FEDERALIZING BENEFITS: THE INTRODUCTION OF SUPPLEMENTAL SECURITY INCOME AND THE SIZE OF THE SAFETY NET*

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Abstract:

In 1974, Supplemental Security Income federalized the previously state-run cash welfare programs for the aged, blind, and disabled, imposing a national minimum benefit and standardized eligibility criteria. Because of pre-existing variation in generosity, SSI raised benefits more in some states than others, but had no effect on benefits in states that were above its benefit floor. This paper shows that SSI increased the size of disability transfer programs in states with the lowest pre-SSI benefit levels, but shrank non-disability transfer programs such as Aid to Families with Dependent Children and General Assistance. For every four new SSI recipients brought onto the program by benefit increases, three came from other welfare programs. Each dollar of per-capita income transferred through SSI increased total per-capita transfer income by just over 50 cents.

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In 1974, Supplemental Security Income (SSI) replaced a set of highly variable state welfare programs for the elderly, blind, and disabled, with a "more uniform and equitable" (Nixon 1974) federal system. SSI raised benefits up to a federal income floor, sought to remove "any stigma of being dependent on welfare" (Senator Wallace Bennet [R-UT] quoted in Berkowitz and DeWitt 2013, p. 40), and delivered "fiscal relief to State and local governments" (Nixon 1974). Observers at the time described SSI as "the most fundamental new departure in U.S. public welfare policy since the 1930s" (Bickel and Wilcox 1974, p. vii).

Within SSI's first year, monthly transfer spending for the aged, blind, and disabled grew by a third, from $1.3 billion to $1.8 billion, and monthly participation grew by a quarter, from 3.2 to 4 million recipients. Yet by federalizing just part of the welfare system, SSI created incentives for recipients and policymakers to "shift" cases away from programs partly or wholly financed by the states (Schmidt and Sevak 2004). Moving an adult from Aid to Families with Dependent Children to SSI, for example, would raise their family income by $200 on average and save their state $150. Therefore, by inheriting an existing pool of recipients and potentially drawing heavily from another, SSI may not have increased the size of the overall safety net, despite its sharp growth.

This paper evaluates how SSI's introduction affected participation and spending in the adult programs that it replaced (for the blind and disabled) and in the programs that remained under state control (for single parent families and, in some states, other poor adults). We exploit the fact that pre-existing variation in benefit generosity combined with the federal nature of SSI led to wide differences in benefit changes across states. As SSA historian Larry DeWitt put it: "SSI was a radical welfare reform in Mississippi and only an incremental reform in New York City" (Berkowitz and DeWitt 2013). Our empirical strategy exploits the ways that SSI differentially

affected states based on their pre-existing benefit levels. We use a difference-in-differences design that compares changes in program participation, payments per recipient, and per-capita spending before and after SSI's introduction in states with lower versus higher pre-SSI benefit levels. Crucially, though, we make these comparisons separately for states where SSI's minimum benefit was binding and actually raised benefits, and for states where the benefit floor was not binding and had no differential effect on benefit levels. This distinct feature of SSI strengthens our design by embedding a falsification test (null effects in the highest benefit states), that can help rule out concerns about confounding factors correlated with state generosity.

To measure participation and spending, we digitized data by state and month on each categorical welfare program from 1936-1988. This provides accurate and detailed new information on the entire history of modern cash welfare programs from their inception in the 1935 Social Security Act. It allows us to examine the evolution of state welfare programs decades before SSI, and to observe the precise timing of changes after its enactment.

We find strong evidence of caseload shifting: SSI increased the size of disability transfer programs but shrank other adult programs, dampening its effect on the overall safety net. Trends in participation rates, for example, were nearly identical for states with different pre-SSI benefit levels from 1950 to 1973. However, immediately after SSI took effect the lowest-benefit states saw the largest jumps in disability participation as well as relative reductions in participation in other programs. We estimate that for every four new SSI recipients induced to participate because of benefit increases, three came from other welfare programs, and that each dollar of per-capita income transferred through SSI increased total per-capita transfer income by just over 50 cents. We cannot explicitly test whether caseload shifting came from individual or state actions, but the cross-state patterns track household-level incentives more closely than those of

states. Finally, because our design necessarily compares states with different pre-SSI benefits, we difference out any effects of SSI that did not vary by state, such as changes in awareness of the program or in stigma. Our estimates therefore understate SSI's total effect on the safety net, but do suggest that it was limited by caseload shifting.

Understanding SSI-induced program substitution matters for several reasons. First, we have little evidence on whether and how the birth of SSI, one of the biggest welfare reforms of the 20th century, reshaped the American safety net. Our results suggest that SSI changed not only the size, but also the composition of safety net assistance. Second, program substitution is central to any cost-benefit evaluation of SSI. Our findings suggest that because SSI spending came at the expense of other transfer programs, its direct costs are larger than its costs net of fiscal externalities (Mayshar 1990). SSI benefit statistics also overstate the extent to which SSI actually redistributed income. Failing to account for program interactions thus inflates costs and may understate benefits. SSI's introduction also provides unique insights into economic models of fiscal federalism (Brown and Oates 1987, Brueckner 2000, Oates 1999) that predict that locally administered welfare programs will be smaller than national ones.[1] Our results suggest that recipient mobility between programs operated and funded by different levels of government are important to consider in these models.

I. DISABILITY TRANSFER PROGRAMS BEFORE AND AFTER SSI

Prior to the 1930s, support for the poor was largely left to families, private charities, and a patchwork of state and local programs (Fishback et al. 2010).[2] The severity of the Great

[1] Localities choose lower benefits levels in equilibrium either when recipients migrate, which increases the cost of redistribution, or when people are altruistic toward non-local poor, which creates a positive externality.

[2] This paragraph draws heavily from the Social Security Administration, Historical Background and Development of Social Security.

Depression, however, substantially weakened these traditional forms of the safety net.[3] In response, Franklin Roosevelt created the Committee on Economic Security (CES) in 1934 which quickly provided recommendations to Congress that "sketch[ed] the need for additional safeguards against *the major hazards and vicissitudes of life*." The Committee's report led to the enactment of the Social Security Act (SSA) of 1935, which for the first time committed the federal government of the United States to the economic security of many of its most vulnerable residents. The Social Security Act provided means-tested income support to the elderly in the form of the Old Age Assistance program (OAA), to the blind through the Aid to the Blind program (AB), and to dependent children through the Aid to Dependent Children program (ADC, whose name changed to Aid to Families with Dependent Children or AFDC in 1962) (Grundman 1985). While CES considered the plight of those with disabilities, Aid to the Permanently and Totally Disabled (APTD) was not added until 1950.[4]

Disability policy in the United States remained largely unchanged until the early 1970s, when SSI was introduced largely in reaction to more comprehensive proposals for universal basic income programs.[5] In 1969, President Richard Nixon proposed the Family Assistance Plan (FAP), a negative income tax for families with children that would have replaced AFDC (CQ Almanac 1971). For those in the other "adult" welfare categories (the aged, the blind, and the

[3] Other phenomena contributed to support for the Social Security Act as well including industrialization, urbanization, and the decline in extended families (Social Security Administration, Historical Background and Development of Social Security).

[4] The unpublished studies produced by the CES include studies on both Invalidity Insurance and on Provisions for the Physically Handicapped (https://www.ssa.gov/history/reports/ces/cesvolsix.html). However, in his suggestions to the Advisory Council, Edwin Witte (executive director of the CES and later known as the "Father of Social Security") wrote, "Invalidity is the most serious of all economic hazards that can strike any individual, but fortunately affects only a relatively small part of the population. Experience with invalidity insurance in this country has been very unsatisfactory and there is no basis now for a possible compilation of the costs. Consequently, it is suggested that there be no recommendation on invalidity insurance except that the National Welfare Administration shall collect statistics for the computation of costs and further study the possibilities of invalidity insurance" (Witte 1934). Disability insurance was not created until the 1956 Amendments to the Social Security Act (Grundman 1985).

[5] This paragraph draws heavily from Berkowitz and DeWitt (2013).

permanently and totally disabled), Nixon proposed to add a national minimum benefit level and eligibility criteria but otherwise leave states in control. Senator Russell Long (D-LA) strongly opposed the FAP, and introduced Supplemental Security Income in 1972, a fully federal version of Nixon's plan for the non-AFDC categories, as a way to ensure FAP's failure.[6,7]

SSI initially provided a nominal minimum benefit for a single adult beneficiary with no other income of $140 per month ($756 in 2017 dollars).[8] The program was intended to replace wide variation in APTD benefit levels across states with a uniform system that would be easier to administer across states. Figure 1 maps the difference between SSI's benefit floor and each state's 1971 APTD maximum benefit. Alabama, Mississippi, Louisiana, and West Virginia had benefits that were more than $300 below SSI's level, while Michigan's benefit was $414 above it. Because SSA did not want any individual recipients to be worse off as a result of the implementation of SSI, it imposed a system of benefit supplementation on states that already paid above the SSI level (shown in white in figure 1).

SSI also adopted the existing definition of disability status from the Social Security Disability Insurance (SSDI) program, standardizing eligibility criteria across states, and sought to reduce stigma. Referring to SSI's target populations, Republican Senator Wallace Bennett (R-UT) noted that the congress had "tried to raise their income in such a way that they would be free as far as possible from any stigma of being dependent on welfare (Berkowitz and DeWitt 2013, pg. 40)." SSI checks in all states were designed to look like those paid to Social Security

[6] Long and other conservatives were joined in their opposition to FAP by welfare rights activists in northern states, who observed that benefit levels would fall under the FAP in many states and that the FAP would impose other restrictions on welfare recipients (Burke and Burke 1974). Long later said, "To keep them from coming back with something that was going to make the whole nation into a welfare state, I felt the way to spike their guns on that would be to take all the money they estimated on this family program and apply that to the aged (quoted in Berkowitz and DeWitt 2013).

[7] A number of problems plagued the implementation process for SSI, so the first benefits were not paid until January 1974.

[8] The actual 2018 individual benefit is $750 (see https://www.ssa.gov/OACT/cola/SSI.html).

recipients and to be visually distinct from "welfare" checks (Berkowitz and DeWitt 2013, pgs. 51-52). Policy makers also took the opportunity to entrench notions of deservingness between traditional welfare programs such as AFDC and the new SSI program. In his statement about the signing of the legislation, President Nixon called SSI recipients "especially deserving people" and wrote that his administration "worked hard to see that services are concentrated on those who are truly needy, rather than permitting funds to be spent with little regard for genuine need" (Nixon 1974). Senator Abe Ribicoff (D-CT) praised SSI because it took people "off welfare" (Burke and Burke 1974, pg. 196).

The significance of the creation of SSI was largely missed at the time by both politicians and journalists. A great deal of attention had been paid to the debate over FAP, a radical reform with supporters and detractors across the political spectrum. As a result, the creation of a wholly federal program for the aged, blind, and those with disabilities "escaped detection because few read the plan, because few understood the welfare status quo well enough to appreciate the plan, and because [people] interpreted the triple endorsement of Richard Nixon, Wilbur Mills, and Russell Long as a guarantee that the plan was modest" (Burke and Burke 1974, pg. 197). But for the first time, the federal government of the United States committed to providing a guaranteed level of cash income to certain categories of adults who were considered unable to work.

