 1930–40 drop in labor force participation of men aged 65–74.

We estimate a variety of alternative specifications, all of which support an interpretation of these results as the effect of OAA on labor force participation. For example, we show

[4]For this analysis, our main measure of the size of OAA programs is total OAA payments per person 65 and older. In using this measure, our empirical approach has two features that attempt to isolate policy variation, and in particular to address the reverse causality concerns that would normally arise: (1) in calculating the policy measure for a given individual we exclude his own county, and (2) we restrict comparisons to sufficiently narrow geographic areas that population characteristics and any aggregate shocks should be similar. We discuss the reasons for preferring this measure in Section 4 and show in Section 5 that using alternative measures that rely solely on policy variation does not significantly change the results.

that when we restrict the sample to non-US citizens—who were eligible for OAA in some states but not others—we find similar reductions in labor force participation after age 65 in states in which non-citizens were eligible for OAA, but we can reject comparable reductions in states in which they were ineligible. As another placebo test, we document that in 1930, prior to the passage of OAA, states that would have higher OAA payments in 1940 did not have differentially large reductions in labor force participation after age 65.

Although we find large effects of OAA on labor force participation, variants on our baseline estimates suggest that the welfare cost to recipients of OAA's high implicit tax rate on work was fairly small. An important share of the reduction in labor supply from OAA came from men with poor labor market prospects: between one-fifth and one-quarter of the reduction in labor force participation was due to exit from unemployment, and about one-fifth was due to exit from employment in work relief programs that were targeted at individuals who would otherwise be likely to be unemployed. In addition, the effects of OAA were concentrated among men with low levels of education and, in particular, men with low earnings before receiving OAA.

In order to better understand the effects of OAA on labor supply and its value to recipients, and to shed light on the broader question of how government old-age support programs affected late-life work during the middle of the 20th century, we use our findings on OAA to estimate a model of lifetime labor supply and retirement. Estimation of the model requires two key inputs. First, it requires estimates of how the effect of OAA varied across individuals with different earning opportunities, which we obtain by measuring changes in the earnings distribution at the OAA eligibility age. Second, it requires an estimate of the latent distribution of retirement ages that would arise in the absence of any old-age support programs, which we obtain using the same cross-state variation underlying our reduced-form specifications. This latent retirement distribution is an important determinant of the effects of government old-age support programs and other policies that create non-linearities in the lifetime budget constraint (Moffitt, 1986). Since there were few sources of government old-age support in 1940 other than OAA, and since private and government employee pensions covered only a small share of the population, our setting provides a rare opportunity to estimate this latent distribution using quasi-experimental variation.

Standard economic theory implies that the ex-post value of OAA benefits to recipients was weakly less than their budgetary cost, since recipients may have adjusted their behavior in response to the implicit taxation of earnings from OAA's earnings test. As a reference point for our model-based estimates, we first use our reduced-form estimates to bound the costs to recipients of meeting the earnings test. These bounds are based on the idea that marginal benefits—those received because of a behavioral response—are valued between zero and fully at their cost, while inframarginal benefits are valued fully. The resulting bounds imply that the average male recipient aged 65–74 valued each dollar of OAA benefits he received at between $0.65 and $1 of unconditional late-life income. Consistent with these bounds based on our reduced form estimates, simulations of our estimated model imply that the average recipient valued each dollar of OAA benefits he received at $0.96 of unconditional late-life income. These equivalent-variation results are large relative to those found for many government programs (see, e.g., Finkelstein, Hendren and Luttmer, 2015). This result is particularly notable given that we focus on the segment of the 65-and-older population—men aged 65 to 74—with the highest levels of labor force participation and earnings. Intuitively, in addition to a large fraction of benefits being inframarginal even for this group, the average value of OAA benefits is high because the effects of OAA were highly concentrated among individuals with low potential earnings, for whom the cost of meeting the earnings test was smaller and for whom labor supply responses to OAA were driven mostly by income effects.

In the final section of the paper, we ask what both the reduced-form results and the estimated model suggest about the role of government old-age support—and of Social Security in particular—in the growth of retirement over the mid-20th century. OAA provides useful insight into this question in part because of the earnings test in the early Social Security program, which resembled the earnings tests of OAA.[5] Our results suggest that Social Security had the potential to drive a significant share of the mid-century decline in late-life labor supply. A simple extrapolation of our reduced-form results suggests that the expansion of Social Security from 1940–1960 would be expected to have reduced labor force participation among men aged 65–74 by 9.5 percentage points, 70 percent of the actual decline. We also use our estimated model to derive a lower bound of the effects of Social Security on labor supply, by simulating a version of Social Security that is conservative in that—for example—it does not include the eligibility and benefit expansions that actually occurred after 1939. The results suggest that even this relatively modest Social Security program would be expected to have large effects on labor supply, reducing labor force participation among men aged 65–74 by about 8.0 percentage points, 59 percent of the actual decline.

Although past work has studied the labor supply effects of OAA in the mid-20th century—Parsons (1991) and Friedberg (1999) in particular—our study is the first to use the available variation to shed light on the key features of the OAA program that affected intertemporal labor supply and the ex-post value of the program to recipients, which are key elements for understanding its effects on welfare.[6] More broadly, this paper relates to a large literature

[5]Between 1939 and 1950, for example, Social Security's earnings test limited benefits to people who had less than $15 of monthly earnings—about $230 in 2010 dollars. People who earned more would have their benefits withheld, without any compensating increase in future benefits.

[6]Papers that analyze other aspects of OAA include Costa (1999), who finds that OAA increased the propensity of elderly women to live independently; Stoian and Fishback (2010), who find that OAA had

that has investigated the effects of government old-age support—and Social Security in particular—on labor supply and retirement (for reviews, see e.g. Diamond and Gruber, 1999; Feldstein and Liebman, 2002; Krueger and Meyer, 2002; Coile, 2015). An important branch of this literature further seeks to decompose these effects on labor supply into those due to income transfers and those due to changes in marginal incentives to work associated with the earnings test and other aspects of the tax and benefit rules (e.g., Burtless and Moffitt, 1985; Friedberg, 2000; French, 2005; Gelber, Jones and Sacks, 2013). In using state policy variation to estimate the effects of old-age support on the full age profile of labor supply, our findings complement and extend this earlier work.

The role of government old-age support programs in reducing late-life work around the middle of the 20th century is a question that has arisen repeatedly in this literature: Lumsdaine and Wise (1994), Feldstein and Liebman (2002), Krueger and Meyer (2002), and Coile (2015) all discuss the close correspondence between historical trends in government old-age support and retirement. The causal relationship between the two remains unresolved, however, and other factors, such as rising incomes, are also plausible explanations for the broad trend toward earlier retirement (Costa, 1995, 1998).[7] The relatively small literature that has addressed this question directly has found somewhat mixed results. On one hand, Parsons (1991) and Friedberg (1999) estimate effects of OAA that suggest OAA and Social Security played a significant role, and Boskin (1977) uses estimates from the late 1960s and early 1970s to argue that the expansion of Social Security played an important role in the post-World War II retirement trend. On the other hand, Moffitt (1987) notes that the timing of Social Security benefit increases from the 1950s onwards does not match closely that of reductions in late-life labor supply, at least over short time intervals. Our findings contribute further evidence on this question, and as we discuss in the final section of the paper, simulations of our estimated model uncover a plausible explanation for these seemingly contradictory results.

little effect on elderly mortality in the early years of the program; and Balan-Cohen (2008), who finds that OAA reduced elderly mortality in the later years of the program.

[7] Although the time series relationship over the mid-20th century is striking, Costa (1998) and Lee (1998), based on the long retirement series of Moen (1988), note that attachment to the labor force among men 65 and above declined significantly between 1880 and 1910, so retirement rates had already risen substantially by the time OAA and Social Security were established. Costa (1995) studies Union Army pensions and retirement in the first decade of the 20th century and finds results suggesting that rising incomes could account for much of the rise in retirement over the 20th century.

2 Background on the Old Age Assistance Program

The New Deal legislation of the mid-1930s marked a major expansion of the role of the federal government in the economy and laid the foundations of many of the most important social insurance programs that continue to this day (Fishback, 2007). This was especially true of government programs providing old-age support. The Social Security Act of 1935 established two old-age support programs. One was Old Age Insurance, a payroll tax-financed pension program that in 1939 became Old Age and Survivors' Insurance (OASI) and came to be known as Social Security. Social Security was originally designed as a funded program, and relatively few of the elderly at the time were to receive benefits from it—it made no monthly payments until 1940, and even then to only a small share of the elderly. To provide for more immediate relief, the Social Security Act separately provided for federal matching funds for state-administered, means-tested old age support programs for the low-income elderly through the Old Age Assistance (OAA) Program.

These programs were associated with a major and rapid expansion in government old-age support. In 1929, just seven states had old-age assistance laws in effect. By 1939, every state did. Although Social Security eventually became the larger of the two programs, OAA was much larger than Social Security for many years. In 1940, about 22 percent of people aged 65 and over received OAA payments, and about 93 percent of the combined OASI and OAA payments were OAA grants.[8] Even in 1950, the majority of the combined OASI and OAA payments came from OAA. Both in terms of recipiency rates and average benefit levels, OAA was large relative to other programs at the time and relative to welfare programs today. The average annual OAA benefit in 1940 was \$232 (about \$3,615 in 2010 dollars), about 25 percent of 1939 median wage and salary earnings for 60-64 year olds earning a wage, and slightly over half of 25th percentile wage earnings.

States had a great deal of discretion in the design and administration of their OAA programs, subject to some broad conditions for qualifying for federal matching funds set in the Social Security Act. The key features of OAA programs were their eligibility requirements and benefit levels. The main eligibility requirements were having little income, the exact level of which varied across states, and being at least as old as a minimum age threshold, which was 65 years of age in almost every state. Many states also imposed asset tests; other common eligibility requirements included minimum state residency requirements, US citizenship, and having no legally responsible relatives able to provide support.[9] In almost all states, benefits

[8]See Carter et al. (2006), Series Bf395 and Bf634.

[9]The state residency requirements, which were imposed by all states, prevented people from migrating to states with high benefit levels and claiming benefits soon thereafter. These residency requirements, together with the low rate of migration among the elderly, suggest that systematic migration across states in response to differences in OAA was unlikely to have been quantitatively important, as also noted by Costa (1999)

were set in such a way as to provide either an income floor or a consumption floor, both of which implicitly tax recipients' income at a 100 percent rate, as benefits are phased out dollar-for-dollar with income.[10] In practice, either state or local OAA staff evaluated the "needs" and resources of each applicant, sometimes using a standard amount of $30 per month (i.e., $360 per year or about $5,600 per year in 2010 dollars) for the needs. The excess, if any, of needs over resources determined the size of the payment, up to a maximum level.[11] The maximum benefit level was $30 per month in most states, with a range from $15 to $45, plus eight states with no legislated maximum.

The large differences in the administration of OAA programs across states were reflected in large differences across states in both recipiency rates and payments per recipient. Table 1 shows summary statistics on recipiency and payments in December 1939. States varied widely in the share of the population 65 and older that received OAA, from 8 percent in the District of Columbia to 49 percent in Oklahoma, as well as in payments per recipient, from 6 dollars per month in Arkansas to 33 dollars per month in California. State payments per recipient and recipiency rates were positively related to one another across states, but the connection was weak, with a correlation coefficient of 0.17. The combined variation in recipiency rates and benefits per recipient generated significant variation in OAA payments per person 65 and older. Benefits per person 65 and older were just $1.01 in Virginia, whereas they exceeded $8 in several western states (with a maximum of $13.17, in Colorado).

Some of our robustness checks use an approximate 95th percentile payment as a measure of state OAA policy. As we discuss in Section 5, this measure approximates maximum payments but is both defined for all states and not driven by outliers—the eight states with no legal maximum had a small number of very large payments but for nearly all recipients had a *de facto* maximum that was well in line with other states' legal maxima. To calculate the 95th percentile payment, we use summary tables on the distribution of grants to new recipients by state in fiscal year 1938-39 (from U.S. Social Security Board (1939*b*)).[12] For most states these 95th percentile payments were the same as the legal maxima, as can be seen in Appendix Figure A3, although in some cases there were significant differences: Georgia,

and Friedberg (1999). We show evidence that migration is not a concern for our analysis in Section 5.

[10]The difference between an income floor and a consumption floor is that an income floor takes into account only income when determining benefits, whereas a consumption floor takes into account all of the resources available to an individual, including not only income but various assets as well.

[11]Lansdale et al. (1939) report that in most OAA programs, cases were re-evaluated regularly, usually every six months, and a non-trivial share of cases were closed due to the recipient becoming self-supporting or his or her relatives becoming able to provide adequate support. For recipients who wished to continue receiving OAA benefits, regular re-evaluations meant that any behavioral effects of the program were likely to be permanent.

[12]This publication reports the share of payments in 5-dollar bins, so we cannot always calculate the 95th percentile precisely. Instead we identify the bin containing the 95th percentile and use the smaller value of the upper endpoint of the bin or (when it exists) the state's legal maximum payment.

for example, had a legal maximum of 30 dollars per month but 95 percent of payments were for 15 dollars or less.

In considering the potential effects of OAA on labor supply, it is useful to consider the health and disability status of the elderly population at the time. Unfortunately, data comparable to modern measures is scarce over this period. Life expectancy among 65-year-old men was about 77.7 in 1940; in comparison, it was 80.3 in 1990.[13] On health status, Costa (1996) uses medical records from the Union Army pension program and the 1985-1991 NHIS to generate comparable measures of health in the early and late 20th century. She finds that the health of older men was worse in the earlier period but also that labor force participation was less responsive to health at the time.

3 Theoretical Predictions

The simplest model for understanding how OAA might affect the timing of retirement is a model of the lifetime budget constraint relating total lifetime consumption to the length of retirement, as illustrated in Figure 3.[14] OAA expands the set of consumption-leisure opportunities available to potential OAA recipients by paying recipients $\bar{y}$ for each period they do not work after the OAA eligibility age. OAA has an income effect that tends to hasten retirement and, for people who would retire after the OAA eligibility age if OAA benefits did not depend on earnings, a substitution effect that also tends to hasten retirement.

By reducing the private return to work after the OAA eligibility age but not before, OAA introduces a convex kink in the lifetime budget constraint at that age. For retirement ages younger than the OAA eligibility age, working an additional year increases total lifetime consumption by the full amount of earnings, w. For retirement ages older than the OAA eligibility age, working an additional year increases total lifetime consumption by the excess, if any, of earnings over the OAA benefit level, $\max\{0, w - \bar{y}\}$. OAA therefore imposes an implicit marginal tax on earnings after the OAA eligibility age, with implicit tax rate $\tau = \min\{1, \bar{y}/w\}$. With a smooth distribution of preferences for consumption versus leisure in the population, such a convex kink attracts more people than nearby allocations on the budget constraint. We measure the extent of such "excess bunching" of retirements at the OAA eligibility age in our empirical work. We use these patterns of bunching to estimate a model of lifetime labor supply, which we use to better understand the effects of OAA and to predict the effects of Social Security on retirement in the middle of the 20th century.[15]

[13] `https://www.ssa.gov/history/lifeexpect.html`

[14] This framework is better-suited to analyzing OAA programs that provide income floors than programs that provide consumption floors, since the latter might distort the timing of consumption.

[15] In the U.S. today there are many factors that might cause retirements to bunch at age 65, including

Another key prediction of the model is that OAA leads to a hollowing out of the distribution of labor earnings among people who are eligible for OAA, as earnings levels between zero and somewhat above the OAA benefit are replaced by zero earnings. This can be seen most easily by inspecting the within-period budget constraint, which relates income to leisure hours in a given period (e.g., a month). Such a budget constraint is shown in Figure 4. People whose optimal earnings levels in the absence of OAA fall between zero and not much above the OAA income floor would be better off exiting the labor force, since working would involve giving up much leisure for little if any gain in income.[16]

4 Data and Empirical Approach

4.1 Data

The key data source that enables many of our empirical tests is the full-population microdata from the 1940 Census, which was digitized in its entirety by Ancestry.com and the Minnesota Population Center. The data include basic demographic characteristics for all individuals enumerated in the Census, as well as basic employment and income information for all individuals age 14 and older. In addition to the large size of the sample, an advantage relative to previously available datasets is precise geographic location, which enables empirical tests that would not otherwise be possible.

We focus on men aged 55 to 74 in states in which the OAA eligibility age was 65 for all individuals in 1939.[17] Our sample therefore includes men within ten years of the OAA eligibility age of 65. Within these ages and states, we further restrict the sample to men with non-missing information on birthplace, race, citizenship status, marital status, and years of education. Our analysis below investigates two sets of outcomes: work behavior at the time of the 1940 Census and work and income outcomes in 1939. Restricting attention to men with non-missing information on all outcomes of interest would drop a significant share of the

Social Security, Medicare, and private pensions. Many of these factors either did not exist in 1940 or were much less important than they are today.

[16]This simple model predicts that no one who is eligible for OAA would choose to earn less than the OAA benefit level. In our empirical implementation of this test, however, there are reasons to expect non-zero mass at these earnings levels. For example, we do not observe all determinants of eligibility, and stigma or lack of awareness may also reduce take-up.

[17]This restriction excludes men residing in three states—Missouri, New Hampshire, and Pennsylvania—that had an OAA eligibility age of 70 in 1939, all of which reduced the eligibility age to 65 on January 1, 1940, as was required to continue to qualify for federal matching funds. It also excludes Colorado, in which long-term residents became eligible at age 60. Although in principle different age eligibility requirements could provide a useful source of variation, unfortunately it is not one that is straightforward to use since the age requirement was changed just a few months before the 1940 Census was taken.

sample, so for each set of outcomes we exclude from the sample only those men with missing information on work (or income) outcomes in the relevant year.[18] As discussed in more detail below, our main empirical tests rely on comparability of age-work profiles across states with different OAA policies. To help assure that differences in age-work profiles across states are not due to differences in unobserved population characteristics, some of our specifications limit comparisons to counties on either side of a state boundary. The "border county" sample is derived from the full sample by limiting to counties that bordered other states (except for counties bordering only the four states excluded from the full sample).

Table 2 describes the characteristics of the men in our sample. About 71 percent were in the labor force, and 65 percent were employed. An important component of overall employment in the late 1930s was "public emergency" employment—employment through one of the federal programs that provided work-based relief to the unemployed, such as the Work Projects Administration (WPA).[19] For men in our sample, about 62 percent were employed in either private or non-emergency government work and about 4 percent were employed in public emergency work. The 1940 Census was the first federal census to ask about income, and it asked separately about wage and salary income and income from other sources. About half of men reported receiving any wage or salary income in 1939.[20] Including those who reported zero wage and salary income in 1939, the average reported income was \$557 (corresponding to about \$8,672 in 2010 dollars). There was no question on the amount of income from sources other than wage or salary, but there was a question to each individual asking whether he or she received income from these sources of \$50 or more (about \$780 in 2010 dollars).[21] Slightly more than half of our sample reported that they did.

A comparison of means across the full and border county samples indicates only small differences between the two. Men in the border county sample were about two percentage

[18]Hence, our analysis relies on two different but largely overlapping samples. One comprises the 6,722,869 men aged 55 to 74 with non-missing 1940 labor supply and basic demographic information; the other comprises the 6,283,146 men with non-missing 1939 work and income information as well as non-missing basic demographic information. Restricting the analysis to a common sample with no missing information does not affect the results.

[19]Fishback (2007) is one source of further information on these programs; they targeted those who were unemployed but "employable" and hired individuals at a wage that was low compared to similar private-sector jobs. Although in principle the WPA was meant to be only temporary relief for workers unable to find other employment, Margo (1991) shows that many of the individuals on work relief through the WPA were essentially working full time for the WPA, in large part because of its perceived stability relative to private sector jobs.

[20]The share of men reporting receipt of wage and salary income is smaller than the share of men who reported working in 1939 because, as indicated in the instructions to enumerators, the former excluded income earned by businesspeople, farmers, and professionals through business profits, sale of crops, or fees.

[21]The instructions to enumerators indicated that non-wage income included, among other things, income from business profits or professional fees, income from roomers or boarders, cash relief payments, regular contributions from family members not in the same household, in-kind income, and commodities consumed from the individual's own business.

points less likely to have completed primary school, and the various measures of labor force attachment were higher by about 1 to 1.5 percentage points in the border sample. These differences are quite small relative to their respective means. The similarity of the full and border county samples suggests that inferences drawn from the border county sample can be reasonably applied to the population as a whole.

In our empirical tests below we also use state- and county-level data on OAA. State-by-month level data on the number of OAA recipients and OAA payments from 1936 through 1939 come from the 1939 *Social Security Yearbook* (U.S. Social Security Board, 1940*b*). We also digitized county-level data on the number of OAA recipients and the amount of OAA payments in December 1939, reported in U.S. Social Security Board (1940*c*).