II. HISTORICAL DATA ON CASH TRANSFER PROGRAMS

One impediment to research on the development of the American safety net is the lack of reliable, high-frequency, local data on the size of cash transfer programs throughout the 20th century.[9] To address this limitation we created a new state-month panel of the number of

[9] Most survey datasets and one administrative dataset of AFDC recipients (see Moffitt 1987) only become available in the 1960s, and the Census does not include welfare income until 1970. State-by-month data only exists for AFDC (Blank 2001), county-by-year data exist only as aggregates across several programs (Almond, Hoynes, and Schanzenbach 2011). Research on the long-run development of the welfare system relies on periodic snapshots of policy variables (Fishback et al. 2010, Moehling 2007), narrative evidence (Alston and Ferrie 1985). Other work

recipients and amount of benefit spending for the entire history of the modern cash safety net from 1936 through 1988. Primary source information comes from either the Department of Health, Education, and Welfare or the Social Security Administration (see data appendix for sources and details on data cleaning) and covers OAA, AB, AFDC, General Assistance (GA), and (beginning in October, 1950) APTD, and Medical Vendor Payments (MVP).[10] In 1974, OAA, AB, and APTD data stop and we observe the same information for the corresponding eligibility categories in SSI. Except for information on AFDC participation after 1960, these data have not previously been available.

We focus on disability transfers, which motivates several sample restrictions. First, we include the years 1950 to 1980 because APTD began in 1950 and 1981 marks a major AFDC reform (OBRA 1981). Second, we exclude Nevada, which never enacted APTD, as well as Alaska, Hawaii and the territories, which are inconsistently measured. We do not examine OAA since the scope for shifting from other programs was limited, and we do not examine AB since it was a very small and shrinking program (with about 3 percent of the spending and recipients as APTD).

We create measures of participation and spending in cash programs for adults only.[11] APTD did not typically cover children, so we use the reported number of recipients. GA did sometimes cover children, so we use the reported number of cases. The number of AFDC adults equals the total number of recipients minus children. We also adjust recipient counts in some cases to exclude those who received medical care only (see data appendix). SSI spending includes federal

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focuses on watershed periods like the New Deal (Fishback, Haines, and Kantor 2007) or the War on Poverty (Bailey and Duquette 2014). Recent research exploits unique datasets from a point in time such as Mother's Pension case records (Aizer et al. 2014) or the full-count 1940 Census' (Fetter 2017, Fetter and Lockwood 2016).

[10] Starting in October, 1950 states could claim federal reimbursement for medical payments made directly to providers (known as "vendor payments") on behalf of welfare recipients.

[11] SSI covered relatively few children immediately after the creation of the program, but the child SSI caseload grew quickly after Sullivan v. Zebley (1990) liberalized child eligibility rules (Garrett and Glied 2000).

spending and state supplementation. SSI participation includes recipients who received federal payments, federal and state payments, or state supplementation only. We study three outcomes: the average monthly adult participation rate (recipients per adult aged 25-64), the average benefit (cash payments per recipient), and per-capita transfer income (cash benefit expenditures per adult aged 25-64).[12] We average our data by year to avoid differences in seasonality across states.

Table 1 presents summary statistics for the participation counts and spending totals (panel A) and the outcome measures (panel B). More than 6.3 million adults received about $4.5 billion in cash benefits in 1980, with half of them on AFDC, a third on SSI because of a disability, and most of the remainder on GA. Proportionally, disability programs grew fastest: the number of recipients increased by almost seven times between 1960 and 1980, while the participation rate grew by a factor of five, from 0.35 percent to 1.74 percent. AFDC participation grew strongly as well, but most of this growth had already taken place by 1970. Notably real benefits rose in the 1950s and 1960s, but fell in the 1970s as policy failed to keep up with inflation.

We also use two microdatasets that measure multiple program participation or switching over time. The first is a set of administrative data on AFDC recipients that report the benefit status of everyone in sample of 155,528 households in 1967, 1973, 1975, and 1977 (DHEW 2011). These data allow us to measure the probability that an adult in an AFDC household received disability benefits before and after SSI. This directly measures the pattern that we would expect for caseload shifting between AFDC and SSI. The second dataset is the 1974 wave of the Survey of Low Income Aged and Disabled (SLIAD; Social Security Administration 1992), which was fielded in two waves in 1973 and 1974 in order to study SSI specifically. We use two samples:

[12] Data on state populations come from Census counts (Haines and ICPSR 2010) and the Surveillance, Epidemiology, and End Results (SEER 2013), and we convert all benefit values to 2017 dollars.

6,140 adults who received APTD in 1973 and 2,740 adults who reported a disability (but did not necessarily receive benefits).

III. What was the SSI treatment? Different Incentives Across States

Our empirical approach exploits the "highly uneven effects" of "shifting from the divergent array" of APTD programs to a "nationally standardized system of subsistence-income grants" under SSI (Bickel and Wilcox 1974, pg. 15). A key advantage is that SSI's structure created a distinct pattern of changes in statutory benefit levels across states and over time. By bringing states up to a national floor, denoted G^{SSI}, SSI mechanically raised payment levels in areas that had the lowest APTD benefits, denoted G_s^{APTD}. Congress, however, set G^{SSI} close to "the median level of payment standards established in state assistance programs (Bickel and Wilcox 1974, pg. 16)" so SSI was only binding for about half of states. Where it did not bind, states had to supplement benefits up to their APTD levels (at least for recipients transferred from APTD). Therefore, distance *above* SSI's floor bore no mechanical relationship to post-SSI benefit increases.

SSI thus led to the following approximate change in maximum benefits for state s:

$$\Delta G_s \approx |G^{SSI} - G_s^{APTD}| \times 1\{G_s^{APTD} < G^{SSI}\} = d_s \times B_s \qquad (1)$$

$B_s \equiv 1\{G_s^{APTD} < G^{SSI}\}$ equals one for states where SSI's benefit floor was binding, and $d_s \equiv |G^{SSI} - G_s^{APTD}|$ is the absolute value of the gap between APTD benefits and SSI's floor. Figure 1 maps the variation in $G^{SSI} - G_s^{APTD}$, and shows that even neighboring states were affected very differently by these provisions. North Carolina's APTD benefit fell $138 below SSI's minimum compared to a gap of $296 in South Carolina. Wide differences appear between non-binding states as well, although state supplementation meant that these did not translate to post-SSI benefit changes. Michigan's APTD benefit was $414 over SSI's minimum, while Wisconsin's

was just \$89 above. Equation (1) defines three predicted effects of SSI that form the basis of our empirical strategy.

A. Benefits increased in proportion to how far APTD benefits were below SSI's level

In "binding" states, SSI increased maximum benefits by however much its national minimum exceeded the APTD benefit (d_s). Panel A of figure 2 provides clear evidence of this relationship. It plots the pre/post-SSI change in benefits per recipient (not statutory benefits) against APTD maxima in 1971, as well as non-parametric regression estimates. In this group, each \$100 difference between G_s^{APTD} and G^{SSI} was associated with about \$45 higher payments per recipient after SSI. (The change is not one-for-one because recipients with other income got less than the maximum.) Panel B shows a similar relationship for changes in participation rates, which follows partly from the fact that benefit increases mechanically raise eligibility.[13]

B. Benefits did not increase in proportion to how far APTD benefits were above SSI's level

In states where $1\{G_s^{APTD} < G^{SSI}\} = 0$ SSI's benefit floor did not bind and mandatory supplementation ensured that benefit levels did not change differentially in more versus less generous APTD states. Accordingly, figure 2 shows no correlation between APTD benefit levels and changes in either benefits per recipient or participation rates in non-binding states. In fact, without specifying the SSI minimum, the non-parametric regression clearly show a kink near SSI's benefit floor (and this is statistically significant in a piecewise linear specification).

C. Benefits increased immediately in 1974.

We expect SSI to have its biggest effects relatively quickly after it went into place on January 1, 1974.[14] Figure 3 plots average monthly benefits and participation rates in disability transfer programs from October 1950 to December 1980, and supports this prediction. Benefits spike immediately after January 1974, and while they fall relatively quickly thereafter due to rapid

[13] SSI also typically treated other income, including earnings, as well as assets, more generously than APTD. This reduction in the benefit tax rate also mechanically increases eligibility (Ashenfelter 1983).

[14] Although it had been passed two years before, Congress added provisions intended to prevent states from making anticipatory policy changes.

inflation, panel A makes it clear that SSI changed the value of disability transfers as soon as it took effect. Panel B shows similarly sharp changes in disability transfer participation.[15] In APTD's first 15 years participation rates reached one percent, but they grew by half this amount just in the two years following SSI's introduction.[16]

D. Did SSI's Non-Benefit Provisions Affect States Differently?

As a multifaceted national reform, SSI changed the way disability transfer programs operated in all states. Its attempts to reduce stigma and standardize disability determination processes, for example, may have affected take-up decisions nationwide, and publicity surrounding its implementation and targeted outreach likely made many potential recipients aware that it existed. If these factors differed systematically by APTD benefit levels our research design will necessarily capture a combination of the effects of higher benefits and changing time or stigma costs.

Figure 4 uses direct reports about these phenomena from the SLIAD to show that, at least from recipients' point of view, programmatic features of stigma did not change differentially by APTD benefits. Using 3,434 adults who responded to and received disability assistance in both waves of the SLIAD, we calculate state-level means of the share who said that SSI was "better than public assistance" or the difference across waves in the share who felt either "bothered by having to accept aid" or would be "embarrassed to admit" receiving aid. Panel A shows that about 80 percent of SSI recipients felt it was "better" than APTD/AB and, importantly, there is no correlation between this opinion and APTD benefits. Panel B shows that the likelihood that

[15] Participation appears not to increase exactly in January, but Berkowitz and DeWitt (2013) suggest that part of this came from problems with SSI's new computerized benefit system in the first few months.

[16] Figure 3 also reinforces our claim that SSI had bigger effects in the states where its benefit floor was binding. In 1973, low-benefit states paid about $150 less on average than high-benefit states, but SSI cut this gap in half. Similarly, participation rates in low-benefit states were 0.2 to 0.3 percentage points above the high-benefit states in the early 1970s, but this difference doubled soon after SSI. Participation rates were *higher* in states with lower benefits because those states were also poorer and had more adults with disabilities. We return to this point below.

recipients perceived stigma on SSI fell by about 20 percentage points relative to APTD/AB and, again, this pattern is uncorrelated with APTD generosity.

E. Expected Effects of Higher Benefits: Caseload Shifting

We expect SSI's benefit increases to bring many new disabled adults onto the program, especially in states with low APTD benefits (as shown in figure 2). Ashenfelter (1983) shows that higher benefit levels increase program participation mechanically by raising the break-even level of income below which someone is eligible, and by inducing behavioral reductions in income (in proportion to the compensated labor supply elasticity). However, given differences in benefit levels and funding streams across programs, both individuals and states also faced new incentives to shift caseloads away from other welfare programs and into the newly created SSI program. At the individual level, SSI typically paid higher benefits (on an individual rather than case basis) than AFDC or GA, and the children of an SSI recipient could remain on AFDC. This meant that moving a parent onto SSI would increase family income. Figure 5 illustrates these potential gains for a non-working adult by plotting (in circles) the difference between SSI benefits (including state supplements) and the adult portion of AFDC benefits against the 1971 maximum APTD benefit. Individuals gained the most from caseload shifting in the lowest APTD benefit states—about $400 per month—and the least in states where APTD was most generous.[17] Therefore, the incentive for individuals to switch programs is strongest in the areas where SSI was binding.[18]

At the state level, SSI was federally funded (except some state supplementary payments), so moving recipients off of partially or fully state funded programs and onto SSI would save the state money. State AFDC costs were highest where benefits and per-capita income (which

[17] Note again that our cross-state comparisons will necessarily net out any effect of SSI that operates nationwide through factors like stigma, changing application costs, or information.
[18] Wiseman (1975) is a vivid account of navigating welfare bureaucracy for AFDC, local transfers, and SSI.

determined the state's contribution) were both high. These were typically the same places that paid high APTD benefits. The open triangles in figure 5 plot the potential gain to states against 1971 APTD benefits. (States paid for all of GA, but we do not have information on its statutory benefit levels.) The gain from switching a recipient from AFDC to SSI equals the state share of the adult portion of the AFDC benefit minus the cost of the SSI state supplement. States stood to save relatively little in low-APTD-benefit states, however, it was clear that some states had recognized the benefits of caseload shifting even before SSI was implemented.[19]

F. Evidence on Caseload Shifting

Substantial evidence of caseload shifting from other programs to SSI has been documented in the later years of the program. For example, SSI has absorbed cases after related programs shrank. Bound, Kossoudji, and Ricart-Moes (1998) find that after Michigan eliminated its GA program, state outreach efforts led to increases in SSI applications. Schmidt and Sevak (2004) find that state-level waivers reforming welfare prior to 1996 led to a significant increase in the likelihood that single-mother families reported SSI receipt. Shifting to SSI has also been strongest among those who can receive higher benefits in other programs. Garrett and Glied (2000) find that in the years following the Sullivan v Zebley decision liberalizing child SSI eligibility, states with the highest AFDC benefits saw the smallest increases in child SSI participation. Kubik (2003) finds that families who were likely to receive higher levels of cash benefits from other programs were less likely to apply for SSI. Most closely related is Albritton (1979), who uses time-series methods to evaluate SSI's introduction. He finds large increases in disability participation as well

[19] Berkowitz and DeWitt (2013) write: "Members of Congress had received reports that New York was manipulating its welfare rolls. Local officials there realized that it was far more advantageous for a woman to be on the SSI rolls than on the Aid to Families with Dependent Children (AFDC) rolls, since SSI benefits were much cheaper to the state and higher to the beneficiary than AFDC benefits. As a consequence, the state rushed to transfer women with disabilities from AFDC to SSI in the hope that they might be grandfathered into the new program" (Berkowitz and DeWitt 2013, pg. 62).

as reductions in AFDC by extending pre-SSI time-series parameter estimates to the post-SSI period.

Figure 6 provides initial evidence both that SSI led to large growth in adult safety net participation and that caseload shifting occurred in 1974. It plots the share of adults who received any cash transfer over time, including disability benefits. Most clearly, all states experience large jumps in participation after 1974. In figure 3, binding states experienced relative increases in disability participation, but in figure 6 the jump it total participation is about the same size in binding and non-binding. This is exactly what we would expect from caseload shifting because it suggests that something offset the relative increases in disability participation in the places where SSI raised benefits the most.

IV. Empirical Strategy: Difference-in-Differences Using Pre-SSI Benefits

We test for the three expected patterns—sharp changes starting in 1974, a proportional relationship between distance to SSI in binding states, and no relationship between distance to SSI in non-binding states—in the following event-study specification for outcome y_{st} for state s in year t:

$$y_{st} = \alpha_s + \boldsymbol{\beta}'\boldsymbol{X_{st}} + \left[\alpha_t + \sum_{t=1950}^{1973} \lambda_t^U \alpha_t d_s + \sum_{t=1974}^{1980} \gamma_t^U \alpha_t d_s\right] B_s + \left[\alpha_t + \sum_{t=1950}^{1973} \lambda_t^O \alpha_t d_s + \sum_{t=1974}^{1980} \gamma_t^O \alpha_t d_s\right](1 - B_s) + \epsilon_{st} \qquad (2)$$

α_s are state fixed effects, and $\boldsymbol{X_{st}}$ includes the share of each year that a state operated an APTD program or an AFDC-UP program and year effects for groups of states that implemented Medicaid in different years. α_t are year fixed effects and we allow them to differ for states that were above or below SSI's minimum benefit.

The event-study interactions between year dummies and the distance to the SSI benefit floor (d_s defined in equation (1)) trace out changes in the relationship between outcomes and generosity in each year before and after SSI. (We scale d_s by 100, so all coefficients refer to a \$100 difference between G_s^{APTD} and G^{SSI}.) The interaction of these variables with B_s and $1 - B_s$ reflects the prediction that pre-SSI generosity should have much different effects depending on whether SSI's benefit floor did or did not bind. The λ_t coefficients are falsification tests that show whether trends in safety net outcomes were correlated with APTD generosity in the 1950s, 1960s, and early 1970s (prediction 3). The γ_t^U coefficients test for relative changes in outcomes after SSI in the lowest benefit states (prediction 1). The γ_t^O coefficients have a similar interpretation, but reflect changes in the correlation between outcomes and APTD benefit levels for the high benefit states where SSI did not bind. Figure 2 suggests that these coefficients should be close to zero because higher benefit states did not experience differential benefit increases (prediction 2).

Motivated by the event-study results we also estimate specifications that omit the interactions for the "non-binding" states (whose coefficients are zero), and replace the event-study dummies for the binding states with a time-trend and post-SSI trend breaks interacted with d_s:

$$y_{st} = \alpha_s + \alpha_t + \boldsymbol{\beta}'\boldsymbol{X_{st}} + [\alpha_t + \Lambda_t^U(t - 1973)d_s + \Gamma_t^U(t - 1973)1\{t > 1973\}d_s]B_s + \epsilon_{st} \quad (3)$$

$\widehat{\Lambda}_t^U$ test for differential linear pre-trends in the low-benefit states (and will have higher power to detect them than the event-study estimates), and $\widehat{\Gamma}_t^U$ measures the difference in outcomes per year due to a \$100 difference in APTD benefits. These reduced-form specifications increase power and provide a single parameter measuring SSI's state-level effect.

Finally, we summarize these magnitudes using an instrumental variables (IV) model with the same specification as in (3), but that uses the post-SSI trend break in binding states as an

instrument for the disability variables (participation rates or per-capita transfers). The results equal the ratio of the $\hat{\Gamma}_t^U$ from a model for non-disability outcomes to the same estimate for disability outcomes. The participation results then reflect the change in the number of non-disability or overall recipients for each new SSI recipient, and the per-capita transfer results reflect the change in per-capita transfer income for each $1 increase in per-capita SSI income.

A. Correlates of 1971 APTD Benefit Levels

Internal validity of our design requires that no other important determinants of changing safety net outcomes correlate with APTD generosity in the specific way that SSI did.[20] Fortunately, our long time-series and SSI's unique structure go a long way toward ruling out these kinds of confounders. First, our event-study results show pre-trends directly and separately for binding and non-binding states. Second, if the determinants of APTD generosity were correlated with sudden unobserved changes in 1974, we would expect to see evidence of it in all states, not just the binding states directly affected by SSI. Third, evidence of caseload shifting will show increases in SSI participation, but *decreases* in other program participation, while confounding changes in factors like labor demand, program stigma, or bureaucratic burdens would tend to move participation in these programs in the same direction. Therefore any sources of bias must be correlated with benefit levels and outcomes only for low-benefit states, only after 1974, and in opposite directions for disability and non-disability programs.

We test for such confounders using data from the 1960 through 1980 Censuses. We first regress APTD participation rates in 1960 on a range of demographic and economic characteristics from the 1960 Census, and use the coefficients to predict participation rates in

[20] In fact, many changes in the 1970s could have affected the population targeted by SSI. For example, the 1970 Clean Air Act led to large changes in employment in regulated areas (Greenstone 2002), the EITC's introduction in 1975 increased employment among single mothers (Bastian 2018); and President Nixon's War on Cancer reduced mortality rates potentially among those on the margin of SSI participation (Honoré and Lleras-Muney 2006).

1970 and 1980.[21] Figure 7 plots predicted participation rates in 1970 and the change in predicted participation rates from 1970 to 1980 against the 1971 APTD benefit. Predicted participation is slightly higher in states with very low benefits, but the relationship does not have the same kinked pattern that SSI predicts. More important for our design, *changes* in predicted participation do not vary systematically with APTD generosity. Unlike figure 2, which shows a clear kinked relationship between *actual* participation changes and pre-SSI benefits, figure 6 shows that changing economic and demographic characteristics were not the cause.

The 1960 Census does not include one key variable that could explain increasing participation in disability programs but decreasing participation in other programs: the rate of work-limiting disability. Appendix figure A1 shows that in fact disability rates in 1970 were higher in lower benefit states, and they display a kink around the SSI benefit level.[22] Importantly, though, these differences in disability rates did not change between 1970 and 1980, when we may be concerned that underlying changes could cause bias.[23] The imbalance in disability levels may affect the interpretation of our estimates (see de Chaisemartin and D'HaultfŒuille 2018), but not necessarily their internal validity. The striking balance in disability *changes* shows that estimates of equation (2) will not confound trends in health-related eligibility with SSI's effect on benefits.

V. Results: SSI's Differential Effect Across States

Figure 8 presents our main evidence that SSI's benefit floor increased disability benefit take-up partly at the expense of participation in other adult programs. We plot estimates of the event-

[21] Characteristics include the share of adults who are institutionalized, male, white, employed, out of the labor force, poor, veterans, married, living with parents, under age 40, between age 40 and 49, or have either 12 or 16 years of education. We also include the average age, average individual income, and dummies for the year in which states implemented Medicaid.

[22] This is likely part of the reason why disability participation is higher in low-benefit states.

[23] We may also expect SSI to increase the probability of reporting a disability (Bound, Schoenbaum, and Waidmann 1996). Schmidt and Sevak (2004), however, find no effect of AFDC waivers on reported disability rates (as a means to qualify for SSI).

study coefficients from equation (2) for the binding states and non-binding states. The flat pre-trends in panel A show that the evolution of disability program participation was uncorrelated with benefit levels during APTD's first 24 years. Immediately after SSI took effect, however, participation jumped in states with lower benefits under SSI's floor (predictions 1 and 3), but bore no relationship to APTD benefits where SSI did not bind (prediction 2). By 1980, states whose APTD benefits were $100 farther below the SSI floor had added an additional 0.4 percent of adults to the SSI rolls. Whatever the average growth in SSI participation in the high benefit states, it did not differ by APTD benefit generosity.

This result confirms that SSI worked as intended—it raised benefits and participation the most in parts of the country that had been the least generous. Panel B shows that, consistent with strong caseload shifting, these same areas saw relative reductions in participation in the other adult assistance categories, AFDC and GA. We again find no evidence that other welfare participation trended differentially between 1950 and 1973.[24] Non-binding states did not have systematically different change in other welfare participation according to their APTD generosity.

The first panel of Table 2 summarizes the event-study results using the reduced-form trend-break specification the in equation (3). In each year after SSI started, states that were $100 farther below the benefit floor gained 0.05 additional percentage points in disability participation (column 1; 95% C.I. = 0.00023, 0.00075), but lost 0.038 percentage points in other welfare participation (column 2; 95% C.I.: -.000079, 0.0002). We find no significant change in overall

[24] APTD generosity is correlated with some changes in after 1962 when states gained the option to extend AFDC to two-parent families (AFDC-UP). While we control for the share of the year that states operated any such program, we have no way to control for how restrictive it was or how many adults participated, and we interpret these changes as stemming from AFDC-UP. In appendix figure A4 we show that when using AFDC cases rather than adult recipients these pre-SSI shifts are much smaller. This makes sense because adding one AFDC-UP case actually adds two adults. The fact that these shifts are larger for recipients than adults (and line up with the timing of UP's introduction), suggest that we are picking up heterogeneity in the scale of state AFDC-UP programs.

welfare participation, which is the difference in the other two estimates (0.00011, 95% C.I.: -0.00031, 0.00053).

Panel B quantifies the degree of caseload shifting and SSI's effect on total participation rates in terms of its obvious impact on disability participation. To see how, note that if lower benefit states added 0.05 percentage points per year in disability participation at the expense of 0.038 percentage points in other welfare participation, then 0.77 (0.038/0.05) recipients left non-disability programs for each person that got SSI. Since the IV estimate is just identified it exactly equals this ratio (95% C.I.: -1.51,-0.03). Column 3 again shows no strong evidence that SSI differentially affected adult welfare participation rates overall.

While SSI did not have large effects on overall welfare participation in the least generous states, it did raise benefits above AFDC levels and so may have boosted incomes by moving recipients onto a more generous program. To test this, table 3 presents reduced-form and IV estimates for per-capita transfer income. We find that reductions in income from non-disability programs are about half the size of the increases in disability transfer income due to SSI, but these estimates are much less precise. The IV estimates in panel B imply that for each dollar transferred by SSI, adults received $0.55 less from non-disability programs (95% C.I.: -1.34, 0.24), raising per-capita transfer income by just $0.46. The confidence interval for total per-capita transfers, however, includes both *reductions* and values as high as $1.20 (95% C.I.: -0.33, 1.21).