4.2 Empirical approach

We use two key sources of variation to investigate the effects of OAA. The first of these is the age-based eligibility requirement that was a feature of OAA programs in all states, nearly always providing assistance only to persons 65 or older.[22] OAA was by far the largest means of old-age support for which 65 was a cutoff age as of 1940. In more recent periods, changes in behavior at or around age 65 could be associated with any of a number of factors, such as eligibility for Social Security or Medicare. However, Medicare did not exist until 1965, and monthly payments under Social Security (OASI) did not begin until January 1940, and even in 1940 went to less than two percent of the population 65 and older.[23] Other sources of old-age pensions at the time were significantly smaller than OAA and were not likely to have been correlated with state OAA policy variation.[24]

[22]Most states did not have mandatory birth certificates for the cohorts in our sample, so in addition to birth certificates a range of other means were used to determine age-eligibility. What records were valid depended on state law or administrative procedure. Valid records often included marriage records, school records, earlier Census records, or earlier voter registration records; in some cases the affadavit of a "reputable person" with knowledge of the applicant's age would be accepted in the absence of the normal documentary proof. Given the absence of nationwide mandatory birth records, Lansdale et al. (1939) acknowledge that verification of age was sometimes difficult. Indeed, Ransom and Sutch (1986), among others, note that Census counts of 65-74 year olds in 1940 were somewhat higher than would be expected given the number of 55-64 year olds in 1930, and suggest that the excess may have been due to incentives to misreport one's age after the passage of the Social Security Act. Although this may have been the case, our results indicate no apparent anticipatory effects among people below the eligibility age, suggesting that that misreporting of age is not a major concern for our findings. We discuss this point in more detail in Section 5.2.

[23]Social Security did make lump-sum payments to workers turning 65 in the first three quarters of 1939, but these were only one-time payments to reimburse taxes collected.

[24]The other major sources of old-age pensions at the time were private pensions, state and local government pensions, federal civil service pensions, and railroad pensions. In 1940 there were about 160,000 monthly beneficiaries of private pensions (Carter et al., 2006, Series Bf848). McCamman (1943) estimates that there were about 158,000 beneficiaries of state and local government pensions, but notes that a significant share of these were for police and firemen, who typically had retirement ages before 65. There were about 141,000 beneficiaries of railroad retirement benefits (Carter et al., 2006, Series Bf753) and about 32,000 beneficiaries of

The second key source of variation that we use is the heterogeneity in state policy discussed in Section 2. Variation in both the conditions of eligibility and the generosity of benefits allows comparison of labor supply behavior of individuals of the same age but facing different state policies. Combining variation in both age-based eligibility and state policy, we can control flexibly for any age-specific effects common across states or for the possibility that state OAA policies were correlated with unobserved factors also affecting labor force participation, provided that they do so in a way common across ages.

We expect that, especially during the late 1930s, the population likely to be eligible for OAA had little access to the formal financial sector and would therefore have had difficulty borrowing against future OAA benefits before becoming eligible at age 65 (see, e.g., Anari, Kolari and Mason, 2005; Rose, 2014; Carlson and Rose, 2015). Nevertheless, anticipatory responses before age 65 might be possible to the extent that, for example, people close to reaching 65 and likely to qualify for OAA might informally borrow from their children. Further, OAA means tests could have provided an incentive to change behavior prior to reaching eligibility in order to increase the likelihood of receiving OAA once one reached age 65. Because we rely on age eligibility for identification, we do not directly identify anticipatory effects, and our estimates of the effects of OAA are net of such effects. Differential trends across states in the age profile of labor force participation, however, will provide some indication of the likely size of anticipatory effects, and will also speak to the relative size of the net-of-anticipatory effects between the young elderly (those just turning 65) and older individuals.

The main results reported below use state OAA payments per person aged 65 and older as a summary measure of the generosity of state OAA programs, with modifications to address the reverse causality concerns that would normally arise. There are two motivations for using this measure. First, it summarizes two aspects of state policy—broadness of eligibility and payment levels—that are both relevant for the effect of OAA on labor supply. Second, it offers more continuous variation than the basic features of state policy, such as maximum payments. We use this measure for our main results, but as we discuss in Section 5, the results of the paper are robust to using a measure of maximum payments, although some are less precise due to the relative coarseness of variation in maximum payments across states.[25]

federal civil service pensions with a retirement age of 65 (Reticker, 1941). By way of comparison, slightly more than 9 million people were aged 65 and older in the 1940 Census. Hence, the total number of beneficiaries of these plans was only about 5 percent of the population 65 and older in 1940, and some of these plans had retirement ages other than 65. Average payments under these plans were also much larger than OAA (between 750 and 950 dollars per year) and were likely primarily relevant for people higher in the income distribution than OAA recipients.

[25]A simulated IV strategy parameterizing the full range of payment and eligibility criteria in a spirit closer to Currie and Gruber (1996) would also be a natural approach. In practice, state maximum payments provide most of the predictive power in such a measure, most likely because data limitations make it difficult to construct a direct measure of eligibility. There is some information relevant to eligibility in the Census and in the 1935–36 Survey of Consumer Purchases, but several factors that determined eligibility are unobserved.

State OAA payments per person is a function of both policy and the level of need in the state, and we seek to use the variation in this measure that is due solely to differences in policy. Two features of our empirical approach allow us to do so. First, to correct for the mechanical relationship between labor hours and OAA payments, the measure of OAA payments we use for a given individual excludes his own payments and recipiency status. In particular, for an individual i in state s and county c we measure the payments per person 65 and older across all counties other than c in state s. If we made no other adjustments, we would estimate equations of the form

$$y_{iacs} = \alpha_a + \beta_c + \sum_{a \neq \bar{a}} \gamma_a * \log(\text{payments per person } 65+)_{s \backslash c} + \Lambda' \mathbf{x}_{iacs} + \varepsilon_{iacs}, \tag{1}$$

where a indexes age (either in single years or groups of years), $\bar{a}$ is a reference age, $\mathbf{x}_{iacs}$ is a vector of controls, and the variable of interest, $\log(\text{payments per person } 65+)_{s \backslash c}$, is the log of the December 1939 OAA payments per person 65 and older in state s outside of county c, which we refer to as a "rest-of-state" payment per person.[26] In this specification, identification relies on the assumption that once we have corrected for the direct mechanical relationship between OAA payments and labor supply, age profiles of labor force participation would have been parallel across states in the absence of OAA. In estimating equation (1), this assumption can be weakened somewhat by introducing controls in the vector $\mathbf{x}_{iacs}$ to limit comparisons of age profiles to more similar groups: in some specifications we include race-by-age and years of education-by-age fixed effects and Census region-by-age fixed effects.

A second feature of our empirical approach is meant to address two remaining concerns. To the extent that spatially correlated factors, such as differential age trends in disability or labor demand shocks, drive exit from the labor force and thereby increase OAA payments even holding state policy fixed, adjusting for the direct mechanical relationship between labor hours and OAA payments may not be sufficient. Separately, it is also possible that the generosity of state policy may be correlated with underlying age trends in labor force participation. To address these concerns, in our preferred specifications we limit comparisons of age profiles of labor force participation to counties lying on either side of a state boundary. Doing so yields causal estimates of the effect of OAA under the assumption that differential trends of disability with age, or labor demand shocks, exhibit sufficiently smooth spatial variation that comparison across state boundaries differences out these factors. In these specifications we limit the sample to counties lying on the boundary with another state and

Neither dataset provides estimates of eligibility that have a compelling first-stage relationship with observed state-level recipiency.

[26] State-level payments per person 65 and older exhibit a right skew, which motivates our choice of a specification in logs rather than levels; levels specifications for the main regressions are reported in the appendix. Using a rest-of-state measure excludes the District of Columbia from our analysis. We use the county population 65 and older in April 1940 to scale December 1939 OAA payments.

estimate equations of the form

$$y_{iacsb} = \beta_c + \delta_{ba} + \sum_{a \neq \bar{a}} \gamma_a * \log(\text{payments per person 65+})_{s \setminus c} + \Lambda' \mathbf{x}_{iacs} + \varepsilon_{iacsb} \qquad (2)$$

where a border segment b between two states is the set of all counties in either state that touch the boundary between the two. Since some counties border two or more different states, in this specification a county (and hence all the individuals in it) will appear in the data as many times as there are states that it borders. The border segment-by-age fixed effects then limit comparisons of age profiles to men living on either side of the same border.[27]

5 Results

5.1 Age eligibility, state generosity, and OAA receipt

We first show that passing the age eligibility cutoff was a meaningful determinant of OAA receipt and that it was correlated with the basic policy measure we use in our main regression specifications. In the 1940 Census, there was no question directly inquiring about whether an individual received a payment through OAA. However, as noted above, each individual aged 14 and older was asked whether he or she received more than \$50 in income other than from wages and salaries in 1939. Figure 5 shows the share of men receiving non-wage and salary income by age in 1939, for states with above- and below-median OAA payments per person 65 and older. Although receipt of non-wage income at these ages was common and became more so with age, there is a clear break at age 65, suggesting that aging into eligibility was indeed associated with an increase in available resources. The increased receipt of non-wage income after age 65 could not have been driven by OASI monthly payments, which did not begin until 1940. Nor could it have been due to the OASI lump-sum payments at age 65 that were made in 1939 and earlier, since these would not explain the elevated level of non-wage income past age 66. Moreover, the fact that the increase is greater in states with larger OAA programs and arises only after age 65 provides a strong indication that it reflects receipt of OAA benefits.

This "first stage" result also holds conditional on the finer comparisons that we make in

[27] In principle, the comparison across state borders could be refined in at least two ways. One is to compare only adjacent counties on either side of a state border. For all of the core results of the paper, doing so yields virtually the same results as using the entire border as a comparison group. Given this close similarity, we focus on the latter because it is less burdensome computationally. Another refinement would be to use even finer geographical information to implement something more akin to a spatial regression discontinuity design. Although feasible in principle, the placebo checks in Section 5 suggest to us that it is not necessary to do so.

investigating the effects of OAA on labor supply. In Figure 6 we plot estimates of the age-payment interaction from equation (2). The trend in coefficients is quite flat prior to age 65 and increases sharply from ages 65 through 67. The similarity of trends prior to age 65 further suggests state OAA generosity was not correlated with unobserved factors driving differential trends in receipt of non-wage income by age.

Table 3 shows corresponding estimates from estimating equations (1) and (2). To obtain a summary measure of these patterns allowing more statistical precision and economy of presentation, we group ages into 5-year bins, with ages 60–64 as the reference age. Columns (1)-(3) show estimates of equation (1) in the full sample. The results confirm that in states with larger OAA programs, there was a differential increase after age 65 in receipt of non-wage income that is economically and statistically significant. The point estimates change only modestly with the addition of region-by-age fixed effects and are essentially unchanged with the addition of education-by-age and race-by-age fixed effects. Although there is some indication of a slight differential increase prior to age 65 in states with larger OAA programs in the specification with no controls, the coefficient on the interaction between OAA payments and the age-55-to-59 indicator declines in magnitude and becomes statistically insignificant at conventional levels with the addition of region-by-age fixed effects. In specification (3), which includes all controls in the full sample, the point estimates on the interactions of OAA payments with age indicate that a one standard deviation increase in log payments per person 65 and older—an increase of about .62 log points—was associated with a differential increase in the probability of receiving non-wage income of 3.6 percentage points at ages 65–69 and 5.6 percentage points at ages 70–74, both highly statistically significant.

The same patterns are also evident in specifications that limit comparisons to the border sample. Column (4) estimates equation (2) on the border county sample with no additional controls, by way of comparison to column (1). It gives very similar estimates, suggesting that using the border sample comes at little cost in terms of representativeness. Column (5) introduces border segment-by-age fixed effects and results in a small reduction in the magnitude of the coefficients, but they remain both economically meaningful and highly statistically significant. The estimates in column (5) imply that a one standard deviation increase in the size of a state OAA program was associated with a differential increase in the probability of receiving non-wage income of about 3 percentage points at ages 65–69 and 4.3 percentage points at ages 70–74. Inclusion of education-by-age and race-by-age fixed effects leads to no meaningful change in the coefficients.

5.2 Effects of OAA on labor market outcomes

Analogous specifications provide evidence that eligibility for OAA also translated into reduced labor force participation. Figure 7 plots the coefficients on the age-payment interactions from equation (2), which limits comparisons to counties on either side of a state border. The coefficients are all quite close to zero at ages up to 64, but at age 65, states with larger OAA programs exhibit a sharp decline in male labor force participation that levels out around age 69 at about -0.05. This indicates that a 10 percent increase in OAA payments per person 65 and older was associated with a reduction in labor force participation of about 0.5 percentage points. The lack of a pre-trend, combined with the sharpness of the change around age 65, supports the assumption that states with different payments per person were comparable in their underlying trends of labor force attachment with age, and hence that neither reverse causality nor potential correlation of OAA policy with population characteristics drives the results.

Table 4 shows estimates from estimating equations (1) and (2) using 5-year age bins. All specifications indicate that states with larger OAA programs featured differentially large reductions in labor force participation after the OAA eligibility age. The estimates in columns (1) through (3), estimated on the full sample, give estimates of about -0.06 at ages 65–69 and -0.07 at ages 70–74, both highly statistically significant. These estimates are fairly stable across specifications: adding region-by-age, education-by-age, and race-by-age fixed effects has only modest effects on the estimated coefficients. In these specifications there is evidence of a slight differential reduction in labor force participation prior to the age of eligibility: states with 10 percent higher OAA payments saw a reduction in labor force participation from ages 55–59 to ages 60–64 that was greater by about 0.18 percentage points. In principle, these reductions prior to eligibility could reflect anticipatory effects of OAA, but they may also indicate that some portion of the difference in age-work profiles after age 65 reflects differential underlying trends in labor force participation that were correlated with state OAA generosity.

To address this concern, columns (4)-(6) provide estimates of equation (2) based on the border county sample. These results provide strong evidence that most of the relative decline in labor force participation after age 65 in states with larger OAA programs was indeed due to OAA. Column (4) presents estimates in the border sample without border segment-by-age fixed effects, and gives estimates very close to the analogous specification in the full sample. Column (5) introduces border segment-by-age fixed effects, which limit comparisons to counties across state borders. Here there is no evidence of any differential trend across states prior to age 65.[28] The estimates for ages after age 65, moreover, are highly statistically sig-

[28]The lack of any significant pre-trend also suggests that misreporting of age is not a major concern for

nificant and indicate substantial reductions in labor force participation in states with larger OAA programs. In particular, they suggest that a one standard deviation increase in log payments per person 65 and older (about .62 log points) was associated with a 3 percentage point reduction in labor force participation at ages 65–69, and a 3.5 percentage point reduction in labor force participation at ages 70–74. Unsurprisingly, given the tight geographic restrictions on comparisons in column (5), differences across states in demographic characteristics do not drive the results. Inclusion of education-by-age and race-by-age fixed effects in column (6) leaves the coefficients virtually unchanged.

The similarity of the regression coefficients (in opposite directions) in Figures 6 and 7 suggest that receipt of non-wage income and exit from the labor force were tightly linked. To investigate this pattern further and provide additional evidence that it was OAA driving exit from the labor force, we take a simple approach to estimating the bunching of retirements at age 65 and its relationship to the receipt of non-wage income. In particular, we estimate the following model to quantify the "break" at age 65, separately by state:

$$y_i = \beta_0 + \beta_1 \mathbf{1}(\text{age}_i \geq 65) + \beta_2(\text{age}_i - 65) + \beta_3(\text{age}_i - 65)\mathbf{1}(\text{age}_i \geq 65) + \varepsilon_i \tag{3}$$

where the outcome is either receipt of non-wage income or labor force participation.[29] In Figure 8 we plot the estimated breaks at age 65 from equation (3) for receipt of non-wage income against estimated breaks in labor force participation. The results illustrate the extent of variation across states in the overall drop in labor force participation at age 65, from nearly zero in Arkansas to 15 percentage points in Oklahoma. Moreover, declines in labor force participation line up strikingly well with increases in receipt of non-wage income, consistent with OAA income substituting for labor income as men aged into eligibility.

The main specifications suggest that OAA significantly reduced labor supply in 1940 of men aged 65 to 74 and that OAA was a major factor in the decline in labor force participation of the elderly over the 1930s. Re-estimating the state-border specification using the level rather than the log of OAA payments per person 65 and older yields coefficients of -0.013

our results. The concern with age misreporting would be that false reporting of age was more common in states with more generous benefits and that men with high disutility of labor were differentially more likely to misreport that they were eligible. Such misreporting would increase measured labor force participation before the eligibility age and decrease it after the eligibility age. The lack of a pre-trend in labor force participation is evidence against this concern, since it seems reasonable to assume that falsely reporting an age above 65 was more common among men aged 60–64 than among men aged 55–59, for example. If so, we should see elevated labor supply of 60–64 year olds relative to 55–59 year olds in more generous states relative to less generous states. We find no evidence that this was the case. Tests for differential pre-age 65 trends back to ages 50–54 yield a similar lack of a pre-trend.

[29] The 1940 Census has information only on age in completed years at the time of the Census, meaning that individuals who were 65 at the time of the Census may or may not have been eligible for OAA during 1939, the time period covered in the non-wage income question. Hence, in estimating the break in non-wage income we omit 65-year-olds.

for ages 65–69 and -0.016 for ages 70–74; these and other results using a levels specification are reported in Appendix Table A1. The implied magnitudes are reasonably similar to the log specifications at the mean level of OAA payments.[30] Based on the level specification, reducing the OAA payment per person from four to zero dollars would have increased labor force participation by 5.2 percentage points at ages 65–69 and by 6.4 percentage points at ages 70–74. Approximately 60 percent of 65-74 year olds were between 65 and 69, so these estimates imply that OAA reduced labor force participation among 65-74 year olds overall by about 5.68 percentage points. By way of comparison, 5.68 percentage points is about 11 percent of the overall 51 percent labor force participation rate of men aged 65-74 in 1940. This estimate also suggests that OAA can explain just under half of the decline from 1930 to 1940 in labor force participation of men aged 65-74, which fell from about 64 to 51 percent.[31]

5.3 Responses by education and employment status

As we discuss in more detail in Section 6, the degree to which the effects of OAA were due to the transfer to older people, as opposed to the tax on their labor—and hence the impact of OAA on welfare—depends on how much the effects of OAA were concentrated among those with low potential earnings, for whom replacement rates from OAA would be higher. We defer a full discussion to Section 6, but show here that a simple extension of the results so far suggests that responses to OAA were concentrated among people with low earnings. Figure 9 shows the age 65–69 estimates from equation (2) separately by grouped years of education.[32] Education groups are arranged horizontally according to the average 1939 wage and salary earnings of non-self-employed 45–54 year old men of that level of education. The estimates indicate that the effects of OAA were greatest for men with the lowest levels of education, and therefore most likely among those with low levels of potential earnings.[33]

Especially in the context of labor markets of the 1930s—with high unemployment rates

[30]The overall average amount of OAA payments per person 65 and older is close to four dollars (either weighting states equally or by population), so a 10 percent increase in OAA payments per person relative to the mean of 4 is associated in the level specification with a 0.52 percentage point reduction in labor force participation for ages 65–69 and a 0.64 percentage point reduction for ages 70–74, both of which are reasonably close to the predicted changes in the log specification.

[31]This figure for 1930 uses the Durand (1948) adjustments to obtain a labor force participation rate based on the underlying gainful employment data.

[32]Those who had not completed primary school (8 years) are split into two groups; the remaining groups correspond to primary school completion, some high school, high school completion, some college, college completion, and more than college.

[33]The large point estimate for college-educated men is a surprising aberration from the general trend, but is quite imprecisely estimated. The effects at education levels of high school completion and higher may be driven by misreporting of education in the 1940 Census as much as by real effects. Goldin (1998) documents that high school completion rates are significantly overstated in the 1940 Census for older individuals but that over-reporting does not appear to be an issue at levels of education below high school completion.

and the importance of work-based relief through programs such as the WPA—two further questions arise. First, what share of the reduction in labor force participation due to OAA was associated with exit from unemployment or from work-based relief, as opposed to private or public non-relief employment?[34] To the extent that workers who retired as a result of OAA would otherwise have been on work-based relief or unemployed, as opposed to having stronger labor force attachment, the net cost of OAA would be smaller. To address this question, Table 5 shows estimates of equation (2) using overall employment (including work-based relief) as an outcome variable, as well as employment in private or public non-emergency work.[35] Columns (4)-(6) present the preferred specifications, which limit comparisons to state borders and control for race-by-age and education-by-age fixed effects. Comparison of the point estimates for different outcome variables suggests that between 50 and 60 percent of the reduction in labor force participation was associated with exit from private or non-emergency public employment, but exit from unemployment and exit from public emergency work also played important roles. At ages 65–69 about 26 percent of the reduction in labor force participation was associated with exit from unemployment and about 21 percent with exit from public emergency work. At ages 70–74 these figures are 21 percent each from unemployment and public emergency work. Hence, close to half of the effect of OAA on labor force participation was associated with retirement of men who had relatively weak labor force attachment.

A second, and somewhat related, question is whether the effect of OAA on retirement was larger in 1940 than it would have been in a context of lower unemployment. This question has implications for the generalizability of the results to an understanding of the broader 20th century trend in retirement. It is unclear whether one should expect the effect of a program like OAA to be larger or smaller in a context of weak labor demand. On one hand, weak labor demand means more unemployment, and people may be more responsive to OAA if they are unemployed than they would be if they were employed. On the other hand, weak labor demand means that many people with weak attachments to the labor force may have already left the labor force regardless of OAA. To shed light on this question, Table 6 tests for heterogeneity in the effect of OAA by the county unemployment rate for men aged 45–54.[36] The results provide no indication that the estimated effect of OAA is larger than it would have been in a context of stronger labor demand. Point estimates for the interactions of OAA with the unemployment rate at both ages 65–69 and 70–74 are positive

[34] This question provides a historical parallel to the finding in more recent periods that Social Security serves in part as a form of unemployment insurance for older workers (Coile and Levine, 2007, 2011).