A. How do we know this is caseload shifting?

Figure 9 shows that these findings do not depend strongly on the particular specification we use. The disability results are nearly identical without any covariates (except state and year fixed effects and their interaction with B_s), or when we weight by 1950 adult population. The results are smaller when we control for separate year fixed effects by region or by quartiles of the 1970

work-limiting disability rate. In fact, these two sets of controls are similar. SSI-induced benefit increases were largest in the South (Figure 1) which had 12 out of the 13 states in the highest disability quartile. Panel A shows that our three predictions about SSI's effect are still apparent even within these narrow groups of states. Panel B shows similar robustness, although the differences across specifications mainly appear in the pre-period. The negative trend break in non-disability participation after 1974 cannot be explained by simple specification problems, regional factors, or changes in safety net correlated with pre-existing disability prevalence.[25]

We also use the structure of AFDC to provide additional support for the claim that the participation declines in figure 8 actually represent shifting and not some other confounding trend in AFDC. Parents who switched from AFDC to SSI would create one new SSI recipient and one fewer AFDC recipient, but because their children remain on AFDC, this would not change the number of AFDC *cases*. This suggests that shifting should have a larger effect on an AFDC measure that uses adult recipients in the numerator as opposed to cases. Appendix figure A4 and table A1 shows that, in fact, our estimates using AFDC cases per adult about only half as large as when we used adult recipients. This does not come from differences in the baseline means: there are actually more cases than adult recipients (for example, if the AFDC children lived in a foster home or, as in the caseload shifting context, parents received other programs). If the post-1974 reductions in non-disability participation came from new restrictions on eligibility, for example, we should see a reduction in cases and not just recipients.[26]

[25] Appendix figure A5 plots event-study estimates for the ratio of non-farm employment to the adult population. Using this rough measure, we find no evidence of post-1974 changes in employment that are correlated with APTD benefits. This suggests that labor demand changes, for example due to the 1973 recession, cannot explain our results, but also that SSI's introduction may not have had large employment effects (cf. Neumark and Powers 2005), although this conclusion is beyond the scope of this paper.

[26] Appendix figure A2 presents event-study results for payments per recipient and shows that, while disability payment levels increased sharply after SSI in lower-benefit binding states, payment levels in other programs experience no differential changes across states after SSI. Our shifting result therefore does not come from either a confounding changes in benefit policy nor does it appear to come from states cutting benefits to induce recipients to

Our results based on state-level aggregates do not necessarily allow us to conclude that new SSI recipients came from other programs. We present direct evidence that disability benefit receipt grew specifically among AFDC families using the AFDC Surveys described above. For each state and year we calculate the share of AFDC households that contain an adult who receives disability benefits. Nationwide this share rose from 5.5 percent in 1967 to 9.3 percent in 1977. Panel A of figure 10 scatters the 1967-1977 change in this outcome for each state against APTD benefit levels. As in figure 2, we see a clear relationship between movements onto SSI, this time *among* AFDC households, and APTD generosity in binding states, but no such relationship in non-binding states. Panel B is a falsification test that shows no relationship between APTD generosity and changes between two pre-SSI years, 1967 and 1973. Table 5 reports difference-in-differences estimates that summarize these results by interacting d_s with a post-SSI dummy separately for binding and non-binding states. Our preferred specification (column 2) shows that disability participation among adults of AFDC families grew 2.2 percentage points more after SSI for each additional $100 below the benefit floor. The effects are smaller after conditioning on region or pre-existing disability controls, but even within these narrow groups of states there is still a positive relationship between SSI-induced benefit increases and shifting from AFDC to SSI.[27]

VI. Discussion: SSI and the Size of the Safety Net

Our results are among the first to evaluate how SSI's introduction affected safety net participation and spending.[28] SSI clearly increased participation and spending on disability programs. About 1.2 million disabled adults received APTD in 1973, while 2.2 million received

move to AFDC. It is still possible that states took actions to shift recipients, but these actions do not appear in benefit levels.

[27] In the SLIAD, 31 percent of disabled AFDC recipients in 1973 moved onto SSI in binding states compared to 16% in non-binding states.

[28] Albritton's (1979) results for SSI's introduction are also consistent with caseload shifting, but the validity of the time-series approach is hard to verify. He found that *more* recipients left AFDC than went on SSI, implying a rate of caseload shifting above one.

SSI in 1980. But SSI did not increase overall adult welfare participation by nearly this much. Our cross-state design suggests that about three quarters of disabled SSI recipients who were induced to participate because of benefit increases left the AFDC or GA rolls. Therefore, SSI, "our first federal income guarantee (Burke and Burke 1974, pg 188)" represents a large shift in the composition of adult safety net assistance. Between 1973 and 1980, the share of adults on welfare who received disability payments grew from 25 to 36 percent. Without any caseload shifting it would have only reached 31.5 percent. About 300,000 disabled SSI recipients "shifted" from other programs.

This estimate is in line with results about caseload shifting to SSI results in other contexts. Schmidt and Sevak (2004) find that AFDC waivers increase SSI participation among single mothers by 0.6 percentage points, while Schoeni and Blank (2000) find that they reduced AFDC participation by 0.86 percentage points. This implies that about 70 percent (0.6/0.86) of those who were "pushed" off of AFDC switched to SSI, very similar to our finding that 77 percent of those "pulled" onto SSI came from AFDC or GA. For comparison, Kline and Walters (2016) estimate that about one third of Head Start participants were drawn from other public preschools.

The rate at which a dollar of per-capita SSI spending translates to a dollar of per-capita income, which we find to be about 0.5, is also a key parameter in models of fiscal federalism. In these models, altruistic taxpayers redistribute income until their marginal utility of income equals the marginal utility of per-capita income for "the poor" times the cost of actually raising their per-capita income by a dollar. Factors that make it more costly to redistribute income include the relative numbers of tax payers and poor people, changes in labor supply, in-migration of poor people from other jurisdictions (Brown and Oates 1987) or its wage effects (Brueckner 2000), higher local financing requirements (Orr 1976), or positive externalities from altruistic

preferences among non-local taxpayers (Oates 1972). All of these costs are generally smaller from the point of view of the average national taxpayer, so this literature typically concludes that a national redistribution program would be larger than a series of local ones. But these models have only considered a single program. Our results suggest that federalizing just part of the safety net has a smaller effect on the size of cash transfer programs than it appears because recipients can switch programs to increase income or states can shift cases to save money.

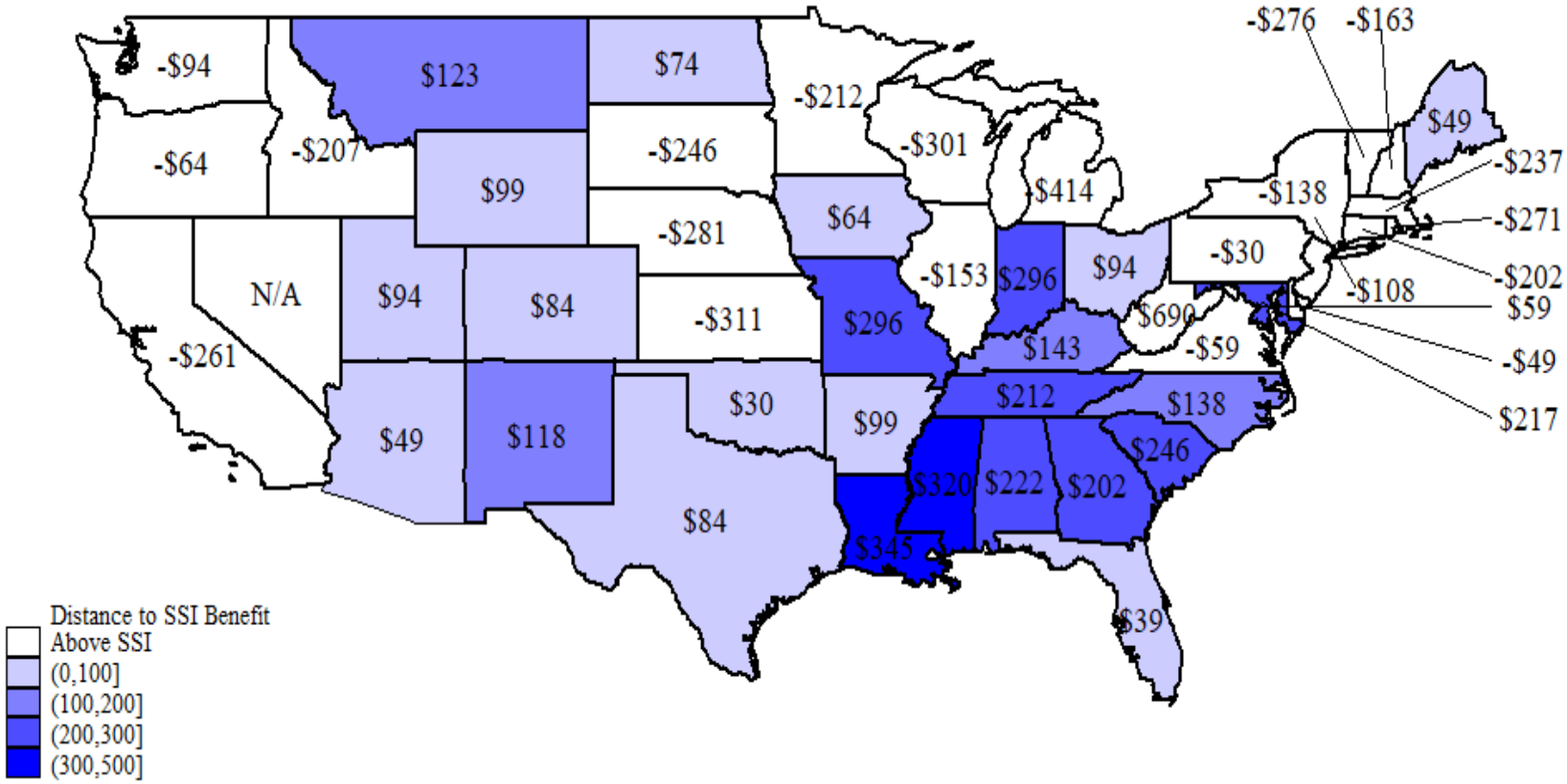
Three important caveats apply to our results. First, we cannot identify whether caseload shifting came from recipient or state decisions. Since individual incentives correlate more closely to our cross-state identifying variation in APTD benefits than state incentives do, we find it more likely that our effects derive from individual behavior. But states stood to gain, too, if they shifted many recipients, and we cannot rule this out as an explanation. Second, we cannot identify effects of SSI that did not differ across states, including a potentially important role for widespread changes in stigma, time costs, or information. Therefore, our results on shifting apply to individuals who switched programs because of the benefit changes generated by SSI's national minimum. Finally, while the confidence intervals for some key estimates, such as the caseload shifting "rate" in table 2, rule out zero, they do not rule out very large or very small amounts of caseload shifting. Additional evidence with a higher-powered design (adding additional cross-section comparisons in microdata, for example) could provide a more accurate estimate of the extent of caseload shifting. Our state-by-year design, however, clearly shows that it took place.

VII. Conclusion

SSI's introduction is a watershed moment connecting the birth of the modern safety net in 1935 to its current split into either state block grants or federal entitlements. As intended, SSI led to large increases in benefit levels and participation in disability transfer programs, particularly in

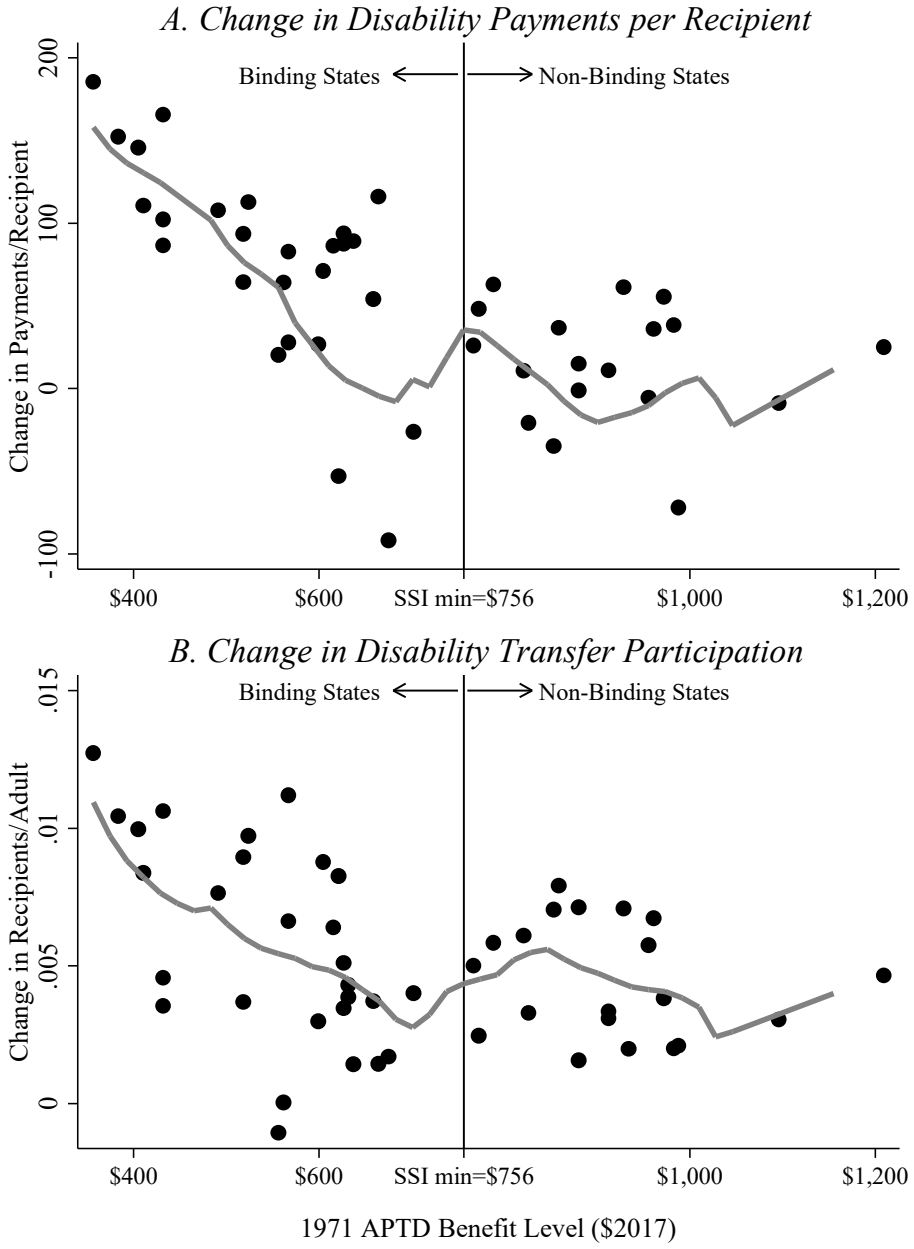
parts of the country that had been the least generous. An unintended consequence, however, was that many adult left non-disability programs that continued to pay low benefits and cost states money in order to get on the new, generous, federally funded SSI program.

Figure 1. The Gap Between SSI's Benefit Floor and Pre-Existing Disability Benefit Levels Across States



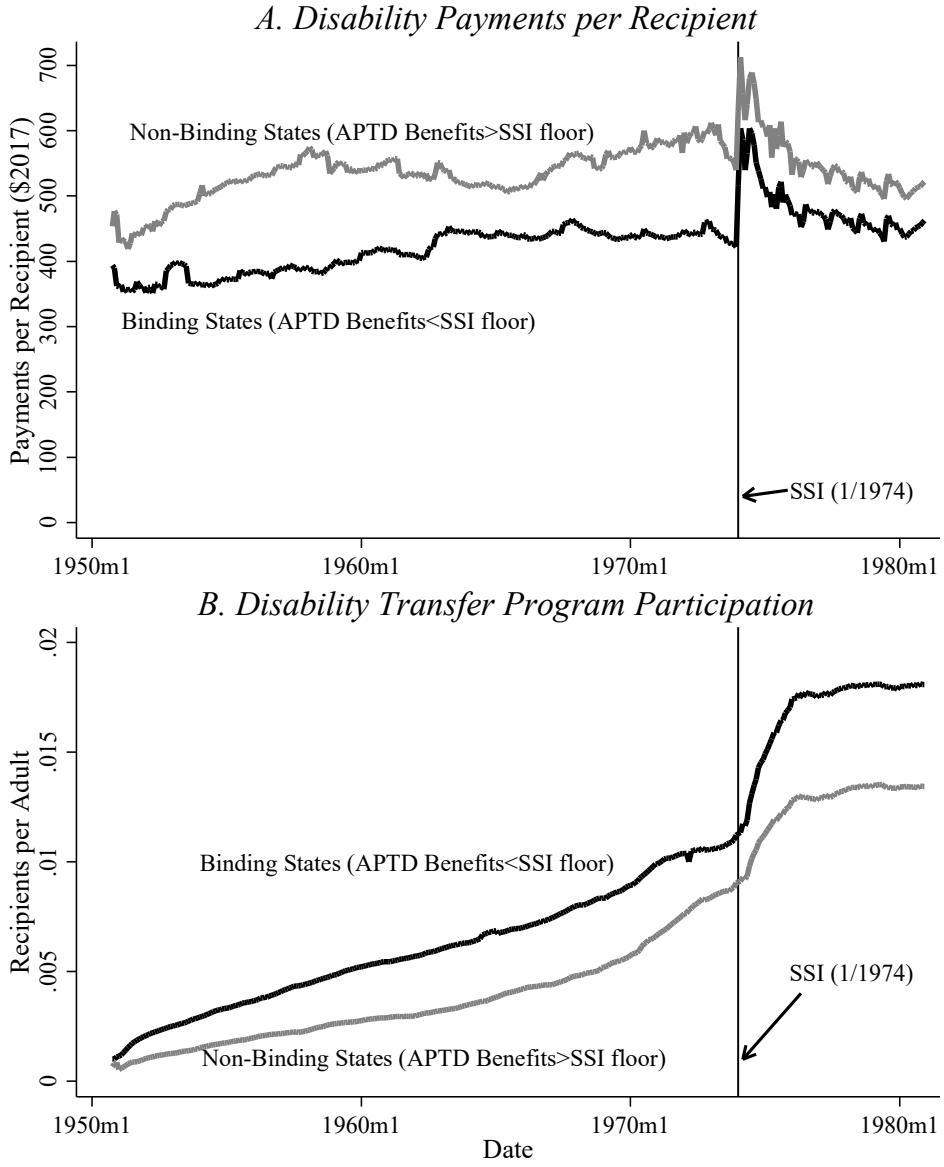
Notes: The figure maps the difference between SSI's initial minimum benefit for a single adult and the comparable APTD benefit in 1971. Positive numbers indicate binding states (shown in darker shades of blue) and negative numbers indicate non-binding states (shown in white).

Figure 2. The Kinked Relationship between Changes in Disability Payments per Recipient and Disability Recipients per Adult and pre-SSI APTD Benefit Levels



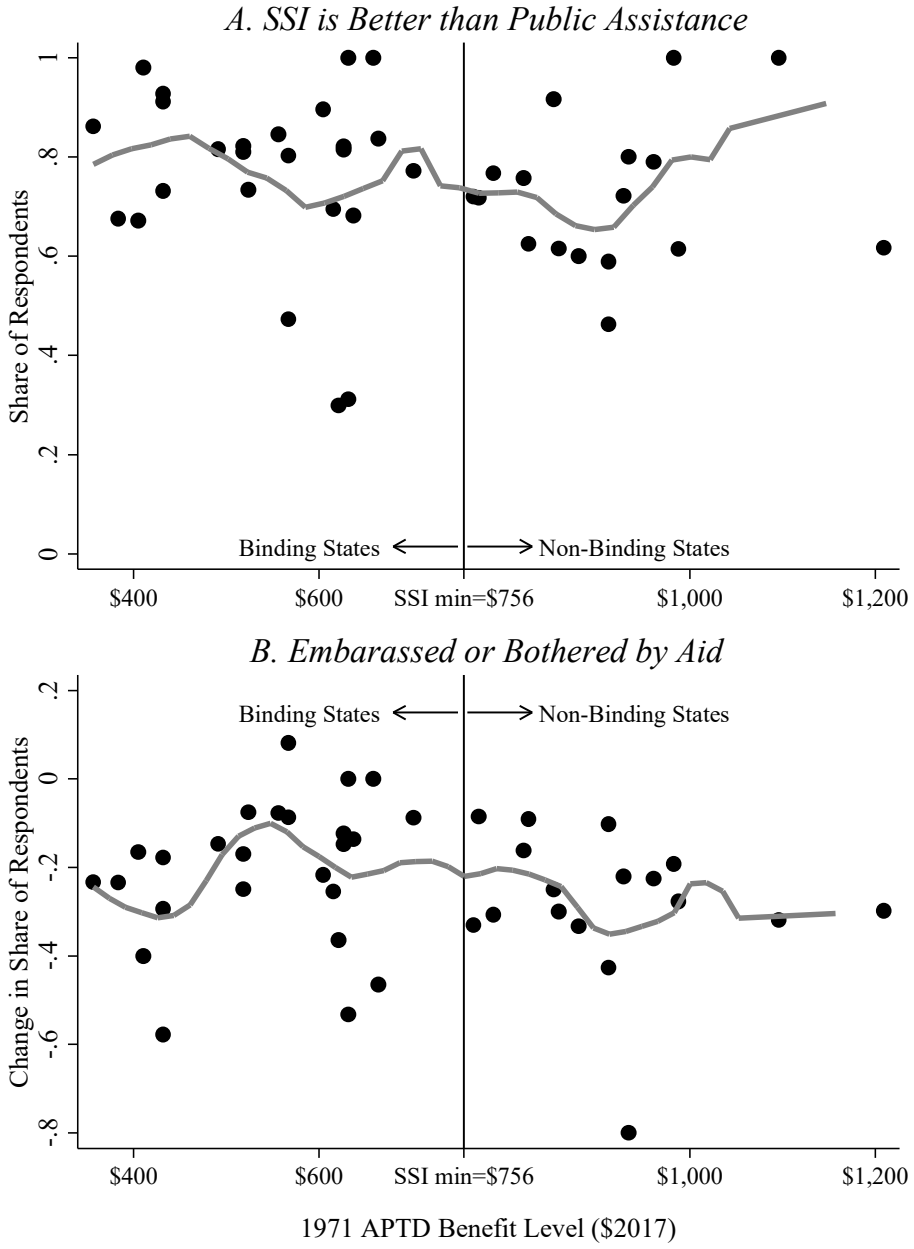
The figure plots the change (averaged over 1970-1973 (pre-SSI) and 1974-1977 (post-SSI)) in payments per recipient (Panel A) and participation rates (Panel B) against the maximum APTD benefit in 1971 expressed in 2017 dollars. The figure also includes nonparametric regression estimates using an Epanachnikov kernel with bandwidth of 30. Panel A shows convergence in benefits per recipient after SSI for states previously below its minimum, but not for those above. The linear fit is -58 per $100 (s.e. = 14.2) in the binding states with a trend-break of 53.5 per $100 (s.e.=21.0) in the non-binding states. Panel B shows that the change in participation was largest in the lowest-APTD-benefit states, but had no relationship with APTD benefits above SSI's minimum. The linear fit is -0.002 per $100 (s.e. = 0.0005) in the binding states with a trend-break of 0.0017 per $100 (s.e.=0.0007) in the non-binding states.