[35] Appendix Figures A1 and A2 show means and estimates of equation (2) by single years of age.

[36] Geographic variation in the severity of the Depression is well documented: see, e.g., Wallis (1989) and Rosenbloom and Sundstrom (1999). The county unemployment rate for men 45–54 provides one measure of the severity of the labor demand shock that does not include our sample individuals directly. Some small counties in the data report no labor force participants at these ages, explaining the slightly smaller sample than in the main regressions.

and statistically insignificant.

5.4 Robustness

Although the lack of a differential trend in labor force participation by age prior to age 65 already provides evidence for our identification assumption, to address any residual concern we provide further support for our interpretation in three ways. In particular, we use a measure closer to underlying policy rather than an observed payments variable; we carry out a placebo analysis examining the labor force participation of non-U.S. citizens, who were ineligible for OAA in many but not all states; and we conduct a second placebo analysis testing for differential age trends in labor force participation in 1930. A secondary concern is that endogenous migration biases the results, which we address by examining migration directly.

5.4.1 Using measures of maximum payments

Our interpretation of the main results relies on the assumption that excluding an individual's own county from the measure of OAA payments applied to him, and restricting comparisons to state borders, addresses any reverse causality concern with using an observed-payments variable as a measure of OAA policy. To assess whether this assumption is reasonable, we report results using two measures of state maximum payments. One possibility is to use legal maximum payments and to restrict analysis to those states that had them; an alternative that approximates *de facto* maxima is to use the 95th percentile payment in a state (as described in Section 2). As can be seen in Appendix Figure A3, the latter measure tracks legal maxima closely in those states that had legal maxima. It also has the desirable feature that it is defined for the eight states that had no legal maximum, which increases the number of comparisons that can be made.[37]

Appendix Tables A2 and A3 report results for the main outcome variables using the annual maximum payment and the annual 95th percentile payment, respectively (both measured in hundreds of 1940 dollars; we report specifications in levels, which can be more easily used to estimate labor force participation in the absence of OAA). Qualitatively, the results correspond closely to our main results: they show differential declines in measures of labor force attachment after age 65 in states with higher maximum payments. Quantitatively, they suggest that if anything, the main results understate the aggregate reduction in labor force

[37] Another alternative would be to use observed maximum payments, but in states lacking legal maxima the observed maxima tend to reflect highly unusual situations (the reported maximum payment in Alabama in 1938-39, for example, was $111 per month, but 95 percent of payments were for $30 or less per month).

participation due to OAA: weighted by population, the average monthly legal maximum payment and 95th percentile payments were both about $29.50 per month, or roughly $350 per year. The coefficients imply that reducing payments to zero from the average level would increase labor force participation by 8.5 to 9 percentage points, compared with the 5.7 percentage point increase implied by the main estimates.

Another way to see the similarity of the results using these more basic features of policy is to instrument for OAA payments. Appendix Figure A4 shows that 95th percentile payments are highly predictive of payments per person at the state level (maximum payments are as well, but have a slightly weaker first stage due in part to the states lacking legal maxima).[38] The IV estimates in Table 7 for each of the four dependent variables—receipt of non-wage income, and the three labor force attachment variables—indicate that our main results are not driven by reverse causality concerns. All are in line with the main coefficient estimates, and the coefficients for labor force participation, employment, and non-emergency employment are slightly larger in magnitude than those shown in Table 5. The breakdown of changes in labor force participation across unemployment, non-emergency employment, and emergency employment are also in line with the main estimates.

5.4.2 Results by citizenship status and citizenship requirements

Another robustness check is to test for responses among an ineligible population: non-US citizens in states requiring citizenship. Because of the relatively small number of non-U.S.-citizens at these ages, estimates of the effect of OAA on non-citizens are extremely imprecise when comparisons are limited to state boundaries. Hence, in these results we use the full sample. The left panel of Figure 10 plots estimates of equation (1) for non-U.S.-citizens separately for states that required U.S. citizenship (of which there are 20 in our sample) and for states that did not require U.S. citizenship or long-term residency in the United States (of which there are 17 in our sample). Here we include Census region-by-age fixed effects and group ages into 5-year bins for statistical precision.

The results provide striking confirmation that our main results are not driven by underlying differences in retirement behavior across states. In states in which non-citizens were eligible, more generous OAA programs were associated with larger reductions in labor force participation after age 65, with statistically significant coefficients on the order of -0.09 at ages 65–69 and 70–74. In contrast, in states in which non-citizens were ineligible for OAA, larger

[38]The state-level regression closest to the first stage for the IV results reported in Table 7 is a regression of log payments per person in December 1939 on the log 95th percentile payment; the estimated coefficient is 1.7, with a robust standard error of 0.24. Regressing the level of payments per person on the level of the 95th percentile payment yields a coefficient estimate of 0.27 with a robust standard error of 0.06.

state OAA programs show no sign of being associated with lower labor force participation of non-citizens: coefficients are close to zero and statistically insignificant for all age interactions. In contrast, the right panel shows that estimates across the two sets of states are quite similar for U.S. citizens. Provided that men who were not U.S. citizens would have exhibited trends in disability status (or in other factors that would determine labor force participation) similar to citizens, these results suggest that the main estimates reflect OAA-induced exit from the labor force rather than OAA take-up being driven by reduced labor force participation or correlation of state policy with underlying population characteristics.

5.4.3 Do 1940 OAA payments predict differences in 1930?

Analysis of labor force outcomes in the 1930 Census provides further evidence that differential trends in the underlying propensity to exit the labor force do not drive our results. If they did, it is likely that we should see the same patterns in 1930. We use the 1930 complete-count Census data to estimate equation (2) in order to test whether observed payments in 1940 have a similar relationship with labor force outcomes in 1930.[39] Because a handful of states did have old-age assistance laws even in 1930, we omit the nine states included in our 1940 sample that also had programs in 1930. Panel (a) of Figure 11 indicates that age profiles of work behavior in states with larger OAA programs in 1940 exhibited little difference from those in states with smaller OAA programs. For comparison, in Panel (b) we show estimates from 1940 using the same sample of states and find results very similar to our main estimates, showing that the absence of differences in 1930 is not an artifact of using a different set of states.

5.4.4 Migration

Another possible concern with the results is that individuals with high disutility of labor chose to move to states with more generous OAA programs when they became eligible, or migrated out of more generous states at a lower rate. In either case, our empirical test would overestimate the reduction in labor supply upon aging into eligibility. The minimum residency requirements imposed by almost all states makes the first type of migration less likely, but to address the possibility of higher in-migration and lower out-migration we test for such effects using information on state of residence in 1935. Appendix Table A4 reports estimates

[39] The 1940 Census was the first Decennial Census to use the concept of "labor force participation," which was based on a person's employment or unemployment status in the last week of March 1940. The 1930 Census provides information on the closely related but distinct concept of "gainful employment," measuring whether an individual reported having had an occupation in the previous year. Costa (1998) provides further details and Moen (1988) an extensive discussion.

of the baseline specifications with the dependent variable indicating whether an individual lived in a different state in 1935. Point estimates are quite small, and the 95% confidence intervals suggest that a one standard-deviation increase in generosity was associated with no more than half a percentage point higher or lower probability of having moved since 1935, a magnitude substantially smaller than our labor supply results.[40] Hence, net migration of individuals with lower baseline levels of labor supply to more generous states after aging into eligibility is unlikely to explain our results.

6 Understanding the Effects of OAA

The results so far indicate that OAA significantly reduced labor supply. This raises the question of the relative importance of the two key features of OAA—its transfer component and its implicit tax on work—in driving this effect, and the welfare cost to recipients of any effects operating through its implicit tax on work. In this section, we use a life cycle model of labor supply and retirement to address these questions.

6.1 Model estimation and validation

6.1.1 Model

Consider a standard model of lifetime labor supply in which people choose how much to consume at each date and when to retire. Individual i at age t maximizes the discounted sum of utility from age t forward,

$$U_{it} = \sum_{s=t}^{T} \beta^{s-t} u_{is}(c_{is}, h_{is}),$$

where c is consumption, h is hours of work, and

$$u_{is}(c_{is}, h_{is}) = \frac{c_{is}^{1+\eta}}{1+\eta} - \delta_i \mathbb{1}(h_{is} = \bar{h}),\ \eta \leq 0,$$

[40] If someone under age 65 migrated to a more-generous state in anticipation of taking up OAA benefits upon reaching age 65 but continued to work while still ineligible, the baseline specification may not pick up such migration. To assess the extent to which migration of this sort would influence our results, we estimated an alternative specification that restricts comparisons to state borders and simply tests for differences in the probability of migration within each age group (as in equation (4), below). The results of this alternative specification are similarly small in magnitude.

subject to a constraint on hours of work, $h_{is} \in \{0, \bar{h}\}$ (so there is only an extensive-margin labor supply decision), and a dynamic budget constraint,

$$a_{it+1} = (1+r)(a_{it} + N_{it} + \hat{w}_{it}h_{it} + b_{it} - c_{it}) \geq 0.$$

a_{it} are assets, N_{it} is non-labor income, $\hat{w}_{it}$ is the wage, $\hat{w}_{it}h_{it}$ is labor earnings, and b_{it} is OAA benefits. The last inequality reflects the constraint that the individual cannot borrow.

We consider an "income-focused" OAA program that provides an income floor of $\bar{y}_{it}$ to individual i at age t:

$$b_{it} = \max\{0,\ \bar{y}_{it} - \hat{w}_{it}h_{it}\},$$

where $\bar{y}_{it}$ is the OAA benefit available to individual i in the period in which i is t years old. This is a simplified version of a typical OAA program in 1940.

6.1.2 Parameter values

Preferences.— We estimate the key preference parameters—the coefficient of relative risk aversion, η, and the distribution of the disutility of labor, $F(\delta)$ (individuals may differ in their disutility of labor, δ_i)—using certain key features of the data, as described below. We adopt a standard value of the discount factor for our main specification, $\beta = \frac{1}{1+r} = \frac{1}{1.03} \approx 0.97$, and test the robustness of the key results to alternative values.

Budget constraints.— Individuals in the estimation and simulations are drawn from the joint distribution of earnings and OAA benefit levels observed in 1940; details of the calculation of this distribution are given in the appendix.[41] We assume that potential earnings, $w_{it} = \hat{w}_{it}\bar{h}$, are constant in real terms over the life cycle and that OAA benefits are fixed at their real values in 1940. We further assume that people learn about OAA in 1936, a year in which many state OAA programs were introduced. That people had relatively little time by 1940 to incorporate OAA into their plans may have shaped the effects of OAA on behavior.[42] Because assets and non-labor income are measured only very coarsely in the data, we make the simplifying assumptions that initial assets when the individual enters the labor market at age 21 are zero and that OAA is the only source of non-labor income. Individuals live to age 75 with certainty ($T = 75$). Any assets the individual accumulates earn a constant real

[41] In short, we calculate the distribution of wage and salary earnings for individuals aged 48–52 with positive wage and salary earnings. We assume that the (unobservable) distribution of self-employment earnings is the same as the (observable) distribution of wage and salary earnings. This is a strong assumption, but some broadly supportive evidence is that the education distribution of the self-employed in 1940 was quite similar to that of wage and salary workers.

[42] Simulations of the model indicate that OAA would have had greater effects on labor supply in 1940 had people had more time to build OAA into their plans.

return of 3 percent per year, $r = 0.03$. As described in the appendix, our main conclusions are robust to making plausible alternative assumptions.

OAA programs limited eligibility based on several criteria in addition to age and earnings. We assume that the probability that a randomly-chosen individual with potential earnings w_i is "eligible" for OAA—by which we mean that he would receive a positive OAA benefit if he met the age requirement and had no earnings—is a piecewise-linear function in the individual's potential earnings,

$$Pr(\text{eligible}_i|w_i) = \max\{0,\ \min\{1,\ \alpha_e + \beta_e w_i\}\}.$$

We estimate the parameters governing the eligibility-potential earnings relationship, α_e and β_e, using the procedure described below. This measure of eligibility—which is exclusive of the minimum age restriction and the earnings test—is meant in part to approximate the many other restrictions that individuals must have met in order to qualify for OAA, including any requirements related to citizenship, residency, housing wealth, and relatives' characteristics. We have to infer rather than simply measure an individual's eligibility for OAA because eligibility for OAA depended on many characteristics that are not available in the Census or alternative sources of data.[43] It is also important to note that in addition to being exclusive of the age and earnings tests, this notion of eligibility further departs from the standard one in that it bundles together many things that are conceptually distinct and not necessarily related to the usual meaning of the word "eligibility." For example, it includes any unmodelled factors, such as incomplete information and stigma, that limit take up of OAA benefits. In the simulations, by definition take up is universal among people who are "eligible," old enough, and have no earnings.

6.1.3 Estimation strategy and empirical inputs

We estimate the key preference parameters, η and $F(\delta)$, and the parameters governing eligibility for OAA, α_e and β_e, using the empirical relationship between earnings and the extent of "bunching" of retirements at the OAA eligibility age; the estimated distribution of retirement ages in the absence of OAA; and the empirical relationship between earnings and housing wealth, an important determinant of eligibility for OAA.

[43] For example, while the Census data include information about an individual's citizenship status and housing wealth, the data do not include information about non-housing wealth. Moreover, it is unlikely that any available data source would allow one to accurately determine whether an individual would be eligible on the basis of relatives' responsibility laws, given both the demanding data requirements involved (detailed information about the financial conditions of all of an individual's responsible relatives) and the uneven application of these requirements (Lansdale et al., 1939).

Intuitively, the estimation is based on comparing the extent of "bunching" of choices (here, retirement ages) at convex kinks in the budget set (here, the OAA eligibility age) that differ in their sharpness (due to different individuals facing different replacement rates from OAA, $\bar{y}/w$).[44] As discussed in the theory section, OAA creates a convex kink in the lifetime budget constraint relating lifetime consumption to retirement length, and this kink is sharper for people who face higher replacement rates from OAA, $\bar{y}/w$. With a smooth distribution of the disutility of work in the population, $F(\delta)$, such convex kinks lead to bunching of retirements at the OAA eligibility age, as some of the people who would have retired somewhat after the OAA eligibility age in the absence of OAA choose to hasten their retirements due to the income and substitution effects of OAA. The amount of bunching is informative about the level of eligibility for OAA, α_e. The greater the observed bunching, the greater the inferred eligibility. The speed with which the bunching "fades out" as the replacement rate declines is informative about the curvature of utility, η. The faster the fade out, the greater the curvature of utility (and so the more negative is η). In principle, all parameters can be identified on the basis of the bunching of retirements at the OAA eligibility age and the counterfactual distribution of retirement ages in the absence of OAA. But intuition suggests and estimations confirm that the slope of the eligibility-potential earnings relationship, β_e, is not well identified using this information alone (we provide further details in the appendix). This fact motivates the use of the relationship between earnings and housing wealth to estimate β_e.

In constructing the first input to the estimation, heterogeneity in "bunching" of retirements by potential earnings level, we focus on situations as similar as possible to the model by analyzing bunching in Massachusetts, a state whose OAA program appears to have closely approximated an income floor with a common level for all individuals.[45] Potential earnings (w) are unobserved for those out of the labor force, so we approximate differences across earnings groups in bunching of retirements using changes in the distribution of earnings at the OAA eligibility age. To do this we must make two strong assumptions. First, we assume that actual earnings in 1939 measures potential earnings, so that someone either earns his potential earnings or zero. Second, because the available measure of earnings does not include self-employment earnings, we assume that self-employment is independent of earnings and responds in the same way to OAA as does wage and salaried employment.[46] We create

[44]The general strategy is closely related to the bunching strategy of Saez (2010), with allowances made for the complications that arise from the possibility of binding borrowing constraints and our inability to measure eligibility in the data. These factors prevent us from being able to isolate the substitution effect from the bunching of retirements at the eligibility age.

[45]As described in more detail in the appendix, many other states had significantly more variation in benefits even for people with no other source of earnings. This fact suggests that these states either had consumption floors rather than income floors or income floors with heterogeneous income levels.

[46]For these assumptions to have much effect on our results, it would need to be that they lead us to estimate too small a response (and hence estimate too low an eligibility rate) among people with high levels

separate indicator variables for reporting 1939 wage and salary income of zero, of \$1–100, of \$101–200, and so on in multiples of 100. We then estimate equation (3), reproduced below, with an indicator for each level of earnings as a separate dependent variable.[47]

$$y_i = \beta_0 + \beta_1 \mathbf{1}(\text{age}_i \geq 65) + \beta_2(\text{age}_i - 65) + \beta_3(\text{age}_i - 65)\mathbf{1}(\text{age}_i \geq 65) + \varepsilon_i \tag{3}$$

Under our assumptions, the share of men of a given potential earnings level who retire upon reaching 65, conditional on working up until age 65, is given by β_1/β_0.[48]

The results provide further evidence that the effects of OAA on labor supply were concentrated among those with low potential earnings. The left panel of Figure 12 shows estimates of β_1, measuring breaks in the share of Massachusetts men earning each amount from zero up to \$1,800 (Appendix Figure A6 shows underlying shares by age for amounts up to \$800). The vertical line indicates an annual amount of \$360, the prevailing benefit level in Massachusetts at the time. Aging into eligibility was associated with an increase of 8 percentage points in the probability of having no wage or salary income. Hence, there was a meaningful shift of mass in the earnings distribution to an earnings level of zero. The positive levels of earnings from which this mass shifted primarily cluster at and just above the level of the income floor. The right panel of Figure 12 shows estimates of β_1/β_0. The point estimates suggest that at levels of potential earnings up to \$800 per year, about 20 percent more men retired at age 65 than would have been expected based on general trends in labor force participation by age.

The second input to the estimation is the counterfactual distribution of retirement ages in the absence of OAA. To predict this distribution, we use a slight variant of our earlier empirical strategy together with the assumption that the observed cross-sectional relationship between labor force participation and age is a good proxy for the unobserved life-cycle relationship.

of true earnings. This possibility seems unlikely. For example, self-employed people with high earnings might have assets related to their businesses that would cause them to fail OAA asset tests. Separately, although some people had both self-employment earnings and wage and salary earnings (meaning that wage and salary earnings understates their true earnings), their prevalence among people with wage and salary earnings levels above about \$300 was small (less than 10 percent).

[47] We use a uniform kernel and the Imbens and Kalyanaraman (2012) approach to select a bandwidth separately for each dependent variable. The results are robust to alternative choices on these dimensions. As in the estimation of equation (3) above, since we observe only a person's age at the time of the Census (in April 1940) we cannot determine whether a given 65 year old would have been eligible for OAA in 1939. Hence, we omit 65 year olds from the regression.

[48] Note that this approach simply measures a change at age 65 and does not necessarily difference out factors other than OAA that may have induced retirements at 65. As we discussed in Section 4, the other sources of old-age support that would have started making payments at age 65 were smaller in scale than OAA. Other old-age pensions also tended to make significantly larger payments than OAA, and hence were likely relevant for individuals higher in the income distribution. As we shall see, we find displacement primarily for lower earnings groups. To the extent that we wrongly attribute to OAA some of the bunching of retirements at age 65 among groups with higher earnings, our estimates of the value of OAA to recipients would tend to be biased downward.

We compare labor force participation profiles across state boundaries, and we assume that if OAA levels were the same in two states, the *levels* of labor force participation would be the same on either side of the boundary (whereas our main analysis only required equal *trends*). Formally, we estimate

$$y_{iacsb} = \alpha_{ba} + \sum_{a} \gamma_a * (\text{payments per person 65+})_{s \backslash c} + \varepsilon_{iacsb} \tag{4}$$

where the summation is over all age groups (that is, with no omitted age). The predicted level of labor force participation with payments per person set to zero in all states yields the counterfactual no-program relationship between age and labor force participation in Figure 13. It is noteworthy that using this approach, we find reductions in labor force participation after age 65 similar to our main estimates, and any anticipatory effects of OAA on labor supply before age 65 appear to be quite small (also consistent with our main estimates). The ability to identify the counterfactual distribution of people along the lifetime budget constraint—a key determinant of the effects of pension programs on labor supply—on the eve of the major mid-20th-century expansions in Social Security greatly facilitates an understanding of the role of government old-age support programs in reducing late-life work at this time.[49]

The third input to the estimation is the empirical relationship between earnings and housing wealth. Appendix Figure A7 shows the shares of Massachusetts men aged 60–64 who had less than \$3,000 of house value, as a function of wage and salary earnings. This reflects the share of men in these states who met the real property test for OAA eligibility, since Massachusetts limited OAA eligibility for people with more than \$3,000 of equity in real property. Massachusetts also limited OAA benefits to people who met additional conditions in addition to housing wealth and earnings, including having no relatives able to support the individual. This means that the actual share of the population eligible for OAA at any given potential earnings level was less than the share that would be eligible based on the property test alone. We focus on house value because it is the main determinant of eligibility (other than income and age) that is observable in the Census data.