Figure 3. Payments per Recipient and Participation Rates in Means-Tested Disability Programs by Pre-SSI Benefit Level, 1950-1980



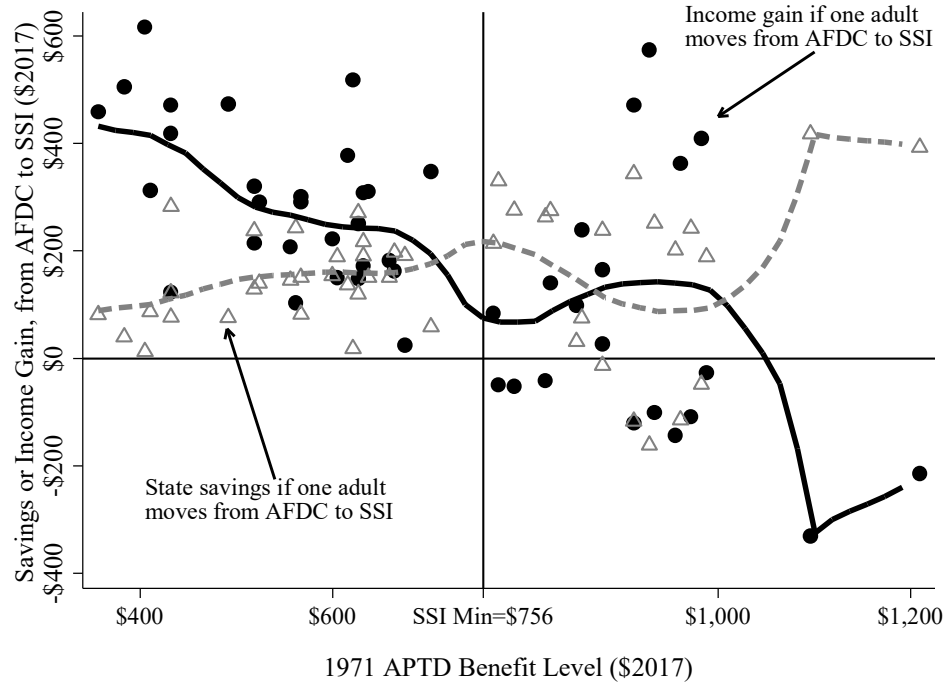
Notes: The figure plots payments per recipient (panel A) and participation rates (panel B) for disability transfer programs calculated separately for states with 1971 APTD maximum benefits that were above (gray line) or below (black line) SSI's benefit floor.

Figure 4. No Relationship between APTD Benefit Levels and Changes in Recipient's Assessment of Program Quality or Stigma



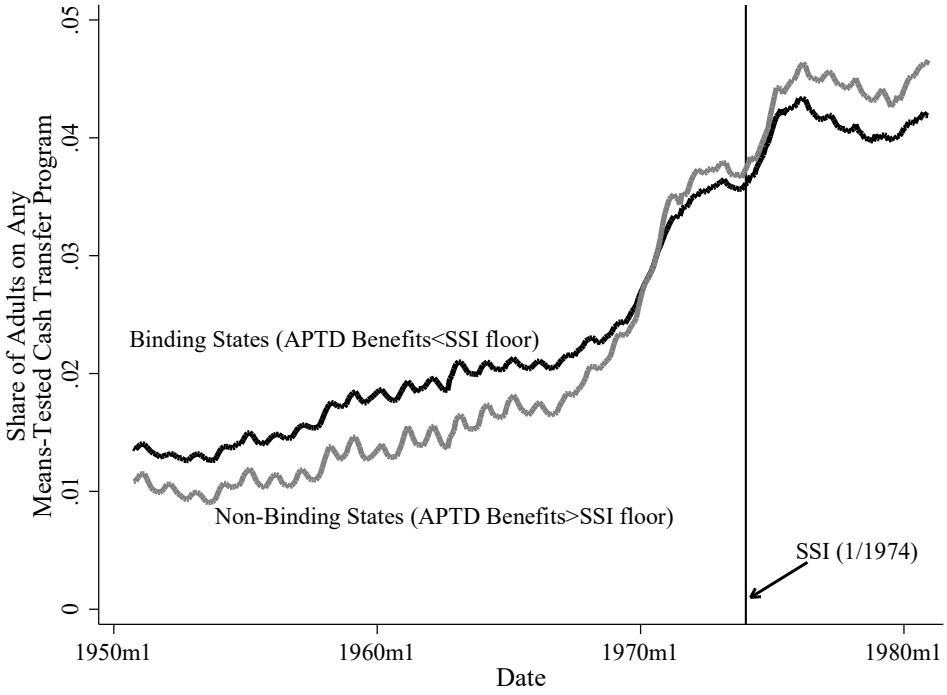
Notes: The figure plots recipients' assessment of SSI relative to APTD/AB in panel A and the change from 1973 to 1974 in disability recipient's likelihood of feeling "bothered" or "embarrassed" about receiving benefits. The figure also includes nonparametric regression estimates using an Epanachnikov kernel with bandwidth of 30. The linear fit in panel A is an insignificant -0.0003 (s.e. = 0.0003) in the binding states with an insignificant trend-break of 0.0004 (s.e.=0.0005) in the non-binding states. The linear fit in panel B is an insignificant 0.0003 (s.e. = 0.0003) in the binding states with an insignificant trend-break of -0.0006 (s.e.=0.0004) in the non-binding states. Source: Survey of Low Income Aged and Disabled (Social Security Administration 1992).

Figure 5. The Relationship between State Savings and Individual Income Gains to Switching from AFDC to SSI



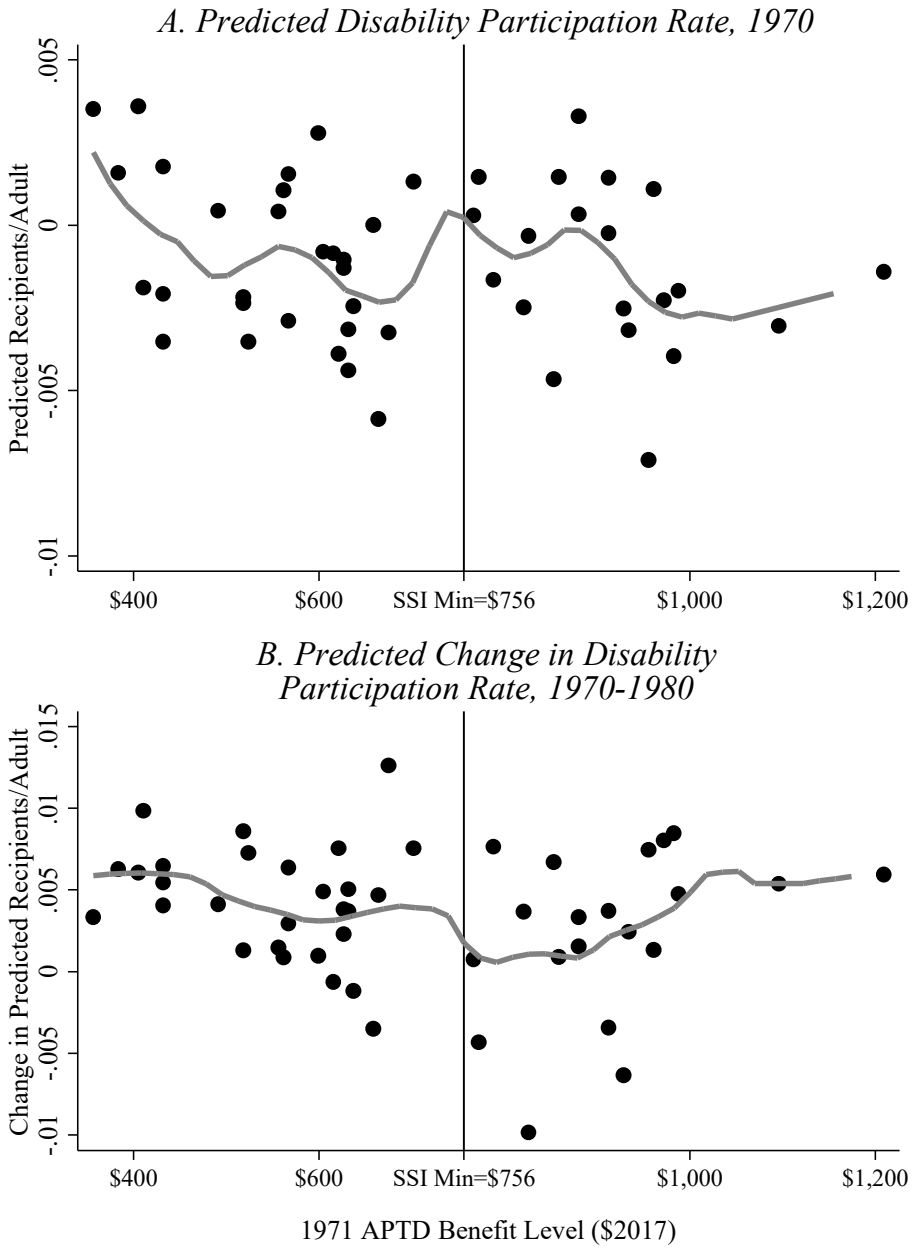
Notes: The figure plots the potential savings to states and the potential gain to families from moving one adult from AFDC to SSI. State savings equal the state's share of the adult portion of the maximum AFDC benefit level in 1973 minus its supplementation amount for basic needs for an individual disabled beneficiary living alone. The federal/state cost sharing rate equaled the federal medical assistance percentage (FMAP) for most states. We estimate the adult portion of the maximum benefit by comparing the total benefit for a family of 4 to a family of 2, and subtracting half the differences from the 2-person benefit level. The individual supplementation policy comes from Rigby and Morrison (1975), and does not include supplements given to recipients living with others or in institutions, or who receive supplements for "special" needs. The potential income gain to an individual from switching equals the total SSI benefit (federal minimum plus state supplementation) minus the adult portion of the AFDC max. The *x*-axis equals the nominal maximum APTD benefit level in 1971. The figure also includes nonparametric regression estimates using an Epanachnikov kernel with bandwidth of 50.