6.1.4 Estimation procedure

We estimate the model in two steps. In the first stage, we estimate the slope of the eligibility-potential earnings relationship, β_e, using the empirical relationship between earnings and

[49]This is simply the standard result that the distribution of people along a non-linear budget constraint plays a key role in determining how changes in the constraint translate into changes in average behavior (e.g., Moffitt, 1986).

housing wealth. The estimate of the slope of the eligibility-potential earnings relationship, $\hat{\beta}_e$, is the slope of the empirical relationship between earnings and the fraction of people in Massachusetts with house values below the Massachusetts OAA eligibility threshold of \$3,000. The underlying assumption is that the slope of the relationship between potential earnings and eligibility for OAA based on house value alone equals the slope of the relationship between potential earnings and eligibility for OAA based on all determinants of eligibility.[50]

In the second stage of the estimation, we estimate the remaining parameters of the model, η, $F(\delta)$, and α_e, based on the bunching of retirements at the OAA eligibility age and the counterfactual distribution of retirement ages in the absence of OAA, taking as given the first-stage estimate of β_e. This stage of the estimation is based on the Method of Simulated Moments, with the target moments being the probability of retiring at the OAA eligibility age for different earnings groups in Massachusetts.[51] We weight each moment by the inverse of its variance; more-precisely estimated moments receive greater weight in the estimation. Specifically, we estimate η and α_e by attempting to match the pattern of bunching of retirements at the OAA eligibility age, while requiring that the model also match the counterfactual distribution of retirement ages in the absence of OAA (which pins down $F(\delta)$). The key assumptions are that any heterogeneity in retirement behavior among people who face the same budget constraint is due to heterogeneity in the disutility of labor and that all earnings groups have the same counterfactual no-OAA retirement distribution.[52] Further details about the model and estimation, as well as a battery of tests of the robustness of our key results, are in the appendix.

6.1.5 Estimation results and validation

The estimation is well-behaved and yields plausible results. Roughly 18 percent of the male population is estimated to be eligible for OAA, with eligibility declining from about 33 percent among those with the lowest potential earnings to about 9 percent for people with

[50]While this is a strong assumption, data constraints limit our ability to estimate a more realistic model of eligibility. As shown in the appendix, our key results are not sensitive to plausible alternative values of the slope of eligibility with respect to potential earnings, including the value that arises from estimating β_e jointly with the other parameters as part of the second-stage estimation.

[51]In applying the "bunching" results from Massachusetts, the relevant moment for the model includes *all* retirements between age 64 and age 65, not just "excess" retirements. In terms of Equation (4) the target moments are $(\beta_1 + \beta_2)/(\beta_0 - \beta_2)$, not β_1/β_0. (The difference between the two at all earnings levels is quite small.) Although we interpret these measures as conditional probabilities of retirement, nothing guarantees that these empirical moments will be non-negative, and two take negative values. This is awkward but has little effect on the results.

[52]Given our estimate of η, this assumption implies that the disutility of labor is negatively correlated with potential earnings.

potential earnings of \$2,000. Fitting a linear relationship suggests that no one with potential earnings greater than about \$2,822 would have been eligible. Both the level of eligibility and its slope with potential earnings appear to be reasonable based on what is known about OAA eligibility rules and recipiency rates. Individuals are estimated to be slightly more risk averse than they would be under log utility, as the coefficient of relative risk aversion is 1.1 ($\hat{\eta} = -1.1$), whereas under log utility it is one. This is well within the usual range reported in the literature.

The estimated model matches the targeted empirical moments well. Figure 14 shows the empirical and simulated probabilities of retiring at the OAA eligibility age conditional on not retiring before that age for people with different potential earnings levels. The model matches the overall empirical pattern well, including the result that increases in the probability of retiring at the OAA eligibility age are concentrated among groups with low potential earnings—primarily those between \$0 and about 2.5 times the OAA benefit level, or about \$900 in this case of a \$360 benefit level. The probabilities of retiring at the OAA eligibility age, especially the probabilities among groups with low potential earnings, pin down the level of the eligibility-potential earnings relationship. If everyone were eligible for OAA, nobody whose potential earnings were less than the OAA benefit level would work past the OAA eligibility age (doing so would give up leisure for no consumption benefit). In this case, the probability of retiring at the OAA eligibility age conditional on not retiring before that age would be one—about four times the observed probabilities among groups with low potential earnings of roughly 0.25. The model infers from this pattern that even among groups with low potential earnings, no more than about one-third of individuals were eligible for OAA.[53]

As a validation exercise, we simulate the relationship between age and labor force participation in 1940, which is not used directly as an input to the estimation. We find that the simulated profile matches its empirical counterpart quite closely. Simulation of the model also suggests that OAA should have reduced labor force participation by 5.5 percentage points in 1940, quite close to our reduced-form estimate of 5.7 percentage points. These and further results are reported in the appendix. Overall, the results suggest that the model can provide a useful benchmark for better understanding the value and labor-supply effects of OAA in 1940 and for predicting the effects of Social Security during the middle of the twentieth century.

[53] An alternative version of the model with perfect capital markets is highly inconsistent with the pattern of bunching of retirements at the OAA eligibility age. With perfect capital markets, the simulated breaks in the earnings distribution at the OAA eligibility age among groups with very low potential earnings are zero, since everybody in these groups who is eligible for OAA retires strictly before age 64. The importance of borrowing constraints is consistent with the poor functioning of household credit markets at the time (see e.g. Rose, 2014).

6.2 The role of OAA's earnings test in reducing labor supply and the ex-post value of OAA to recipients

The key feature of OAA that may have reduced its value to recipients is its earnings test, which implicitly taxed the earnings of eligible individuals.[54] Simulation of the model suggests that about half of the overall effect OAA had on labor force participation was due to its earnings test. Figure 15 shows the simulated age profile of labor force participation under three scenarios: no OAA, actual OAA, and a counterfactual unconditional OAA program, i.e., a program that pays the same fixed benefit regardless of the individual's current earnings. The results indicate that about 45 percent of the reduction in labor supply among men aged 65–74 was due to OAA earnings tests.

Despite the result that earnings tests were an important factor driving the effects of OAA on labor supply, further results suggest that the degree to which they reduced the value of OAA to its recipients was quite small. As a reference point for the predictions based on the structural model, it is useful to use the reduced-form results to put bounds on the welfare costs to recipients of meeting the earnings tests. Our finding that OAA reduced labor force participation among men aged 65–74 by about six percentage points suggests that about six percent of the person-years of men aged 65–74 were marginal benefit person-years, in the sense that these person-years would not have received benefits had it not been for labor-supply responses. To know how many person-years were inframarginal, we need to know the overall OAA recipiency rate in 1940 for men aged 65–74. Unfortunately we do not observe this directly, but some information is available on the age and sex of new recipients from 1936 through 1940. As we describe in more detail in the appendix, adding up flows and adjusting for aging and mortality suggests that about 17 percent of men 65–74 received OAA in 1940. Given this estimate, our labor-supply results suggest that about 35 percent (6/17) of OAA recipient-years were marginal and 65 percent (11/17) were inframarginal. As a rough approximation (ignoring discounting), this result suggests that the average value of OAA to recipients was between 65 and 100 percent of OAA benefits received: Despite the high average rates of implicit taxation imposed by OAA's earnings test, any costs of meeting the earnings test were no more than about one-third of benefits received. The reason for this relatively high lower bound on the value of OAA is that a large fraction of OAA benefit person-years were inframarginal.

In order to get a point estimate of the value of OAA benefits and the role of OAA's earnings

[54] Another important feature of OAA was its minimum age requirement, which meant that OAA payments were back-loaded to later ages. Given that borrowing constraints appear to have been important, OAA's back-loaded payment structure may have reduced the value of OAA benefits to recipients relative to a cost-equivalent transfer made earlier in life.

test in reducing labor supply, we use the estimated model to simulate the behavior and outcomes of a particular cohort of the US population—that aged 55 in 1940—under a variety of budget constraints based on state OAA programs in existence in 1940.[55] The results suggest that OAA's earnings tests had little effect on the value of OAA benefits to recipients, with the average recipient valuing \$1 of benefits equivalently to about \$0.96 of unconditional late-life income, on the upper end of the bounds calculated above.[56,57] The low cost to recipients of meeting the earnings test is driven by the fact that OAA benefits were highly concentrated among people with low potential earnings, in part because people with lower potential earnings were more likely to be eligible for OAA. The cost of meeting the earnings test is lower for people with lower potential earnings because they forgo less consumption by not working. Moreover, people with lower potential earnings tend to be less "exposed" to the earnings test since the large income effects of the program for them (a given OAA benefit buys more years' worth of earnings for people with lower potential earnings) mean that many would retire before the OAA eligibility age even if OAA did not impose an earnings test.

7 Social Security and the Rise in Retirement

Government old-age support expanded dramatically during the 1940–1960 period, both in terms of recipiency rates and benefit levels. Combined OAA and Social Security payments per person 65 and older grew by a factor of more than six, from about \$850 to more than \$5,300 in 2010 dollars. This was partly due to an expansion of OAA in the late 1940s but was mainly due to the much greater growth of Social Security, which grew from \$41 per person 65 and older in 1940 to \$677 in 1950 and \$4,644 in 1960 (all in 2010 dollars). The large effects we find of the comparatively small and highly-targeted OAA program of 1940 suggest that the combined effects of the much larger OAA and Social Security programs of

[55] These calculations assume that all states implement "income-focused" OAA programs that do not limit benefits based on assets, other than any limitations that operate through our estimated model of eligibility. They also assume that the relationship between eligibility and potential earnings is the same in all states and equal to the relationship we estimated using data on Massachusetts alone. Details of all of the calculations in this section are reported in the appendix.

[56] An alternative equivalent-variation calculation compares OAA to an increase in initial assets. The individual weakly prefers receiving a given present value transfer earlier than later, given borrowing constraints. We find that the average OAA recipient values \$1 of present value worth of OAA benefits equally to \$0.67 in initial assets. This difference between the present value of OAA benefits received and the equivalent variation of these benefits in initial assets reflects a combination of the small cost of meeting the earnings test and a fairly large cost of OAA's back-loaded payment schedule in the presence of borrowing constraints.

[57] Of course, this is only one component of a complete accounting of the welfare effects of OAA, as we have purposefully used a simple model that excludes many factors relevant for a full welfare analysis of OAA, including the taxes required to finance the program, risk and the insurance benefits of OAA, and fiscal externalities (e.g., from people substituting to OAA from other government programs such as unemployment insurance and the WPA). We have also focused entirely on men aged 65–74, the group of potential OAA recipients for whom the costs of meeting OAA's earnings test are likely to be greatest.

the 1950s and 1960s could be quite significant.

In this section, we use two approaches based on our findings about the effects of OAA in 1940 to predict the labor-supply effects of OAA and Social Security from 1940 to 1960. Our goal is not to provide a definitive point estimate. Instead, our goal is to use our findings to shed light on the probable importance of expansions in government old-age support in the broad mid-20th century trend in retirement. The first approach we use is a simple extrapolation based on our main regression results. This extrapolation implies that the large growth in combined OAA and Social Security between 1940 and 1960 would be expected to decrease labor force participation among men aged 65–74 by 9.5 percentage points, or about 70 percent of the observed reduction of 13.5 percentage points.

Our second approach uses our estimated life cycle model to try to obtain a lower-bound estimate of the importance of the growth in government old-age support from 1940 to 1960. To do so, we attempt to consistently make conservative assumptions that tend to reduce the magnitude of the implied effect. We ignore OAA entirely and focus only on Social Security. We simulate the effect of a Social Security program fixed at its 1939 characteristics, ignoring the large expansions in Social Security eligibility and benefits that occurred from 1950 onwards. This assumption is quite significant: for example, in our simulation about 42 percent of men are classified as eligible for Social Security, whereas at the end of 1959 about 67 percent of men 65–74 were actually receiving benefits (Social Security Administration, 1960). We make a conservative assumption about the share of recipients whose households would receive supplemental benefits (that is, benefits going to spouses of retired workers). Finally, using observed wage and price growth to predict wages in 1960, we assume that the cohort used in the simulation received its 1960 wages throughout its career. This reduces replacement rates from Social Security and shifts forward the latent distribution of retirement ages.

The simulation compares the predicted behavior of a single cohort of early recipients—men aged 50 in 1940—under this relatively modest version of Social Security to its predicted behavior in the absence of government old-age support.[58] The results indicate that even this

[58] We provide further details of our assumptions in the appendix. Two important caveats are worth emphasizing. First, because our comparison is to a scenario with no old-age support, program substitution from OAA to Social Security would reduce the implied effect of the program relative to the *observed* level of labor force participation in 1940 (which was already lower because of OAA). The share of Social Security-eligible men who were also eligible for OAA is likely to be slightly lower than the overall OAA eligibility share (which we estimate to be 18 percent), since the earnings of men we classify as Social Security-eligible tend to be higher than those we estimate to be OAA-eligible. A rough correction would be to suppose that about 20 percent of men who left the labor force to take up Social Security would otherwise have taken up OAA, which would suggest multiplying our estimates by about 0.8. The second caveat is that the model does not include some other factors that may have reduced late-life labor supply over this time period, such as private pensions (Stock and Wise, 1990; Samwick, 1998) and changes in the prices of leisure complements (Costa, 1998). To the extent that these other factors reduced labor supply, they might have reduced the impact of

conservative representation of Social Security would reduce labor force participation among members of this cohort from age 65 to 74 by 8.0 percentage points. By way of comparison, this is 59 percent of the 13.5 percentage point observed reduction in labor force participation of 65–74 year old men from 1940 to 1960.[59]

The results of both approaches suggest that Social Security may have accounted for a significant share of the large decline in late-life labor supply during the middle of the twentieth century. Although this is consistent with earlier extrapolations of the effects of OAA (Parsons, 1991; Friedberg, 1999), it is by no means a consensus. An important piece of evidence against this view is that the timing of the expansions of Social Security do not line up well with the timing of reductions in labor supply (e.g., Moffitt, 1987). Two features of our results have implications for this issue. First, we find that the comparatively small OAA programs of 1940 had large effects on labor supply. Second and related, our simulation results point to an important difference between the total effects of a program and the marginal effects of program expansions. We find that even programs with relatively low replacement rates are predicted to significantly decrease labor supply but that marginal increases in benefits beyond a replacement rate of 25 percent or so have a much more modest effect. For example, increasing the replacement rate from zero to one-third increases time spent in retirement by almost five times as much as further increasing it from one-third to two-thirds. A key reason for this diminishing effect of benefit increases is the diminishment of substitution effects, which tend to decrease once replacement rates reach about 25 percent due to more individuals being pushed by income effects to retire before the eligibility age. For this reason, benefits and labor supply might not track each other closely in the time series. This consideration, together with our results about the large effects of the comparatively modest OAA programs of 1940, suggests a potentially large role of Social Security and OAA in the mid-twentieth century reduction in late-life labor supply.

government old-age support on labor supply. Although explicitly modeling such factors is beyond the scope of this paper, the possibility that they reduced late-life labor supply is one of the reasons we attempt to make assumptions on other margins that tend to reduce the implied effect of Social Security.

[59]We do not attempt to evaluate the welfare costs of the Social Security earnings test to recipients. In addition to our thought experiment being a policy experiment that was never actually realized, the set of assumptions we make to understate the overall impact of Social Security unfortunately makes it difficult to sign the bias in the cost of the earnings test. On the one hand, understating benefits reduces implied replacement rates, which tends to overstate the costs of the earnings test. On the other hand, overstating real wage growth overstates the growth in the demand for retirement (given our estimated preferences), which tends to understate the costs of the earnings test by making more years of retirement inframarginal.

8 Conclusion

Many of the most important government programs transfer resources to older people and explicitly or implicitly tax their labor. In this paper, we investigate the labor supply effects of the Old Age Assistance program in 1940. OAA was a large source of government old-age support at the time—nearly one quarter of all individuals 65 and older received OAA in 1940—and it helped pave the way for many of the important social insurance programs of the present day. Even independent of its historical importance, OAA presents many valuable opportunities for learning about the effects of government old-age support programs. Like many modern programs, it had both a transfer component and a high implicit tax on labor. But unlike many modern programs, it varied significantly across states and across otherwise-similar groups of people within states. The recent availability of Census data on the full US population in 1940 makes studying OAA a particularly fruitful way to shed light on the effects of these programs.

Our results suggest that OAA caused large reductions in labor supply in 1940. The baseline estimates imply that OAA reduced labor force participation among men aged 65–74 by about 5.7 percentage points, nearly half of the observed 13 percentage point decline over the 1930s. Analysis based on an estimated life cycle model indicates that while a significant share of this reduction in labor supply was due to substitution effects from the high implicit tax rates of OAA's earnings test, the reduction in the value of benefits to recipients associated with the earnings test was quite small. Predictions based on our regression estimates and our estimated life cycle model both suggest that Social Security accounted for at least half of the large mid-century decline in late-life labor supply. Taken as a whole, our results suggest that government old-age support programs can have large effects on labor supply, through both their transfer and taxation components, but that in the case of OAA circa 1940, the costs to recipients of the high rate of implicit taxation of work were fairly small. Finally, the results also suggest that Social Security played an important role in the growth of retirement over the 20th century.

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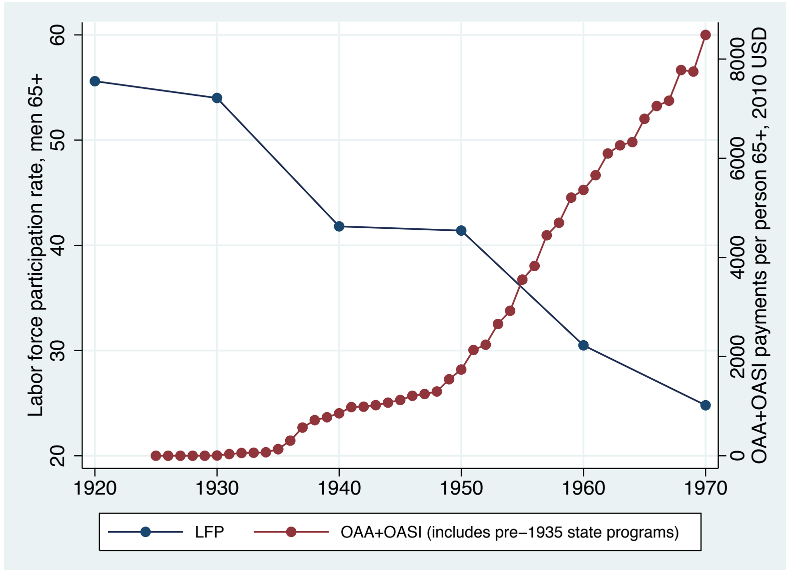
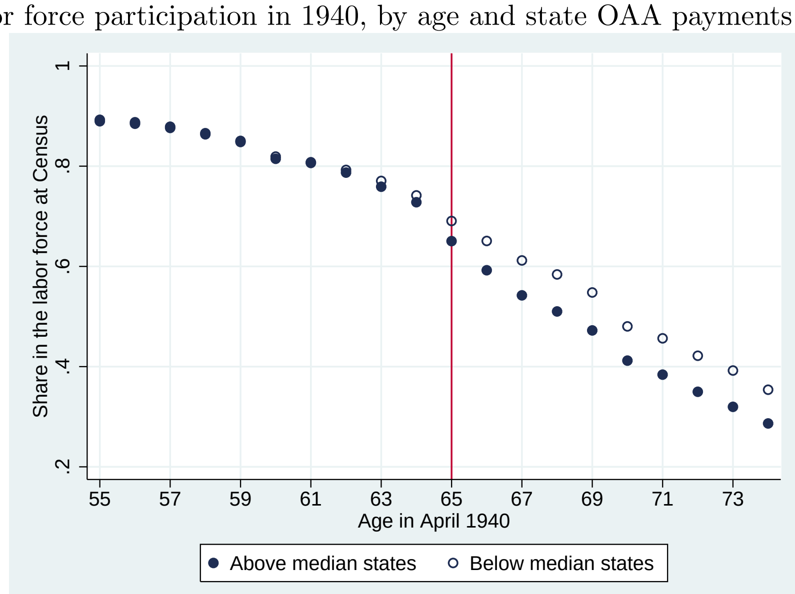
Tables and Figures

Figure 1: Government old-age support and retirement over the mid-20th century



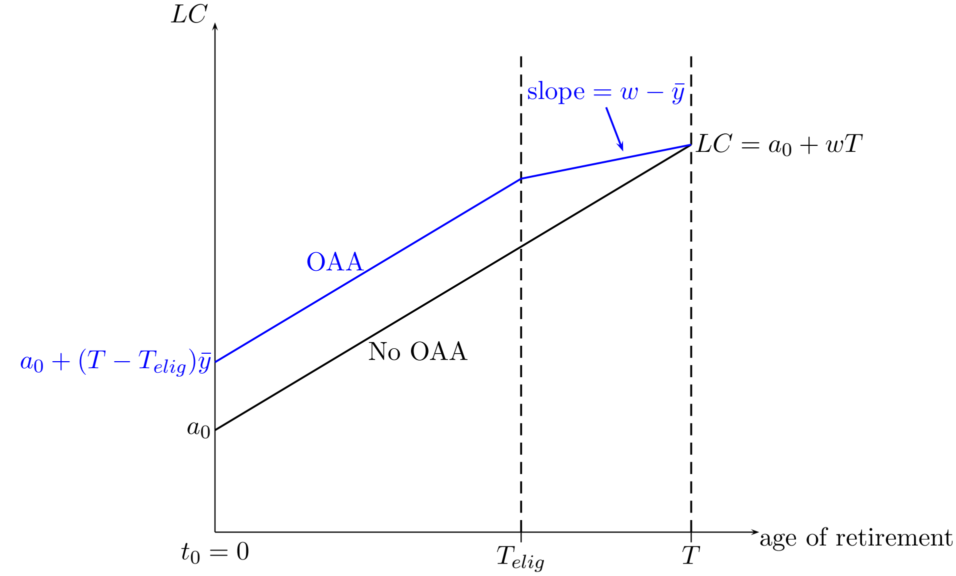
Notes: Figure shows labor force participation rate of men 65 and older, from Series D35 of U.S. Bureau of the Census (1975), and payments under Old Age Assistance (OAA) and Old Age and Survivors Insurance (OASI) per person 65 and older, in 2010 US dollars. OAA payments data come from Parker (1936) for 1925 to 1935 and Series Bf621 of Carter et al. (2006) for 1936 onwards. OASI payments data come from Series BF396 of Carter et al. (2006).

Figure 2: Labor force participation in 1940, by age and state OAA payments per person 65+



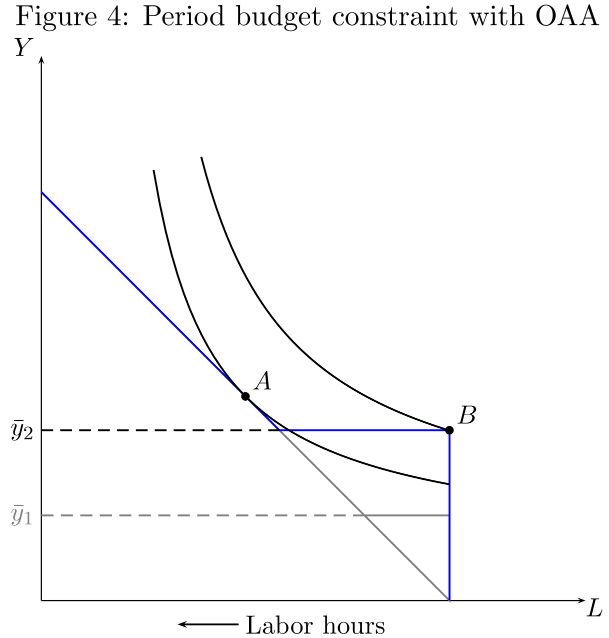
Notes: Figure shows share of men in the labor force at the time of the 1940 Census, in states with above- and below-median OAA payments per person 65+ in 1939, for states with an eligibility age of 65 in 1939.

Figure 3: Lifetime budget constraint with OAA



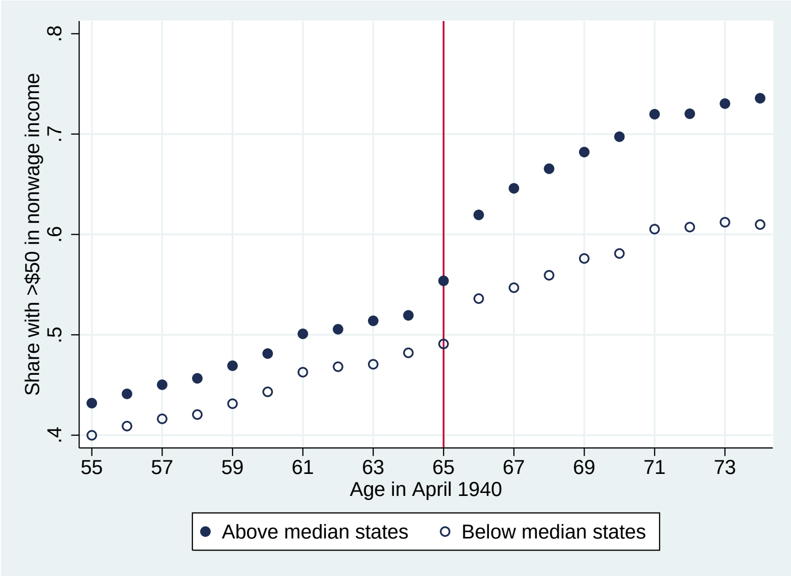
Lifetime budget constraint relating the present value of lifetime consumption (LC) to age at retirement, with and without OAA. The OAA program depicted is an income-floor program with eligibility age T_{elig}, which implicitly taxes labor earnings at a 100 percent rate from the first dollar (by phasing out benefits dollar-for-dollar with labor income). For simplicity, the figure depicts the case in which the rate of return is zero, $r = 0$.

Figure 4: Period budget constraint with OAA



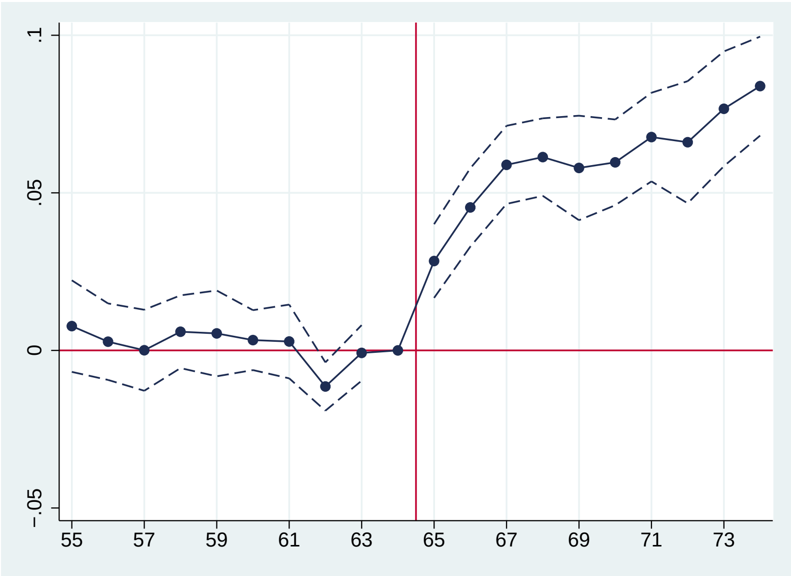
Period budget constraint relating income (Y) to leisure L, with and without OAA. The OAA program depicted is an income-floor program, which implicitly taxes labor earnings at a 100 percent rate from the first dollar (by phasing out benefits dollar-for-dollar with labor income).

Figure 5: Receipt of non-wage/salary income in 1939, by age and state payments per person 65+



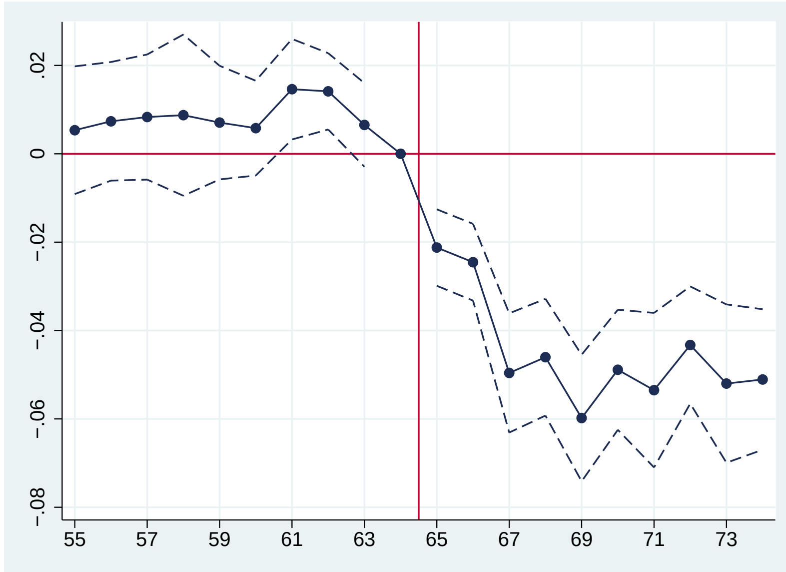
Notes: Figure shows share of men receiving more than $50 in non-wage income in 1939 in states with above- and below-median OAA payments per person 65+ in 1939, for states with an eligibility age of 65 in 1939.

Figure 6: Regression estimates for receipt of non-wage/salary income in 1939



Notes: Figure shows point estimates and 95% confidence intervals on age-payment interactions from estimation of equation (2) on border county sample. Standard errors clustered at the state level. $N = 2178112$.

Figure 7: Regression estimates for labor force participation in 1940

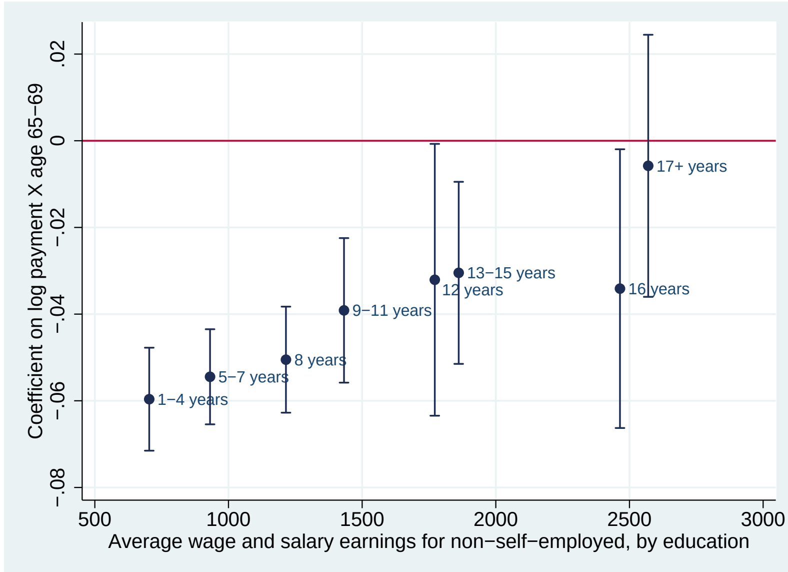


Notes: Figure shows point estimates and 95% confidence intervals on age-payment interactions from estimation of equation (2). Standard errors clustered at the state level. $N = 2334689$.

Figure 8: Breaks in non-wage/salary income versus breaks in labor force participation, by state

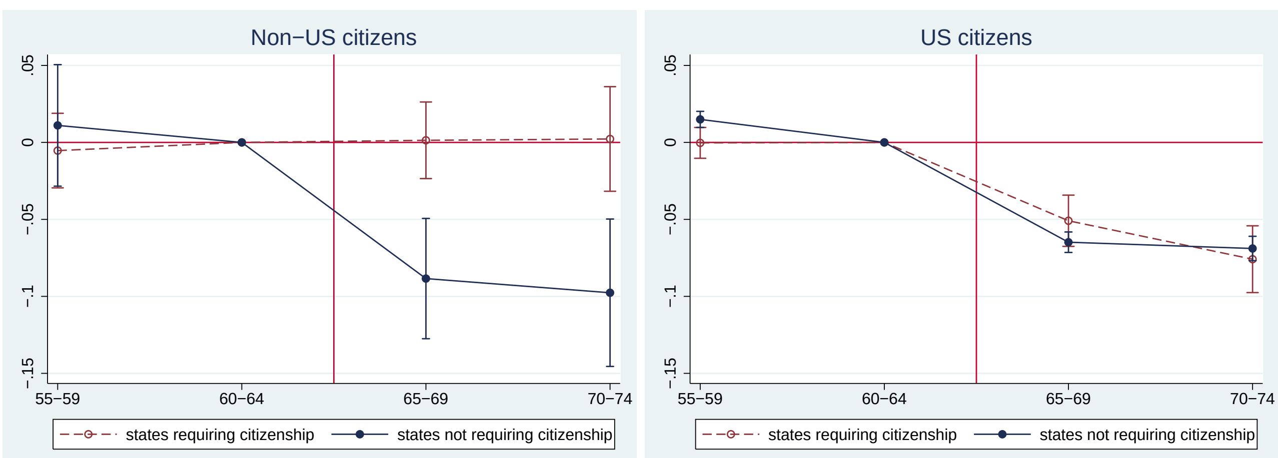
Notes: Figure shows point estimates from estimation of equation (3) for receipt of non-wage income in 1939 against estimates for labor force participation in 1940, separately by state. Sample: men aged 56-64 or 66-73 at 1940 Census, in states with an eligibility age of 65 in 1939; breaks in receipt of non-wage income estimated on sample of men with non-missing 1939 income information ($N = 5277150$) and breaks in labor force participation estimated on sample of men with non-missing 1940 labor force participation information ($N = 5649733$).

Figure 9: OAA reduced labor supply more for men with lower levels of education



Notes: Figure shows estimates of age 65-69 interaction in equation (2) separately by grouped years of education. Education groups arranged horizontally by average 1939 wage and salary earnings for 45-54 year old men who were not self-employed at the time of the Census.

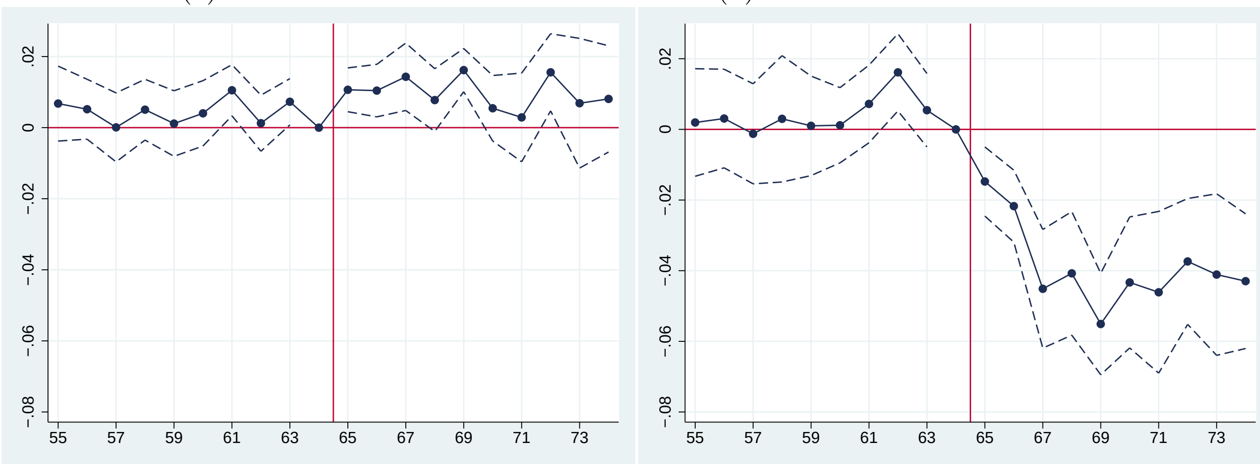
Figure 10: OAA and labor force participation by citizenship and state citizenship requirements



Notes: Figure shows point estimates and 95% confidence intervals from estimation of equation (1) separately by US citizenship status, grouping ages into 5-year bins. 'States requiring citizenship' are those limiting eligibility to US citizens in both 1939 and 1940. 'States not requiring citizenship' are those with no requirement for citizenship or long-term residency in the United States in either 1939 or 1940. In both cases sample is limited to men aged 55 to 74 in states with an eligibility age of 65 in 1939, and with non-missing rest-of-state payments per person 65+. Specification is on full sample, with Census region by age group interactions. For non-citizens in states requiring citizenship $N = 296189$, for non-citizens in states not requiring citizenship $N = 49016$. For citizens, samples sizes are $N = 3876204$ and $N = 1778002$ respectively.

Figure 11: 1940 OAA payments are not associated with differential age trends in 1930

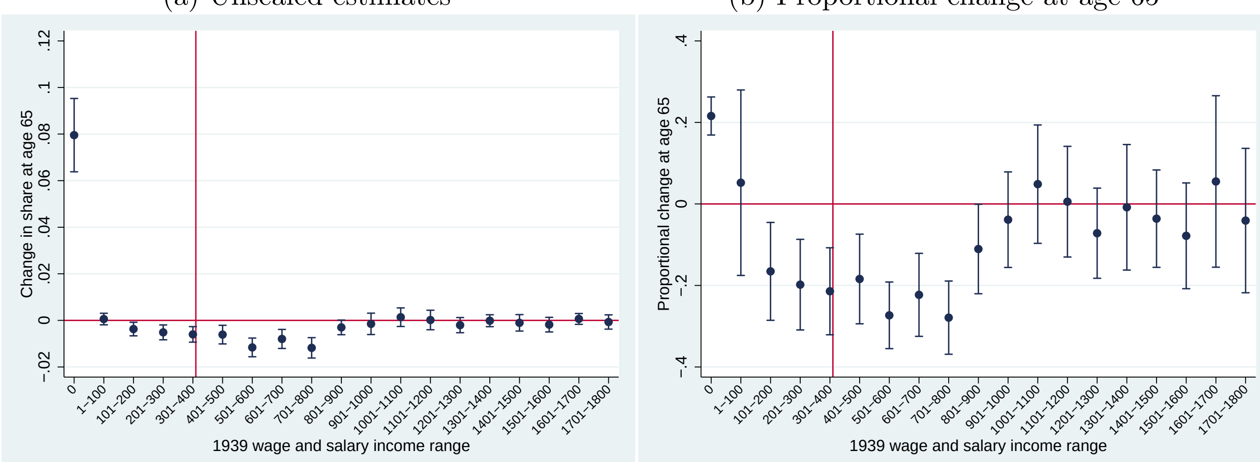
(a) 1930 coefficients (b) 1940 coefficients for same states



Notes: Panel (a) shows point estimates and 95% confidence intervals on age-payment interactions from estimation of equation (2) using 'gainful employment' in 1930 as the outcome and 1940 rest-of-state payments as the payment variable. Sample includes only those states that had no old-age assistance program in 1930. For comparison, panel (b) shows analogous estimates for 1940 labor force participation for the same sample of states. Standard errors clustered at the state level. For 1930 coefficients $N = 1578523$, for 1940 coefficients $N = 1893835$.

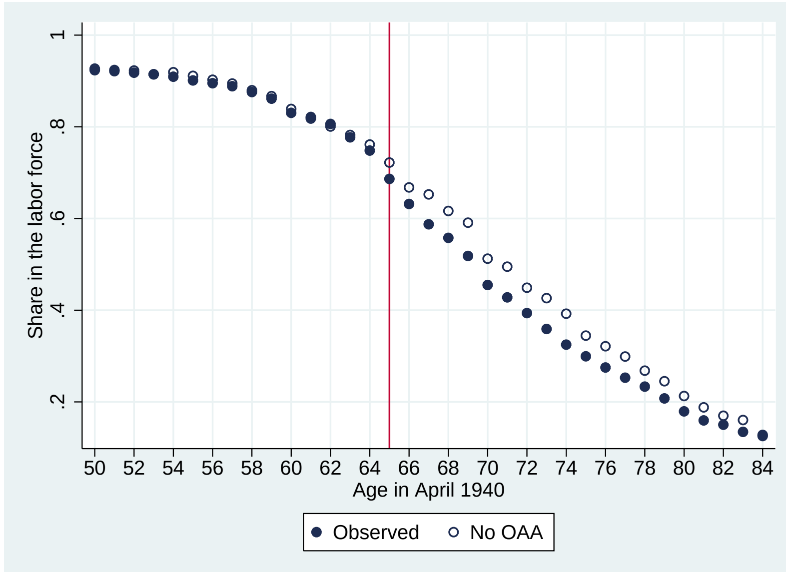
Figure 12: Change at 65 in share of men with specified amount of wage/salary income in 1939

(a) Unscaled estimates (b) Proportional change at age 65



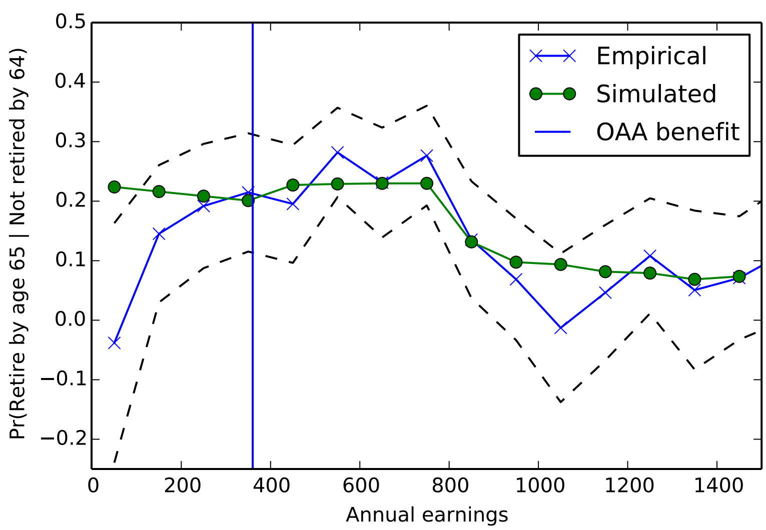
Notes: Figures show point estimates and 95% confidence intervals from separate estimations of equation (3), with dependent variable indicating wage/salary earnings of each specified amount in 1939. Sample: men within IK bandwidth around age 65 at 1940 Census in Massachusetts. Vertical line denotes 'income floor' of \$360 per year. Standard errors clustered by years of age. Panel (a) shows estimates of β_1; Panel (b) shows estimates of β_1/β_0 to measure proportional change at age 65 (with standard errors calculated using the delta method).