Figure 6. Any Public Assistance Participation Rates by Pre-SSI Benefit Level, 1950-1980



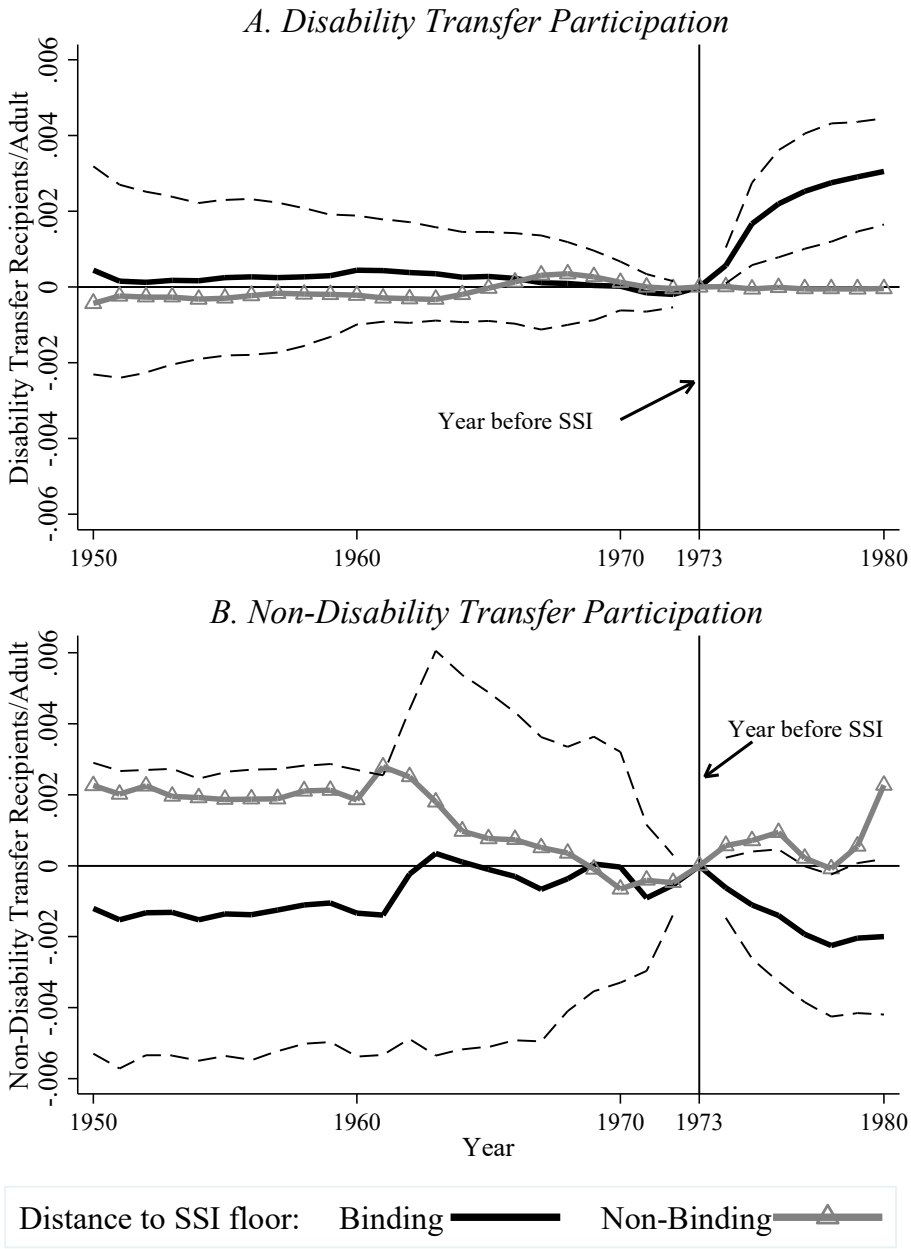
Notes: The figure plots adult participation rates in any cash transfer program (including AFDC, APTD, AB, GA, or SSI) calculated separately for states with 1971 APTD maximum benefits that were above (gray line) or below (black line) SSI's benefit floor. While figure 3 shows that disability participation rates in low-benefit states grew by about 0.8 percentage points after SSI, this figure shows an increase in overall participation rates of just 0.4 percentage points in the same states.

Figure 7. Predicted Participation in 1970 and Predicted Change in Participation from 1970-1980 Based on 1960 Characteristics Are Not Related to APTD Benefit Levels



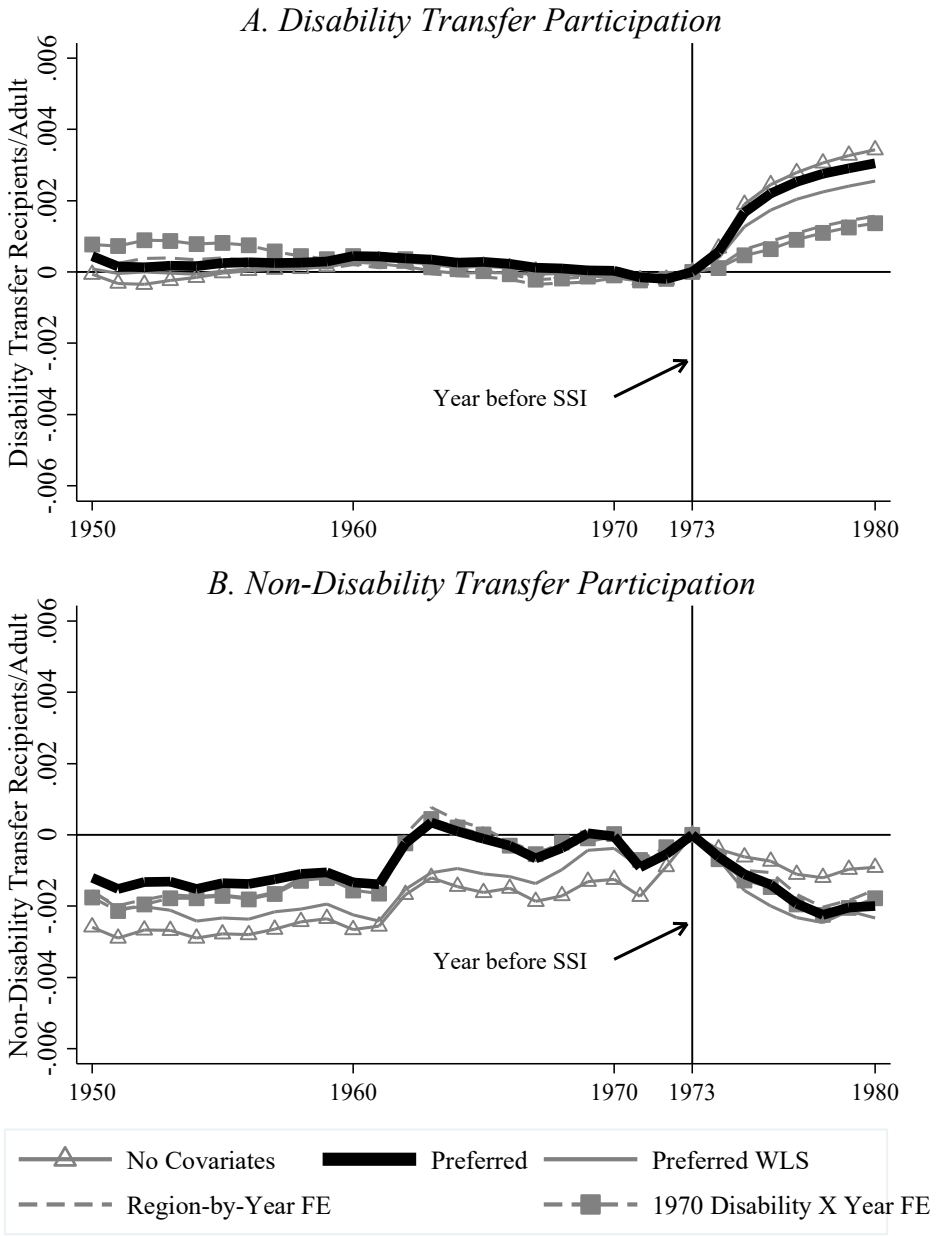
Notes: The figure plots predicted disability transfer participation in 1970 and the change in predicted participation from 1970 to 1980 against the 1971 APTD benefit level. Predictions come from a cross-sectional regression of 1960 state APTD participation rates on the share of adults who are institutionalized, male, white, employed, out of the labor force, poor, veterans, married, living with parents, under age 40, between age 40 and 49, or have either 12 or 16 years of education; and the average age, average individual income, and dummies for the year in which states implemented Medicaid. Gray lines are nonparametric regression estimates using an Epanechnikov kernel with bandwidth of 30.

Figure 8. The Effect of SSI's Benefit Floor on Participation in Disability and Non-Disability Transfer Programs



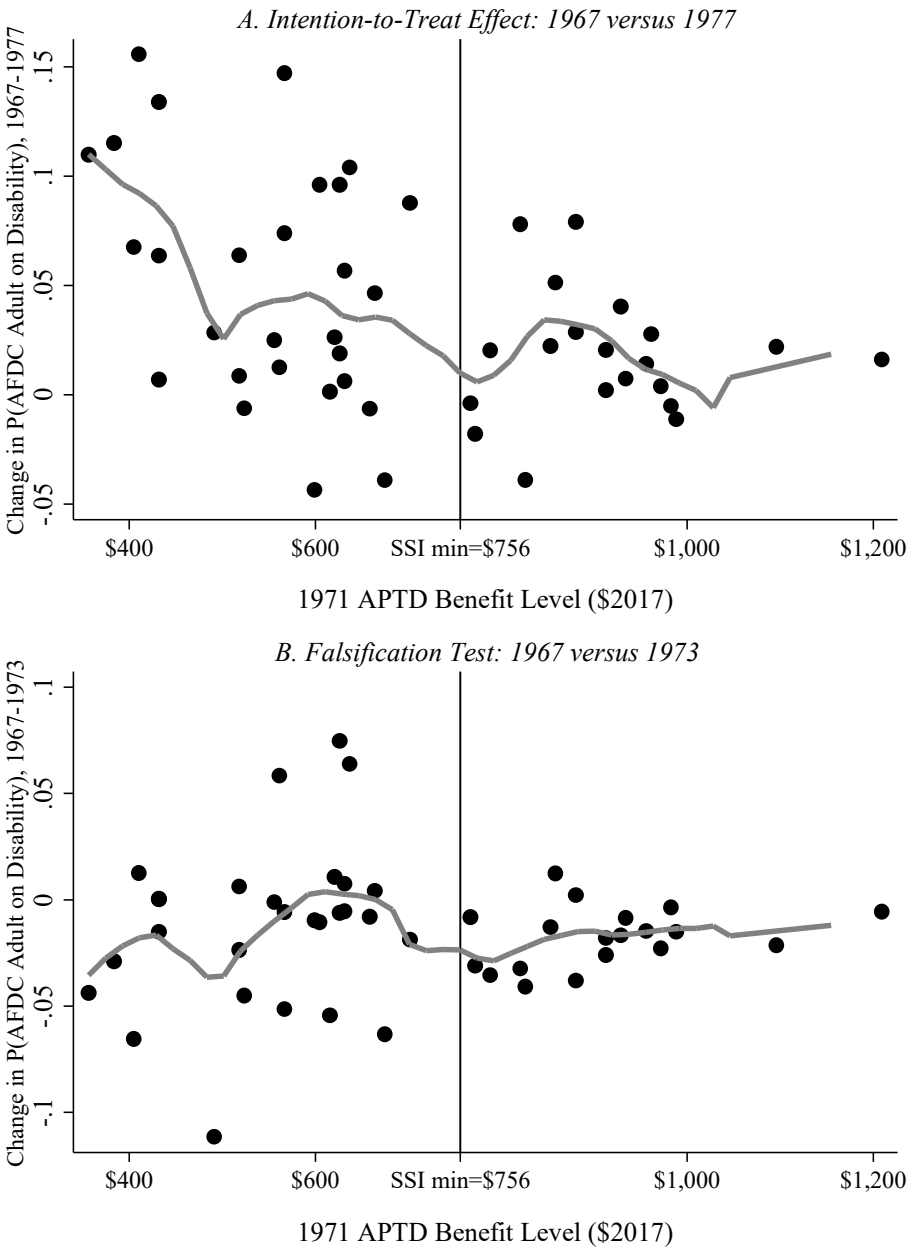
Notes: The figure plots estimates of λ_t^U and γ_t^U (in black with 95-percent confidence intervals based on standard errors clustered by state in dashed lines) and λ_t^O and γ_t^O (in gray) from equation (2). In addition to fixed effects for states and years (and their interaction with an above-SSI dummy), the model also controls for the share of each year that states operated APTD or AFDC-UP programs, and separate year fixed effects for each Medicaid timing group.

Figure 9. Robustness of the Estimates Across Specifications for Binding States



Notes: The figure plots estimates of λ_t^U and γ_t^U from alternative specifications of equation (2). No covariates refers to equation (2) without $\boldsymbol{X_{st}}$. WLS results are weighted by the 1950 adult population. The "1970 Disability X year" controls are interactions of year fixed effects with quartiles of states disability rates in 1970.

Figure 10. Direct Evidence of Shifting: The Relationship between APTD Benefits and Changes in the Probability that AFDC Adults Received Disability Transfer Income



Notes: The figure plots change in the share of AFDC households where an adult (either the AFDC mother or father) received disability income (APTD or AB in 1967 and 1973, SSI in 1975 and 1977). Panel A is a scatter plot and smoothed fit for the change before and after SSI in 1967 and 1977. Panel B is a falsification test that plots the changes between two pre-SSI years, 1967 and 1973. Sources: DHEW (2011).