Figure 13: Actual and counterfactual no-OAA profile of labor force participation



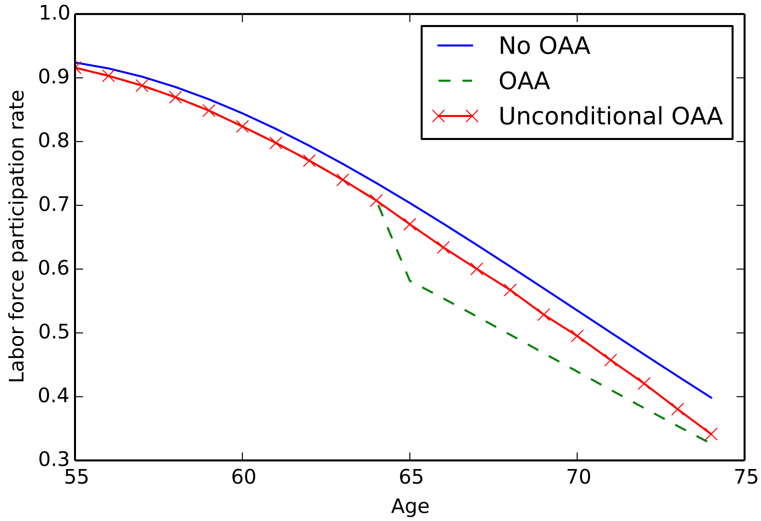
Notes: Figure shows observed rates of labor force participation by age and estimated counterfactual rates of labor force participation in the absence of OAA, based on estimates of equation (4).

Figure 14: Empirical vs. simulated moments



Notes:
Empirical vs. simulated moments. The moments are the probability of retiring at the OAA eligibility age, 65, conditional on not having before reaching the OAA eligibility age. Empirical moments correspond roughly to the breaks at age 65 in the share of men with the specified amount of wage/salary income in Massachusetts in 1939. The vertical line corresponds to the maximum OAA benefit in Massachusetts, $360 per year.

Figure 15: Simulated effects of OAA on labor force participation



Notes: Simulated life cycle labor force participation profiles of the cohort of people aged 55 in 1940 in the US under different OAA programs. The policy underlying the "Unconditional OAA" profile is a counterfactual OAA program that did not impose any means tests (such as tests of income and labor earnings) that reduced the return to late-life work.

Table 1: Basic features of state OAA programs

	Mean	Median	SD	Min	Max	N
OAA recipiency rate, December 1939	0.23	0.23	0.09	0.08	0.49	49
OAA payment per recipient, December 1939	17.93	18.90	6.49	6.01	32.97	49
OAA payment per person 65+, December 1939	4.16	3.59	2.59	1.01	13.17	49
Legal maximum payment	29.38	30	5.34	15	45	41
95th percentile payment	28.26	30	5.78	15	45	49
95th percentile payment, states with legal maximum	27.96	30	5.49	15	45	41

Notes: Includes the 48 states and the District of Columbia. '95th percentile payment' is for new recipients in fiscal year 1938-39. Eight states had no legal maximum payment. Recipiency rate and payments per person 65+ are normalized by state population from 1940 Census. Sources: data on OAA dollar payments and number of recipients from U.S. Social Security Board (1940*b*), data on legal maximum payments from U.S. Social Security Board (1940*a*), data on 95th percentile payment from U.S. Social Security Board (1939*b*).

Table 2: Summary statistics

	Full sample			Border county sample		
	Mean	SD	N	Mean	SD	N
Years of education	7.142	3.74	6722869	7.011	3.724	2000227
Completed primary school	.547	.498	6722869	.528	.499	2000227
Non-white	.079	.269	6722869	.089	.285	2000227
US citizen	.946	.227	6722869	.953	.211	2000227
Currently married	.755	.43	6722869	.756	.43	2000227
In the labor force	.713	.452	6722869	.725	.447	2000227
Employed	.651	.477	6722869	.666	.472	2000227
Employed, non-emergency work	.616	.486	6722869	.631	.483	2000227
Worked in 1939	.72	.449	6283146	.73	.444	1865908
Any wage/salary income in 1939	.48	.5	6283146	.478	.5	1865908
Wage/salary income in 1939	557	911	6283146	550	906	1865908
$\geq$\$50 in non-wage/salary income	.516	.5	6283146	.519	.5	1865908

Full sample: men aged 55-74 in states with 1939 eligibility age of 65 with non-missing demographic information (education, race, birthplace, citizenship, and marital status). For demographic variables and 1940 labor force and employment variables (reflecting labor force status in last week of March 1940), sample restricted to men with non-missing information on labor force status and non-missing demographic information. For 1939 employment and income variables, sample restricted to men with non-missing information for all 1939 employment and income variables and non-missing demographic information. State border county sample further limits to counties that border a state included in the sample.

Table 3: Receipt of non-wage income by state payments per person 65+ and age

	(1)	(2)	(3)	(4)	(5)	(6)
Log per-65+ payment	-0.010**	-0.008	-0.006	-0.007	0.005	0.006
× age 55-59	(0.003)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Log per-65+ payment	0.063***	0.057***	0.058***	0.064***	0.050***	0.051***
× age 65-69	(0.004)	(0.007)	(0.006)	(0.006)	(0.004)	(0.004)
Log per-65+ payment	0.092***	0.089***	0.091***	0.092***	0.070***	0.070***
× age 70-74	(0.009)	(0.009)	(0.010)	(0.009)	(0.005)	(0.006)
Observations	6252698	6252698	6252698	2178112	2178112	2178112
Sample	full	full	full	border	border	border
Census region × age fixed effects	no	yes	yes	no	no	no
Border segment × age fixed effects	no	no	no	no	yes	yes
Education × age fixed effects	no	no	yes	no	no	yes
Race × age fixed effects	no	no	yes	no	no	yes

Dependent variable: receipt of more than \$50 in non-wage income in 1939. Sample for columns (1)-(3): men aged 55-74 in states with 1939 eligibility age of 65 and non-missing rest-of-state payments per person 65+. Columns (4)-(6) include only counties on state boundaries and exclude counties on borders of excluded states. Unit of observation in columns (4)-(6) is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table 4: Labor force participation by state payments per person 65+ and age

	(1)	(2)	(3)	(4)	(5)	(6)
Log per-65+ payment	0.018***	0.016**	0.016**	0.017***	-0.000	0.001
× age 55-59	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)
Log per-65+ payment	-0.060***	-0.060***	-0.063***	-0.059***	-0.047***	-0.047***
× age 65-69	(0.004)	(0.007)	(0.007)	(0.006)	(0.005)	(0.004)
Log per-65+ payment	-0.067***	-0.069***	-0.074***	-0.071***	-0.058***	-0.059***
× age 70-74	(0.008)	(0.008)	(0.008)	(0.009)	(0.006)	(0.006)
Observations	6687910	6687910	6687910	2334689	2334689	2334689
Sample	full	full	full	border	border	border
Census region × age fixed effects	no	yes	yes	no	no	no
Border segment × age fixed effects	no	no	no	no	yes	yes
Education × age fixed effects	no	no	yes	no	no	yes
Race × age fixed effects	no	no	yes	no	no	yes

Dependent variable: in labor force at 1940 Census. Sample for columns (1)-(3): men aged 55-74 in states with 1939 eligibility age of 65 and non-missing rest-of-state payments per person 65+. Columns (4)-(6) include only counties on state boundaries and exclude counties on borders of excluded states. Unit of observation in columns (4)-(6) is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table 5: Alternative labor force participation outcomes by state payments per person 65+ and age

	(1) In labor force	(2) Employed	(3) Non-emergency	(4) In labor force	(5) Employed	(6) Non-emergency
Log per-65+ payment	0.017***	0.016***	0.018***	0.001	0.001	-0.000
× age 55-59	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Log per-65+ payment	-0.059***	-0.044***	-0.031***	-0.047***	-0.036***	-0.025***
× age 65-69	(0.006)	(0.006)	(0.005)	(0.004)	(0.005)	(0.005)
Log per-65+ payment	-0.071***	-0.048***	-0.036***	-0.059***	-0.046***	-0.035***
× age 70-74	(0.009)	(0.007)	(0.007)	(0.006)	(0.005)	(0.004)
Observations	2334689	2334689	2334689	2334689	2334689	2334689
Sample	border	border	border	border	border	border
Census region × age fixed effects	no	no	no	no	no	no
Border segment × age fixed effects	no	no	no	yes	yes	yes
Education × age fixed effects	no	no	no	yes	yes	yes
Race × age fixed effects	no	no	no	yes	yes	yes

Dependent variables: in labor force at 1940 Census (1 and 4), employed at 1940 Census (2 and 5), employed in private or non-emergency government work (3 and 6). Sample restricted to counties on state boundaries, excluding counties on borders of states with age eligibility requirement other than 65 in 1939. Unit of observation is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table 6: Test for heterogeneous labor force participation effects by county age 45-54 unemployment

	(1)	(2)
Unemployment rate × log per-65+ payment × age 55-59	-0.039	-0.036
	(0.046)	(0.048)
Unemployment rate × log per-65+ payment × age 65-69	0.027	0.043
	(0.088)	(0.084)
Unemployment rate × log per-65+ payment × age 70-74	0.208	0.215
	(0.132)	(0.131)
Log per-65+ payment × age 55-59	0.003	0.003
	(0.007)	(0.007)
Log per-65+ payment × age 65-69	-0.046***	-0.049***
	(0.011)	(0.011)
Log per-65+ payment × age 70-74	-0.080***	-0.082***
	(0.017)	(0.016)
Observations	2332847	2332847
Sample	border	border
Border segment × age fixed effects	yes	yes
Education × age fixed effects	no	yes
Race × age fixed effects	no	yes

Dependent variable: in labor force at 1940 Census. Sample: men aged 55-74 in states with 1939 eligibility age of 65 and non-missing rest-of-state payments per person 65+, including only individuals in counties on state boundaries. All specifications include county fixed effects, 5-year age group fixed effects, interactions of age group effects with the unemployment rate, and border segment by age fixed effects. Unemployment rate is that of 45-54 year old men living in the individual's county and includes work relief in unemployment. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table 7: IV specifications using 95th percentile payment

	(1) Non-wage income	(2) In labor force	(3) Employed	(4) Non-emergency
Log per-65+ payment	0.016***	0.005	0.009*	0.004
× age 55-59	(0.004)	(0.004)	(0.004)	(0.004)
Log per-65+ payment	0.060***	-0.058***	-0.045***	-0.026***
× age 65-69	(0.006)	(0.006)	(0.006)	(0.007)
Log per-65+ payment	0.070***	-0.069***	-0.057***	-0.035***
× age 70-74	(0.007)	(0.009)	(0.008)	(0.007)
Observations	2178112	2334689	2334689	2334689
Kleibergen-Paap rk Wald F-stat	12.69	13.02	13.02	13.02
Sample	border	border	border	border
Census region × age fixed effects	no	no	no	no
Border segment × age fixed effects	yes	yes	yes	yes
Education × age fixed effects	yes	yes	yes	yes
Race × age fixed effects	yes	yes	yes	yes

Dependent variables: receipt of non-wage income in 1939, in labor force at 1940 Census, employed at 1940 Census, employed in private or non-emergency government work. Log 95th percentile payment by age interactions used as instruments for log per-65+ payment by age interactions. Sample restricted to counties on state boundaries, excluding counties on borders of states with age eligibility requirement other than 65 in 1939. Unit of observation is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

A Appendix

A.1 Estimation

A.1.1 The OAA budget constraint in Massachusetts and other states

The main input to the estimation is the empirical relationship between earnings and the extent of "bunching" of retirements at the OAA eligibility age. As discussed in the main text, to focus on situations as similar as possible to the model, we analyze the case of Massachusetts, a state whose OAA program appears to have closely approximated an income floor with a common level for all individuals. Although most OAA laws set benefits as the difference between "needs" and "resources," which would suggest a consumption or an income floor, to the extent that "needs" varied across people according to unobserved characteristics, it need not have been the case that the resulting floor was at the same level for all individuals, as is assumed in the model. In practice, in many states payments varied substantially even across people with no other source of earnings. This issue is illustrated in Figure A5, which is based on data from U.S. Social Security Board (1939*b*). In Ohio, among new recipients in 1939, only about 10 percent of payments were at the legal maximum of \$30 per month, even among recipients with no other source of income. (We do not directly observe payments to those with no other source of income, but rather the unconditional distribution of payments and the share of recipients with no other source of income. We assume that the recipients with other source of income received the lowest payments.) However, a few states did have programs that more closely resembled an income floor set at a common level across people. As examples, California and Massachusetts had legal minimum amounts for the sum of income and benefits. For recipients with no other source of income, these states saw payments cluster right around this minimum. For new recipients in Massachusetts in 1939, for example, close to 70 percent of recipients with no other source of income received payments of \$30 per month. California had an even clearer income floor—its program specified both a maximum and minimum income plus benefit of \$35 per month—but also had a \$15 earnings disregard that slightly complicates the nature of the budget constraint relative to the model setup. Although we do not find any apparent effects of the earnings disregard in California, we focus on the simpler program in Massachusetts to estimate the parameters of the model.

A.1.2 Estimating the latent retirement distribution

We estimate the curvature of utility of consumption, η, and the intercept of the eligibility-potential earnings relationship, α_e, by attempting to match the pattern of bunching of retirements at the OAA eligibility age, while also requiring that the distribution of the disutility of work, $F(\delta)$, be such that the model matches the counterfactual distribution of retirement ages in the absence of OAA. The key assumptions are that all heterogeneity in retirement behavior among people who face the same budget constraint is driven by heterogeneity in the disutility of labor and that all potential earnings groups have the same counterfactual no-OAA retirement distribution. We estimate the $F(\delta)$ distribution by using the model to invert the (counterfactual) distribution of retirements without OAA.

The Census data do not contain all of the information necessary to construct individuals' lifetime budget constraints. For example, the data contain only incomplete information about assets (just housing wealth) and non-labor income (just an indicator about whether it exceeds \$50 per year). This means that unobserved heterogeneity in assets or non-labor income could help explain the observed heterogeneity in labor supply among people who share the same observable components of lifetime budget constraints. Given OAA eligibility rules, however, assets and non-labor income are likely to be quite limited among the population of people potentially eligible for OAA.

In order to estimate the full distribution of the disutility of work, $F(\delta)$, we need to know the full latent retirement distribution, out to the maximum age at which the person with the lowest disutility of labor would work if he could. In the model, everyone lives to exactly age 75 and so cannot work beyond that age. So for any given budget constraint, there exists a range of δ values that lead the individual to work until age 75: from the threshold δ such that the individual is just indifferent between retiring at age 74 and 75 down to $\delta = 0$ (people to whom work provides no disutility and so would continue working as long as possible). People with low enough δ values would work longer if they could. They can be said to have a negative latent demand for retirement, where the latent demand for retirement is the number of years an individual would choose to enjoy leisure (not work) were it possible to consume negative amounts of leisure, i.e., to work longer than one's full lifetime. Working longer than one's lifetime has the benefit of increasing consumption through higher earnings and the cost of incurring the disutility of work in the "extra" periods. The latent retirement distribution is fundamentally unobservable, and the data become progressively less informative about this object at greater ages due to the small number of individuals at these ages and the bias induced by selective survival. We therefore use the estimated relationship between labor force participation and age from age 50 to 84 to fit a polynomial out to the age at which labor force participation becomes zero. This polynomial serves as our estimated distribution of latent retirement ages, from which we infer the distribution of the disutility of labor, $F(\delta)$. An important assumption implicit in this procedure is that the cross-sectional relationship between labor force participation and age is similar to what the age profile of retirements would have been for a single cohort (had government policies and other factors been held constant at their 1940 values).

A.1.3 Our application of the Method of Simulated Moments

The Method of Simulated Moments estimator is the parameter vector $\theta \equiv (\eta, \alpha_e, F(\delta))$ that minimizes the distance between the model-simulated moments and their empirical counterparts, where distance is measured by a classical minimum distance objective function. In the baseline specification we estimate η and α_e by attempting to match the pattern of bunching of retirements at the OAA eligibility age, while at the same time requiring that $F(\delta)$ be such that the model matches the counterfactual distribution of retirement ages in the absence of OAA.

Given a candidate parameter vector θ, we simulate the 15 moments (one for each of the 15 potential earnings groups whose probability of retiring at the OAA eligibility age we estimate) using the following procedure. First, we simulate the retirement ages of a large sample of individuals. This involves drawing an individual's potential earnings, disutility of work, and eligibility for OAA, and then calculating the individual's optimal retirement age. Second, we aggregate the simulated data into moments. The simulated moment for potential earnings group W (where W is the range of potential earnings corresponding to a particular moment) is

$$E[\mathbb{1}(T_r^*(i) = 64 \mid i \in I(W, 64))],$$

where $I(W, 64)$ is the set of people in potential earnings group W who retire at least as late as age 64 (which is the age immediately before the OAA eligibility age of 65). We estimate this moment with the sample average

$$\frac{1}{n(I(W, 64))} \sum_{i \in I(W,64)} \mathbb{1}(T_r^*(i) = 64),$$

where $n(I(W, 64))$ is the number of people in the set $I(W, 64)$.

In practice, for computational feasibility we discretize both the potential earnings and disutility of work distributions. We assume that potential earnings take one of 15 values corresponding to the midpoint of the ranges that we use for estimating the empirical moments. For each candidate vector of parameter values, θ, and for each of the 15 possible potential earnings levels, w, we construct the simulated moment condition in the following way. First, we calculate the disutility of work distribution, $F(\delta; w, \eta)$. The $F(\delta; w, \eta)$ distribution is that which matches the counterfactual no-OAA retirement age distribution (predicted using variants on our main regressions), given potential earnings and the curvature of utility of consumption, w and η. Because time is discrete in the model, any given (discrete) retirement age is consistent with a range of δ values. We use the midpoint of these ranges. For each of these δ values, we calculate the optimal (discrete) retirement ages for people eligible and ineligible for OAA, $T_r^*(O; w, \bar{y}, \eta, \delta)$ and $T_r^*(N; w, \eta, \delta)$, respectively. We use these mappings from δ to optimal retirement ages with and without OAA together with the disutility of work distribution, $F(\delta; w, \eta)$, to calculate the full distributions of retirement ages with and without OAA for this potential earnings group, $F(T_r^*(O; w, \bar{y}, \eta, \delta))$ and $F(T_r^*(N; w, \eta, \delta))$, respectively. We use these distributions together with the fraction of people in this potential earnings group eligible for OAA, $Pr(\text{eligible}_i | w_i; \alpha_e, \beta_e)$, to calculate the overall retirement age distribution among this group, including both eligible and ineligible individuals, $F(T_r^*(w, \bar{y}, \eta, \delta))$. Finally, we use this retirement age distribution to calculate this potential earnings group's simulated moment,

$$E[\mathbb{1}(T_r^*(i) = 64 \mid i \in I(W, 64))] = \frac{Pr(T_r^*(i) = 64 \mid i \in I(W, 64))}{Pr(T_r^*(i) \geq 64 \mid i \in I(W, 64))}.$$ [60]

The objective function is

$$g_N(\theta)' \; W \; g_N(\theta),$$

where $g_N(\theta)$ is the vector of moment conditions (whose elements are the differences between the empirical and simulated moments) and W is a positive definite weighting matrix. Pakes

[60] This moment would be undefined if $Pr(T_r^*(i) \geq 64 \mid i \in I(W, 64))$, but this never happens as long as eligibility is strictly less than 100 percent, since a strictly positive fraction of people ineligible for OAA work at least to age 64 (more than 70 percent, in fact) according to the counterfactual no-OAA retirement age distribution.

and Pollard (1989) and Duffie and Singleton (1993) show that the MSM estimator, $\hat{\theta}$, is consistent and asymptotically normally distributed under regularity conditions satisfied here. For our weighting matrix, we follow Pischke (1995) and use the inverse of the diagonal of the estimated variance-covariance matrix of the second-stage moment conditions.

A.1.4 Identification

Figure A8 plots the objective function. The figure reveals that the model is well-identified; moving away from the estimates along any dimension of the parameter vector increases the mismatch between the simulated and empirical moments, as measured by the classical minimum distance-type objective function. If instead of estimating the slope of the eligibility-potential earnings relationship using the observed relationship between earnings and house value (as we do in the baseline specification) we estimate the slope of the eligibility-potential earnings relationship together with the other key parameters in the second stage of the estimation, the model is not as well identified. In this case, the estimation has a hard time distinguishing the source of the fadeout in the bunching of retirements in potential earnings between curvature in the utility function (η) on the one hand and declining eligibility with potential earnings on the other (β_e). This is why we invoke other evidence (the observed relationship between earnings and house value) to estimate the slope of the eligibility-potential earnings relationship in our baseline specification. Fortunately, as discussed in Section A.4, the key results are not sensitive to this choice.

A.2 Validation

The key input to the estimation is the bunching of retirements at the OAA eligibility age of groups who face different replacement rates from OAA. This is a relatively sparse set of statistics, which leaves us with a variety of alternative statistics that could be used to validate the model. A natural choice is the cross-sectional relationship between labor force participation and age in 1940. Figure A9 plots this relationship. The "No OAA" profile shows the counterfactual no-OAA profile predicted based on our regression results and presented in Figure 13. The "OAA" profile is the part that is relevant for testing the model. It is simulated based on the estimated model and can be compared to its empirical counterpart, which is depicted in Figure 13. The model captures the key features of the data well and provides a fairly close fit quantitatively. The model predicts a roughly 5.4 percentage point reduction in average labor force participation over the ages 65–74, whereas our regression analysis indicated a 5.7 percentage point reduction. A relatively minor difference between the model and the data is in labor force participation at ages younger than the OAA eligibility age. The model predicts small but noticeable anticipatory effects in the years before OAA eligibility, whereas there is relatively little evidence of anticipatory effects based on our regression analysis. The close match between the model and the empirical evidence of the effects of OAA on labor supply (including the good fit of the simulated to the empirical moments) suggests that the model may be capturing some of the key factors that determine the labor-supply effects of OAA and so may be useful for understanding the effects of OAA and predicting the effects of the early Social Security program.

A.3 Simulations of the Effects of OAA and Social Security

This section presents details of the calculations underlying the simulations of the life cycle model discussed in Section 6 and Section 7. The goals of these calculations are to understand the observed effects of OAA (in particular, the value of OAA to recipients and the extent to which the labor-supply effects of OAA are due to income vs. substitution effects) and to forecast the effects of Social Security. To this end, we simulate the model under various policies and calculate statistics of the simulated data. The key statistics concern the predicted effects of OAA and Social Security on retirement, the equivalent variation of OAA, and the income and substitution effects of OAA.

A.3.1 Simulating the effects of OAA

We simulate the effects of OAA as it existed in 1940 on the cohort aged 55 in 1940. The key ingredient of the simulation is the joint distribution of potential earnings and potential OAA benefit levels among this cohort. Each individual's potential OAA benefit is the 95th percentile OAA benefit in 1940 in his state. For the distribution of potential earnings among individuals in a particular state, we use the observed distribution of earnings in 1940 among people aged 48–52 with positive earnings in that state. We further assume that potential earnings are constant over the life cycle.

Given the subsequent changes in OAA over the 1940s (most of which increased OAA benefits), this simulation is not representative of the actual experience of any one cohort. Instead, it is meant to answer the question of what effects OAA would have been expected to have had it remained as it was in 1940.

A.3.2 Simulating the effects of Social Security

We simulate the effects of a counterfactually-modest Social Security program on the cohort of men aged 50 in 1940. The goal of this exercise is not to simulate the actual experience of this cohort. The goal is to simulate a simple counterfactual in which Social Security would be expected to have smaller effects than it actually did in order to estimate a lower bound of Social Security's likely effects.

This simulation requires two key inputs. One is Social Security program rules. We base our counterfactual Social Security program on the Social Security program as of the 1939 Amendments, which implied much lower eligibility and benefit levels than members of this cohort actually enjoyed due to subsequent expansions in Social Security. Total household benefits were the sum of primary benefits (for the worker) and supplementary benefits (for spouses and dependent children), up to a maximum of \$85 or 80 percent of the average monthly wage (AMW), whichever was smaller. The primary monthly benefit was the sum of (i) 40 percent of the first \$50 of the AMW plus 10 percent of the amount by which the AMW exceeds \$50 up to an AMW of \$250 and (ii) 1 percent of the amount in (i) multiplied by the number of years in which the individual earned at least \$200 in covered employment. The minimum primary benefit was \$10. Supplementary benefits for aged spouses and dependent children were one half of the primary benefit per person. We assume that only 50 percent of men qualify for supplemental benefits, whereas about 70 percent of 65–74-year-old men in 1940 were married. We assume that everyone had 15 years of covered employment regardless of when they retired. Taxes were 1 percent of covered earnings.

As of the 1939 Amendments, eligibility for Social Security was limited to workers in commerce and industry (except railroads), and excluded farm and domestic workers and non-farm self-employed, among others. We assume that only those individuals whose 1940 occupations were covered by Social Security as of the 1939 Amendments were eligible, thereby ignoring the large expansions in coverage during the 1950s and ruling out the possibility that more people worked in covered occupations after 1940. We estimate the share of people aged 48–52 with positive earnings who were in occupations in 1940 that were covered by Social Security as of 1939. We follow Wendt (1938) to determine which workers were eligible for Social Security under its original provisions. When Census information on occupation and industry is too coarse we make assumptions that tend to reduce the estimated share eligible. These classifications imply that about 42 percent of this cohort is eligible for our counterfactual Social Security program, whereas as of the end of 1959, 67 percent of men aged 65–74 were actually receiving benefits, based on our calculations from the Census and Social Security Administration (1960).

The other key input to the simulation is the wage histories of people eligible for Social Security. Because self-employment was not covered by Social Security under the 1939 Amendments, the lack of information on the amount of self-employment earnings in the 1940 Census does not require us to make any assumptions about this missing information for the purposes of this simulation. We assume that an individual's average real and nominal monthly wages over his entire career were respectively 1.7 and 3.6 times their levels in 1940. These are the real and nominal wages that the individual would have received in 1960—at the very end of his career—had he received the average rate of wage growth from 1940 to 1960 among production workers in manufacturing (Carter et al., 2006, Series Ba4362). To the extent that this rate of wage growth was high relative to wage growth overall during the Great Compression of the 1940s and 1950s (Goldin and Margo, 1992), it will tend to overstate wage growth of this cohort overall. More important, assuming that members of this cohort received their 1960 wages over their entire careers leads us to significantly overstate their lifetime wages. Overstating nominal wages from 1939 until retirement understates the predicted effects of Social Security on labor supply by understating Social Security replacement rates. Under the 1939 Amendments, Social Security benefits were a concave function of average monthly (nominal) wages from 1939 until retirement, so higher nominal wages translated into lower replacement rates and so smaller effects of Social Security on labor supply. Overstating real wages over an individual's career understates the predicted effects of Social Security on labor supply for two reasons. First, it understates Social Security replacement rates. Second and more important, it overstates the effects of real wage growth on the demand for retirement. Given the estimated preferences, higher real wages increase the demand for retirement and so reduce optimal retirement ages. This tends to reduce the effects of Social Security on the key statistic we simulate, labor force participation among people aged 65–74, since it reduces the fraction of people who would otherwise (in the absence of Social Security) retire after age 65. One reason we purposefully overstate the likely effect of real wage growth on retirement is to try to be conservative about the likely effects of Social Security given various un-modeled factors that might have increased the demand for retirement, such as private pensions and changes in the prices of leisure goods.

A.3.3 Decomposition of the effects of OAA on retirement into income and substitution effects

We decompose the effects of OAA into income and substitution effects using the following method. We solve for the optimal retirement age under three budget constraints: OAA, No OAA, and "No OAA with Compensation."[61] We consider two different "No OAA with Compensation" budget constraints. Each is identical to the No OAA budget constraint except for one change. In one case, initial assets are increased exactly enough that the individual is able to achieve exactly the same utility that he would achieve under OAA. In the other case, non-labor income after the OAA eligibility age is increased exactly enough that the individual is able to achieve exactly the same utility that he would achieve under OAA. If capital markets were perfect, the individual would be indifferent between receiving an immediate transfer of assets and receiving a present value-equivalent increase in his future non-labor income. But with borrowing constraints, individuals weakly prefer an increase in initial assets to a present value-equivalent increase in late-life income. The estimated equivalent variation of OAA is therefore weakly greater under the late-life income compensation than it is under the initial assets compensation. In the text, we discuss the equivalent variation of OAA based on both measures, but for measuring income effects we use the late-life income compensation.

The income effect of OAA is the number of years earlier that people retire under the "No OAA with Compensation" budget constraint relative to the No OAA budget constraint due to being richer with OAA.[62] The substitution effect of OAA is the number of years earlier that people retire under the OAA budget constraint relative to the "No OAA with Compensation" budget constraint due to the taxation of late-life labor supply implicit in OAA's means tests.

To develop intuition about the model, observe that lifetime utility is quasilinear in the length of retirement; individual i's marginal utility of leisure is constant and equal to δ_i regardless of how much leisure he consumes.[63] This means that for people who are not at a corner in their time allocation (i.e., for people who wish to spend a strictly positive amount of time working and a strictly positive amount of time in retirement), the entirety of wealth windfalls is spent extending their retirements and none is spent increasing consumption. Thus, within the broad set of preferences in which consumption is non-inferior, these (standard) preferences maximize the income effect's share of the total effect on retirement of any change in budget constraints. This maximum income effect equals the amount of extra time an individual can spend retired while still maintaining the same consumption level. So, ignoring for now discounting ($r = 0$) and the possibility of binding borrowing constraints in order to make this calculation simple, the income effect on retirement of an OAA program that pays $\bar{y}$ per year for someone whose constant potential earnings level is w per year and who claims OAA

[61]We hold utility fixed at the level of utility the individual achieves with OAA in order to ensure invertibility in the presence of borrowing constraints.

[62]Recipients of OAA likely had their opportunity sets expanded by OAA since it was means-tested.

[63]This feature of the model comes from the standard assumption that preferences are additively separable over time.

for t_O years is $\frac{\bar{y}t_O}{w}$. For example, the income effect of OAA on retirement for someone whose potential earnings equals the OAA benefit is the number of years he claims OAA.

A.4 Robustness

Table A5 reports results based on the baseline specification and several other alternative specifications of the model. The parameter estimates and, especially, the key conclusions are quite stable across specifications. The main exception is the simulated effect of Social Security in the case in which the coefficient of relative risk aversion is calibrated to be 2 ($\eta = -2$). In this case, income effects of changes in wages far exceed substitution effects, and optimal labor force participation rates at ages 65 and older are predicted to be zero even in the absence of Social Security. As a result, the predicted effect of Social Security on labor force participation from age 65–74 is also zero. If instead we ignore any effects of real wage growth on retirement, the predicted effect of Social Security on labor force participation from age 65–74 in this case is 7.8 percentage points, similar to the results of the other specifications.

A.5 Estimation of the OAA Recipiency Rate among Men aged 65–74

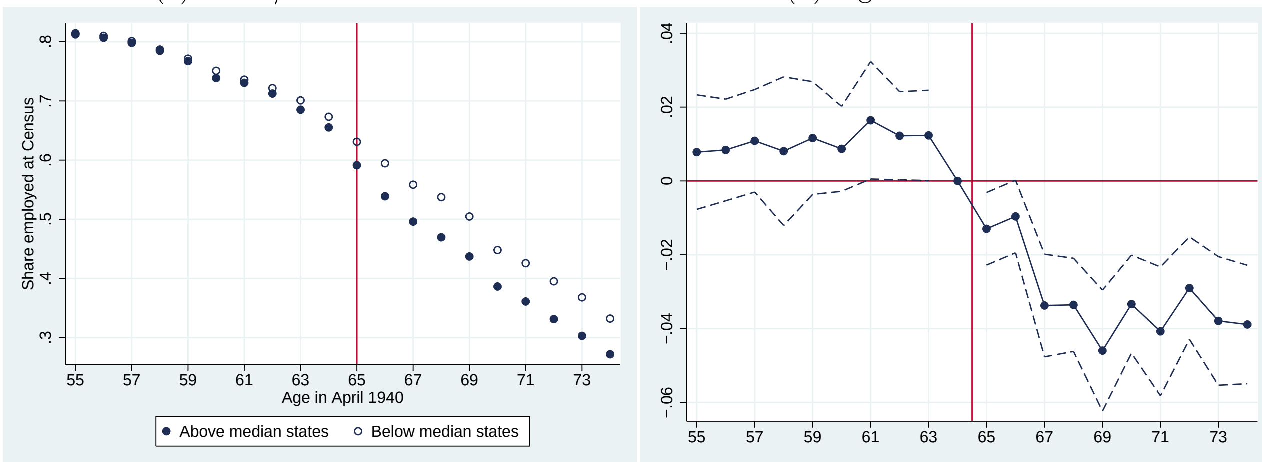
As noted in the main text, we know the total number of OAA recipients, but we do not have data that would allow us to directly calculate the total number of male recipients aged 65–74 in 1940, which is the relevant recipiency rate for the bounding exercise we report. *A priori* it is likely that the recipiency rate for this group would be below the overall 22 percent recipiency rate that includes both men and women as well as older individuals. We do have information on the age and sex of *new* recipients at various points in time, however, so to provide a rough measure of the relevant recipiency rate in 1940 (a stock), we add up flows into the program and adjust for mortality and for aging out of the 65–74 age group. From U.S. Social Security Board (1939*a*), U.S. Social Security Board (1939*b*), and U.S. Social Security Board (1941) we have the number of new male recipients in fiscal years 1937/38 through 1939/40 by age at the end of the fiscal year, where age is reported in two groups: 65–69 and 70–74. The annual reports of the Social Security Board for 1935/36 and 1936/37 (U.S. Social Security Board, 1937*a*,*b*) provide the same information for fiscal year 1936-37, although not all states collected data for the entire fiscal year, meaning that we understate inflows in that year (for the period from July 1, 1936 through September 30, 1936 we observe the age distribution but not separately by sex; we assume half of new recipients aged 65–69 and 70–74 were men, which is approximately true in subsequent years). We do not have data on (and hence exclude from our calculation) any individuals who started receiving OAA prior to July 1, 1936.

In adding up flows, we adjust for aging out of the 65–74 range and for mortality. All men who started receiving OAA between the ages of 65 and 69 from July 1, 1936 onwards would still have been aged 65–74 as of mid-1940. We make a conservative assumption about the ages of 70–74 year olds, which is that no new recipients aged 70–74 by mid-1937 would still be 74 or younger by mid-1940, one-third of those 70–74 in mid-1938 would be 74 or younger in mid-1940, and two-thirds of those 70–74 in mid-1939. We then assume that recipients' mortality rate was 5.5% per year, just above the mortality rate of 65–74 year old men in the second half of the 1930s (Grove and Hetzel, 1968). This calculation yields 523,987 male recipients aged 65–74 in mid-1940, compared to a male 65–74 population in the 1940 Census of 3,167,055, for a recipiency rate of about 16.5 percent.

Results Appendix

Figure A1: Employment in 1940, by age and state payments per person 65+

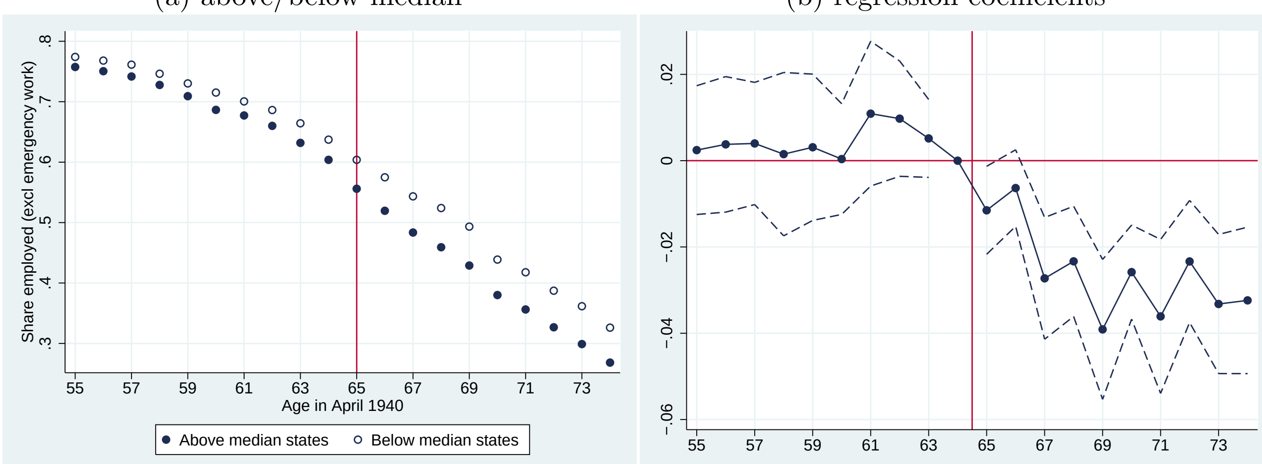
(a) above/below median (b) regression coefficients



Notes: Panel (a) shows share of men employed at the time of the 1940 Census, in states with above and below-median payments per person 65+ in 1939. Panel (b) shows point estimates and 95% confidence intervals on age-payment interactions from estimation of equation (2). Standard errors clustered at the state level. $N = 2334689$.

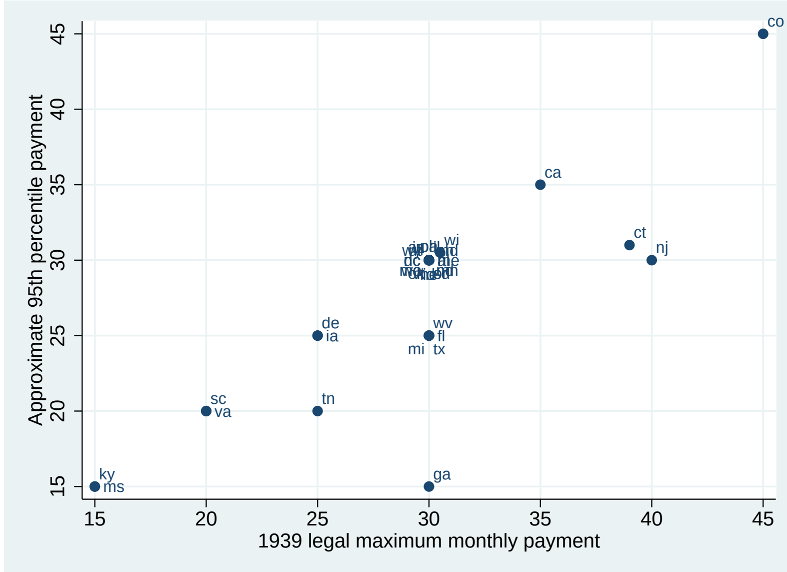
Figure A2: Private or non-emergency employment in 1940, by age and state payments per person 65+

(a) above/below median (b) regression coefficients



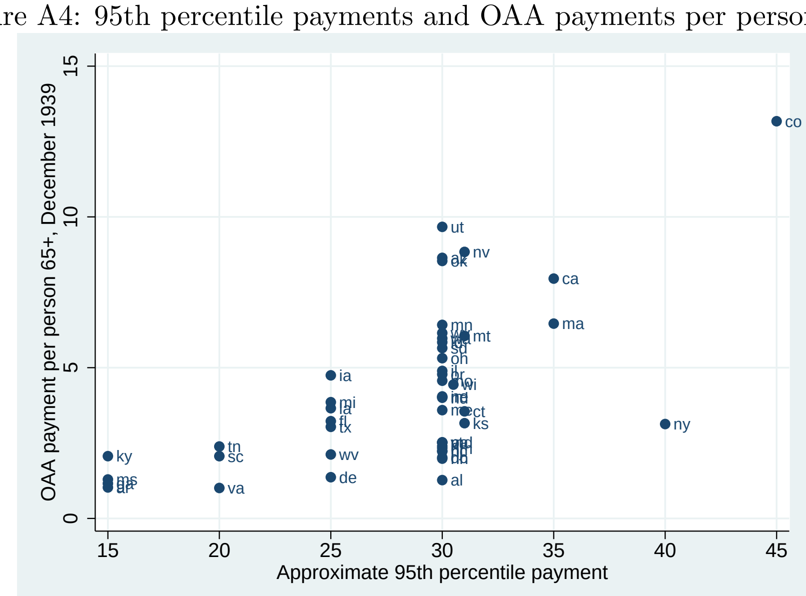
Notes: Panel (a) shows share of men employed in private or non-emergency government work at the time of the 1940 Census, in states with above and below-median payments per person 65+ in 1939. Panel (b) shows point estimates and 95% confidence intervals on age-payments interactions from estimation of equation (2). Standard errors clustered at the state level. $N = 2334689$.

Figure A3: 95th percentile payments and legal maxima, for states with legal maximum



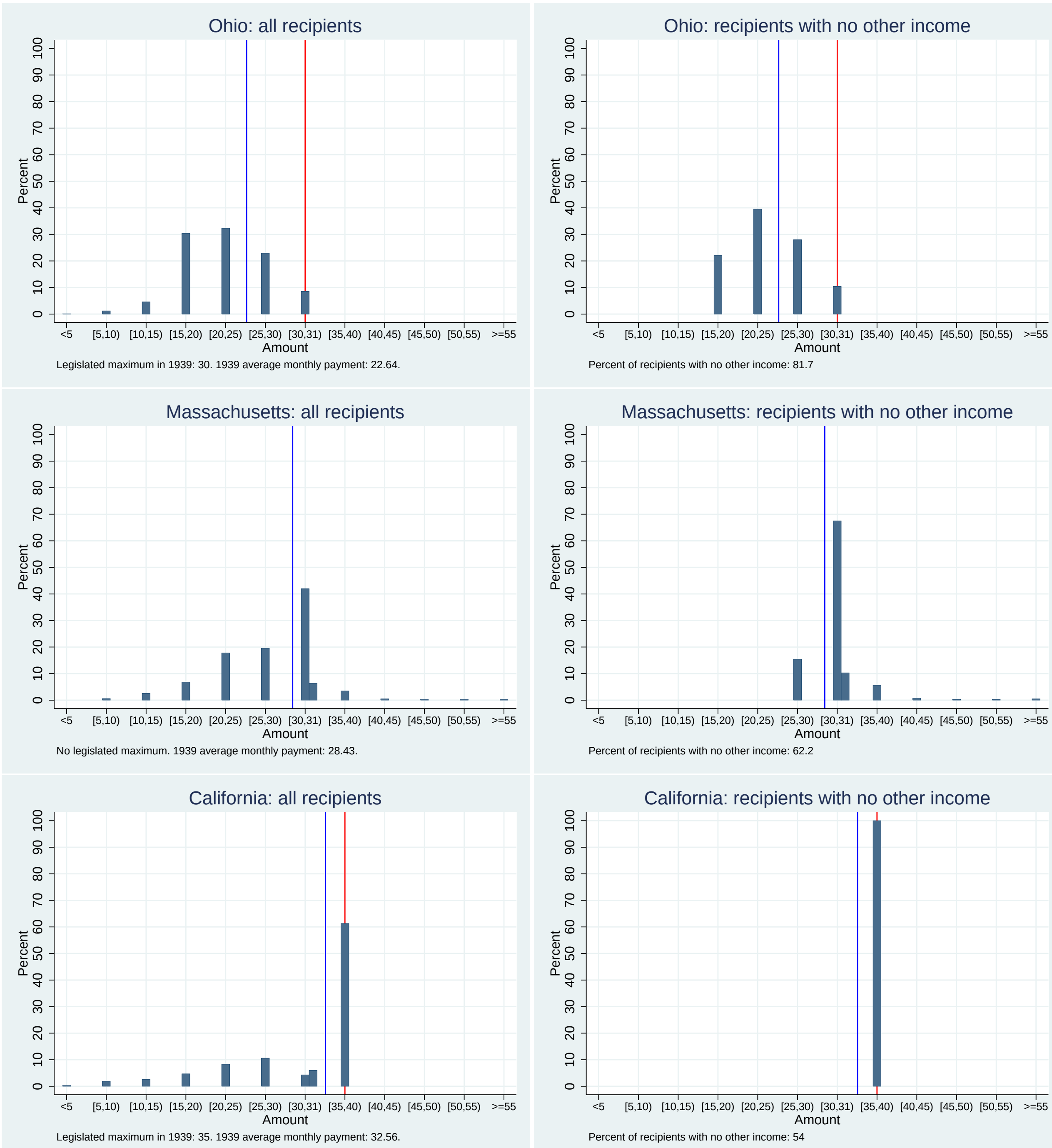
Notes: 'Approximate 95th percentile payment' is for new recipients in fiscal year 1938-39 (see Section 2 for definition). Eight states had no legal maximum payment. Sources: legal maximum payments from U.S. Social Security Board (1940*a*), 95th percentile payment from U.S. Social Security Board (1939*b*).

Figure A4: 95th percentile payments and OAA payments per person 65+



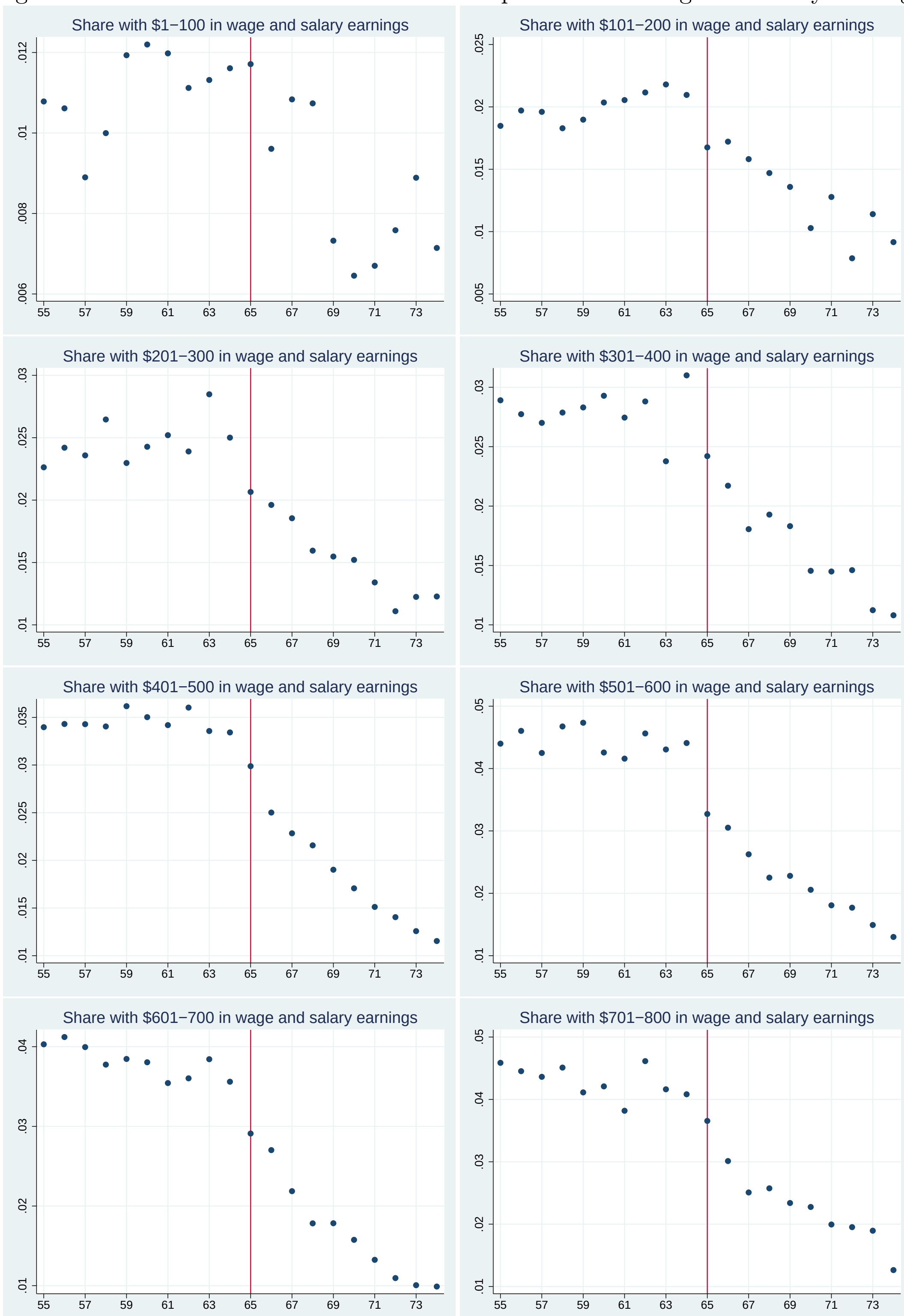
Notes: 'Approximate 95th percentile payment' is for new recipients in fiscal year 1938-39 (see Section 2 for definition). Payments per person 65+ are normalized by state population at 1940 Census. Sources: data on OAA dollar payments from U.S. Social Security Board (1940*b*), data on 95th percentile payment from U.S. Social Security Board (1939*b*).

Figure A5: Distributions of payments to new recipients in 1938-39, by state



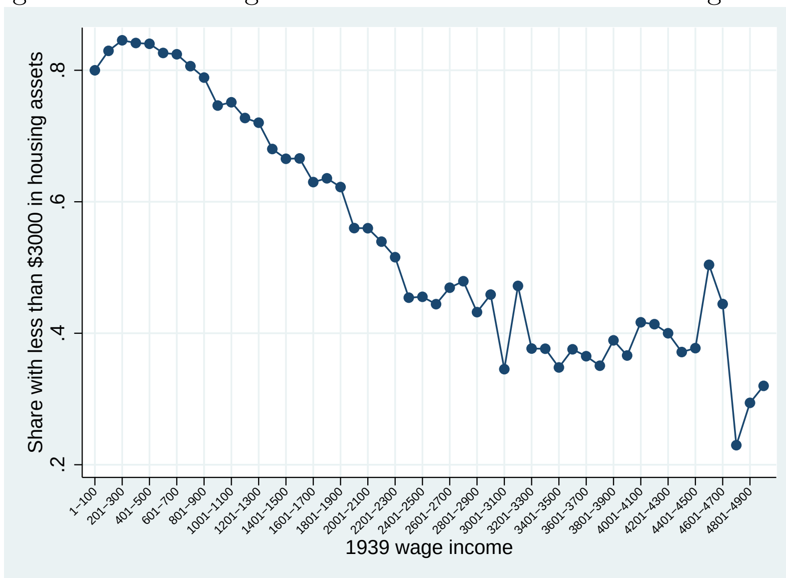
Notes: Left figures show distributions of payment amounts to new recipients in 1938-39 by state, based on data from U.S. Social Security Board (1939*b*). Vertical lines correspond to average monthly payment and legislated maximum payment (if one existed) in 1939. Right figures show estimated distribution for recipients with no other source of income, under the assumption that those with other sources of income received the lowest payments.

Figure A6: Share of Massachusetts men with specified 1939 wage and salary earnings



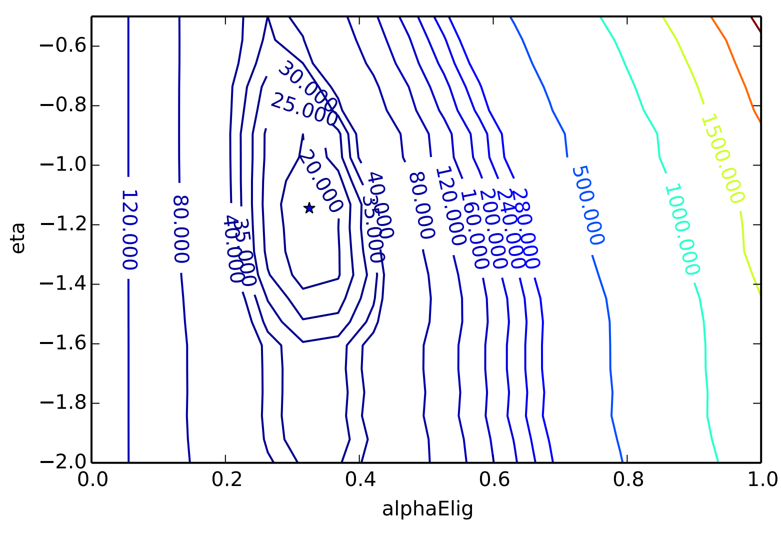
Notes: Figures show share of men reporting 1939 wage and salary earnings in specified range, by age at 1940 Census.

Figure A7: Share eligible for OAA based on their housing wealth



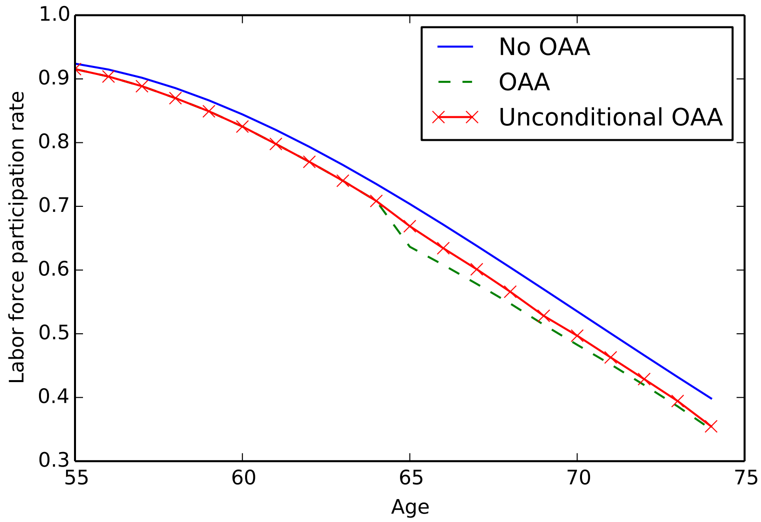
Notes: Share of Massachusetts men aged 60–64 who had less than $3,000 of house value, as a function of wage and salary income.

Figure A8: Method of simulated moments objective function



Notes: Method of simulated moments objective as a function of η (the negative of the coefficient of relative risk aversion) and α_e (the constant in the eligibility-potential earnings relationship). Higher contours indicate a worse fit of the model. The asterisk marks the estimates.

Figure A9: Simulated cross-sectional age-labor force participation profile in 1940



Notes: Simulated cross-sectional relationship between labor force participation and age in 1940 in the US. The "No OAA" profile is the counterfactual no-OAA profile predicted based on our regression results and presented in Figure 13. The "OAA" profile is simulated based on the estimated model. It can be compared to its empirical counterpart, which is depicted in Figure 13. The key difference between this figure and Figure 15 is that this figure focuses on the 1940 cross section, whereas Figure 15 focuses on the life cycle profiles of the cohort of men aged 55 in 1940. The main reason that the predicted effects of OAA in the 1940 cross section are smaller than those over the life cycle of the cohort of men aged 55 in 1940 is that individuals in the latter group have had much more time to build OAA into their plans.

Table A1: Main results using payments per person 65+ in levels

	(1) Non-wage income	(2) In labor force	(3) Employed	(4) Non-emergency
Per-65+ monthly payment × age 55-59	0.002	0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Per-65+ monthly payment × age 65-69	0.012***	-0.013***	-0.010***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Per-65+ monthly payment × age 70-74	0.017***	-0.016***	-0.012***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.001)
Observations	2178112	2334689	2334689	2334689
Sample	border	border	border	border
Border segment × age fixed effects	yes	yes	yes	yes
Education × age fixed effects	yes	yes	yes	yes
Race × age fixed effects	yes	yes	yes	yes

Dependent variables: receipt of non-wage income in 1939, in labor force at 1940 Census, employed at 1940 Census, employed in private or non-emergency government work. Payments in 1940 dollars. Sample restricted to counties on state boundaries, excluding counties on borders of states with age eligibility requirement other than 65 in 1939. Unit of observation is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table A2: Main results using legal maximum payments

	(1) Non-wage income	(2) In labor force	(3) Employed	(4) Non-emergency
Maximum annual payment	0.003	0.004	0.005**	0.003
× age 55-59	(0.003)	(0.002)	(0.002)	(0.002)
Maximum annual payment	0.019***	-0.021***	-0.019***	-0.008**
× age 65-69	(0.004)	(0.003)	(0.002)	(0.002)
Maximum annual payment	0.022***	-0.029***	-0.025***	-0.011**
× age 70-74	(0.004)	(0.004)	(0.003)	(0.003)
Observations	1900760	2034007	2034007	2034007
Sample	border	border	border	border
Border segment × age fixed effects	yes	yes	yes	yes
Education × age fixed effects	yes	yes	yes	yes
Race × age fixed effects	yes	yes	yes	yes

Dependent variables: receipt of non-wage income in 1939, in labor force at 1940 Census, employed at 1940 Census, employed in private or non-emergency government work. Annual payments in hundreds of 1940 dollars. Sample restricted to counties on state boundaries, excluding counties on borders of states with age eligibility requirement other than 65 in 1939. Unit of observation is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table A3: Main results using state 95th percentile payments

	(1) Non-wage income	(2) In labor force	(3) Employed	(4) Non-emergency
95th percentile annual	0.006*	0.002	0.003	0.001
payment × age 55-59	(0.002)	(0.002)	(0.002)	(0.002)
95th percentile annual	0.026***	-0.024***	-0.018***	-0.011**
payment × age 65-69	(0.005)	(0.004)	(0.004)	(0.003)
95th percentile annual	0.031***	-0.027***	-0.022***	-0.013**
payment × age 70-74	(0.006)	(0.006)	(0.005)	(0.004)
Observations	2238476	2403915	2403915	2403915
Sample	border	border	border	border
Border segment × age fixed effects	yes	yes	yes	yes
Education × age fixed effects	yes	yes	yes	yes
Race × age fixed effects	yes	yes	yes	yes

Dependent variables: receipt of non-wage income in 1939, in labor force at 1940 Census, employed at 1940 Census, employed in private or non-emergency government work. Annual payments in hundreds of 1940 dollars. Sample restricted to counties on state boundaries, excluding counties on borders of states with age eligibility requirement other than 65 in 1939. Unit of observation is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table A4: Cross-state migration 1935-40 by state payments per person 65+ and age

	(1)	(2)	(3)	(4)	(5)	(6)
Log per-65+ payment	0.0010	-0.0023	-0.0024	0.0008	0.0009	0.0010
× age 55-59	(0.0009)	(0.0017)	(0.0016)	(0.0017)	(0.0010)	(0.0011)
Log per-65+ payment	-0.0008	0.0013	0.0014	-0.0019	0.0016	0.0017*
× age 65-69	(0.0013)	(0.0022)	(0.0022)	(0.0019)	(0.0008)	(0.0008)
Log per-65+ payment	-0.0021	0.0027	0.0028	-0.0026	0.0016	0.0016
× age 70-74	(0.0020)	(0.0034)	(0.0033)	(0.0034)	(0.0015)	(0.0015)
Observations	6585063	6585063	6585063	2297575	2297575	2297575
Sample	full	full	full	border	border	border
Census region × age fixed effects	no	yes	yes	no	no	no
Border segment × age fixed effects	no	no	no	no	yes	yes
Education × age fixed effects	no	no	yes	no	no	yes
Race × age fixed effects	no	no	yes	no	no	yes

Dependent variable: moved states between 1935 and 1940. Sample for columns (1)-(3): men aged 55-74 in states with 1939 eligibility age of 65 and non-missing rest-of-state payments per person 65+, 1935 state of residence, and 1940 employment information. Columns (4)-(6) include only counties on state boundaries and exclude counties on borders of excluded states. Unit of observation in columns (4)-(6) is an individual-state border pair. All specifications include county fixed effects and 5-year age group fixed effects. All age-interactions are with 5-year age groups. Standard errors (in parentheses) are clustered at the state level. *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Table A5: Estimation results and robustness

	Baseline	β_e in 2nd stage	Alt. $F(T_r(N))$	$\eta = -1/2$	$\eta = -2$	PVBC	$r = 0$, $\beta = 1$	Outside estimates
Parameter estimates								
$\hat{\alpha}_e$	0.33	0.26	0.33	0.26	0.34	0.35	0.32	
$1000 \times \hat{\beta}_e$	-0.12	0.00	-0.12	-0.12	-0.12	-0.12	-0.12	
$\hat{\eta}$	-1.14	-1.17	-1.15	-0.5	-2	-1.16	-0.90	
Key implications								
Percentage of men eligible for OAA[a]	18.3	26.3	18.3	12.7	19.8	20.3	18.1	
Equivalent variation of OAA, %[b]	96.1	94.8	95.7	97.7	95.9	96.0	95.8	
Effect of OAA earnings test, % of total	44.7	51.9	45.3	49.2	34.3	50.0	47.0	
Reduction in LFP(65–74) from Social Security (p.p.)	8.0	6.5	8.5	7.6	0.0[c]	7.1	10.4	
Validation tests								
Reduction in LFP(65–74) from OAA in 1940	5.5	5.7	5.8	5.5	4.7	5.9	5.6	5.7[d]
OAA recipiency rate among men 65–74, %	16.4	21.5	16.2	12.5	16.1	18.1	15.6	16.5[e]
Objective function value	15.5	13.6	15.4	29.4	28.5	18.2	15.9	

Notes:
Estimates and implications of the estimated model under various assumptions. "β_e in 2nd stage" is an estimation in which the slope of the eligibility-potential earnings relationship is estimated jointly with the other parameters in the second stage. In all of the other estimations, this parameter is estimated separately in a first stage based on the slope of the relationship between earnings and house value in Massachusetts. "Alternative $F(T_r(N))$" uses a counterfactual no-OAA retirement distribution based on regressions in which the measure of OAA policy is maximum payments rather than payments per person 65 and older. "PVBC" is based on a model with perfect capital markets, in which individuals can borrow as much as they wish as long as they satisfy a present value budget constraint. In all cases, the objective function is a standard classical minimum distance objective function, so lower values indicate a better fit of the model. The observed reduction in LFP(65–74) from 1940 to 1960 (against which the reductions from Social Security can be compared) was 13.5 percentage points.
(a) Percentage of men "eligible" for OAA is the percentage of men who would receive OAA benefits if they had no earnings and were 65 and older.
(b) Present value of the welfare-equivalent unconditional late-life income stream (received each year from age 65 on regardless of earnings) as a ratio of the present value of actual OAA benefits received.
(c) The zero estimated effect of Social Security in the $\eta = -2$ specification is due to the (implausibly) large income effects of real wage growth from 1940–60 on retirement; even without Social Security, nobody works past age 65. The simulated reduction in LFP(65–74) if we assume that real wage growth has no effect on retirement (as would be the case if income and substitution effects of changes in real wages exactly offset) is 7.8 percentage points.
(d) Extrapolation based on authors' reduced-form regression results.
(e) Authors' calculations based on data on the characteristics of new recipients of OAA, 1936–1940.