Table 1. Summary Statistics: Adult Welfare Participation and Spending

	1950	1960	1970	1980
	A. Participation and Spending			
Adult Population (millions)	87.72	93.69	106.82	128.85
Adult Recipients				
APTD/SSI	73,368	329,660	854,767	2,245,046
AFDC	562,189	644,030	2,172,735	3,274,263
GA	387,966	390,337	497,667	754,644
Millions in Total Spending ($2017)				
APTD/SSI	$30.59	$148.97	$474.36	$1,181.70
AFDC	$416.87	$626.23	$2,319.99	$2,926.40
GA	$164.21	$208.86	$303.73	$339.05
	B. Outcome Measures			
Participation Rate				
APTD/SSI	0.0008	0.0035	0.0080	0.0174
AFDC	0.0064	0.0069	0.0203	0.0254
GA	0.0044	0.0042	0.0047	0.0059
Benefit Level				
APTD/SSI	$416.94	$451.88	$554.96	$526.36
AFDC	$741.52	$972.36	$1,067.77	$893.76
GA	$423.25	$535.06	$610.31	$449.29
Per-Capita Benefits				
APTD/SSI	$0.35	$1.59	$4.44	$9.17
AFDC	$4.75	$6.68	$21.72	$22.71
GA	$1.87	$2.23	$2.84	$2.63

Notes: This table presents summary statistics from the basic items in our source data, participant counts and spending totals, and for the main outcomes we consider, adult participation rates, benefit levels, and per-capita benefits.

Table 2. SSI and Transfer Participation Rates: Post-SSI Trend-Breaks and IV Estimates of the Effect of Each SSI Recipient on Other Transfer Program Recipients

	(1) Disability Transfer Program	(2) Non-Disability Transfer Programs	(3) Any Transfer Program
	A. Reduced-Form Trend-Break Estimates		
Post-SSI Trend Break, Binding States:	0.00049	-0.00038	0.00011
	[0.00013]	[0.00021]	[0.00022]
95% C.I.	*(0.00023, 0.00075)*	*(-0.00079, 0.00002)*	*(-0.00031, 0.00053)*
1973 DV Mean|APTD<SSI	0.010	0.024	0.034
	B. Instrumental Variables: Effect per New SSI Recipient		
Disability Transfer Recipients		-0.77	0.22
		[0.38]	[0.38]
95% C.I.		*(-1.51, -0.03)*	*(-0.52, 0.96)*

Notes: This table presents estimates effects of SSI on adult transfer program participation rates. Panel A contains estimates of Γ^U from equation (3), and panel B contains IV estimates that use the post-SSI trend break as an instrument for disability program participation. These estimates equal the ratio of the trend break estimates in in columns (2) and (3) to the estimate in column (1). We cluster standard errors by state.

Table 3. SSI and Per-Capita Transfer Income: Post-SSI Trend-Breaks and IV Estimates of the Effect of Each Per-Capita SSI Dollar on Other Per-Capita Transfer Income

	(1) Disability Transfer Program	(2) Non-Disability Transfer Programs	(3) Any Transfer Program
	A. Reduced-Form Trend-Break Estimates		
Post-SSI Trend Break, Binding States:	0.34	-0.18	0.16
	[0.06]	[0.15]	[0.15]
95% C.I.	*(0.22, 0.45)*	*(-0.47, 0.11)*	*(-0.15, 0.46)*
1973 DV Mean|APTD<SSI	4.30	16.34	20.30
	B. Instrumental Variables: Effect per SSI Dollar		
Per-Capita Disability Income		-0.55	0.46
		[0.40]	[0.40]
95% C.I.		*(-1.34, 0.24)*	*(-0.33, 1.21)*

Notes: This table presents estimates effects of SSI on per-capita transfer income. Panel A contains estimates of Γ^U from equation (3), and panel B contains IV estimates that use the post-SSI trend break as an instrument for disability program participation. These estimates equal the ratio of the trend break estimates in in columns (2) and (3) to the estimate in column (1). We cluster standard errors by state.

Table 4. IV Estimates of the Effect of Each SSI Recipient on the Number of AFDC Recipients and Cases

	(1)	(3)
	AFDC Recipients	*AFDC Cases*
SSI Disability Recipients	-0.65	-0.30
	[0.41]	[0.27]
95% C.I.	*(-1.47,0.16)*	*(-0.82, 0.23)*
1973 DV Mean|APTD<SSI	*0.021*	*0.023*

Notes: See notes to table 2. The table shows IV estimates for participation rates per adult based on adult AFDC recipients (column 1) and AFDC cases (column 2).

Table 5. SSI Increased the Probability that Adults in AFDC Households Received Disability Benefits

	(1)	(2)	(3)	(4)	(5)
Binding States: APTD Benefit × Post-SSI	2.41	2.29	1.74	1.45	1.28
	[0.68]	[0.84]	[1.02]	[0.93]	[0.75]
95% C.I.	*(1.07,3.74)*	*(0.65,3.92)*	*(-0.25,3.74)*	*(-0.37,3.27)*	*(-0.19, 2.75)*
Non-Binding States: APTD Benefit × Post-SSI	-0.51	-0.27	-0.50	-0.27	-0.29
	[0.28]	[0.43]	[0.30]	[0.40]	[0.57]
95% C.I.	*(-1.06,0.05)*	*(-1.10,0.57)*	*(-1.08,0.08)*	*(-1.06,0.52)*	*(-1.41,0.84)*
Specification	No Covariates	Preferred	Preferred WLS	Region-by-Year FE	1970 Disability-by-Year FE

Notes: The table presents reduced form coefficients that measure changes before and after SSI in the relationship between APTD generosity and the probability that AFDC households contained an adult receiving disability benefits. The first three rows show the results for binding states and the last three rows show the results for non-binding states. These results strongly suggest that many new SSI recipients induced to participate because of benefit increases did indeed come from AFDC since they still have children receiving AFDC benefits.

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IX. DATA APPENDIX

A. Sources

For 1936 and 1937 we collected "Public Assistance, Monthly Statistics for the United States" published by the Social Security Board (Bureau of Public Assistance 1936-1937). From 1938 to 1947 and 1971 to 1980 we collected the "Current Operating Statistics" appendix to the monthly Social Security Bulletin (Social Security Board 1936-1946, Social Security Administration 1947-1980). From June 1948 to December 1970, we collected "Advanced Release of Public Assistance Statistics" published by the Department of Health, Education, and Welfare's Division of Research (Bureau of Public Assistance 1948-1970).

B. Cases versus Recipients

Before 1961, the GA data only record the number of cases, but in 1971 and 1973 they only record the number of recipients. Because recipients sometimes include children and to extend a consistent GA measure back to 1950, we prefer to use GA cases. To fill in missing values for GA cases we predict cases using the observed value of recipients based on an interpolation of number of recipients per case.

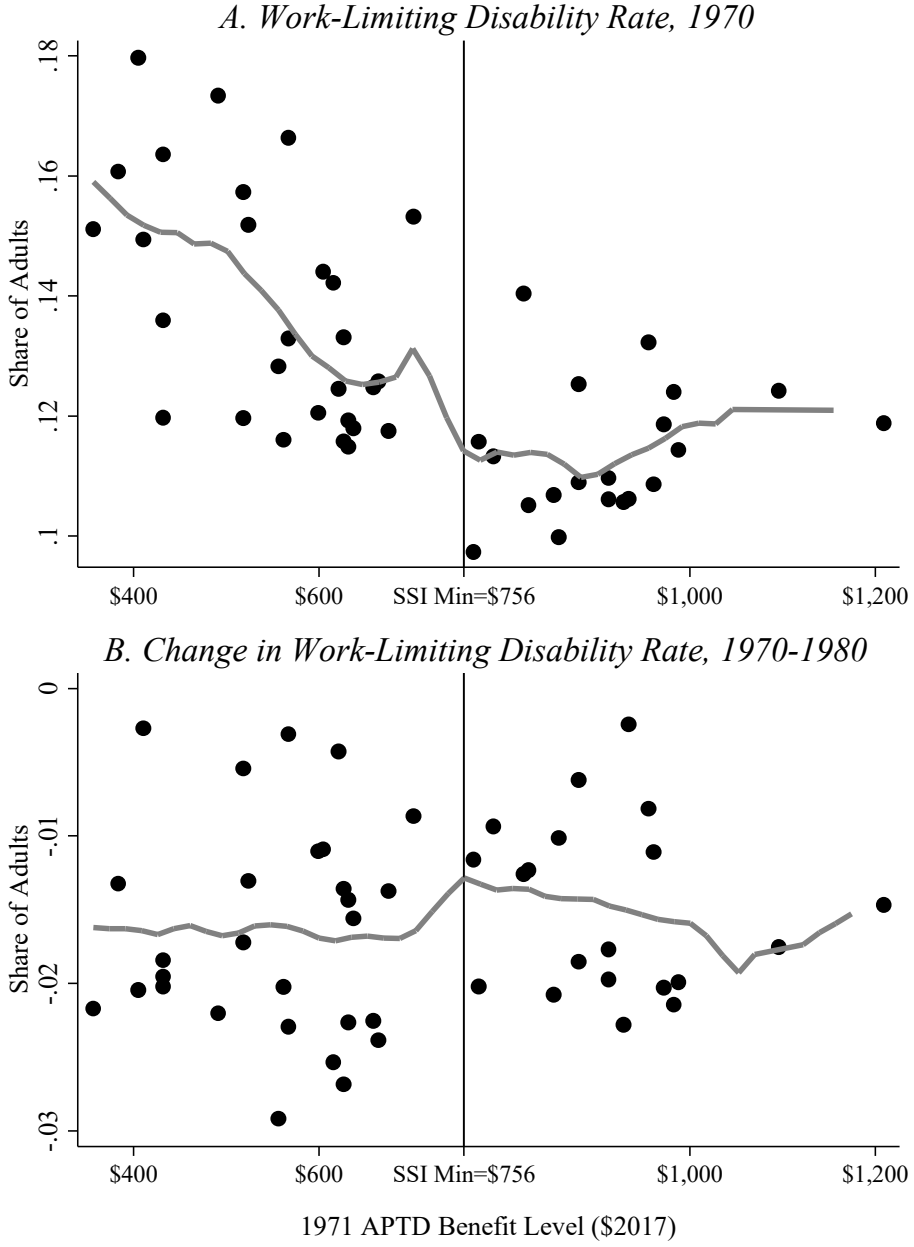
C. Adjusting for Medical Vendor Payments

Starting in October, 1950 states could claim federal reimbursement for medical payments made directly to providers (known as "vendor payments") on behalf of welfare recipients. Medical vendor payments are included in participation and spending data starting in July 1953 and ending either in October 1966 or in the month when a state began its Medicaid program (which replaced MVP). We subtract our separate measure of MVP spending from the combined spending variable to create cash benefit spending. Some states, however, allowed some recipients to get medical payments only. The beginning and the end of MVP reporting, however, provide two pieces of information about the size of this population. We infer the number of medical-only

recipients by calculating the change in caseloads in the first month that medical-only recipients are reported (the earlier of July, 1953 or the date when an MVP program starts) and the last month (the earlier of the month Medicaid began or October, 1966). We linearly interpolate between these two estimates to obtain a guess about the number of medical-only recipients and subtract this from reported participation data. This procedure appears to work well, although we make similar adjustments based on discontinuities in participation (that correspond to spikes in MVP spending) for a handful of states that appear to have let on medical-only recipients sometime after starting to report MVP spending. In some cases (CT, ID, OR, UT, WA), the shift is large and there is a similarly sized shift in some earlier period. We calculate the size of these two shifts, interpolate between the two, and remove that number of cases.

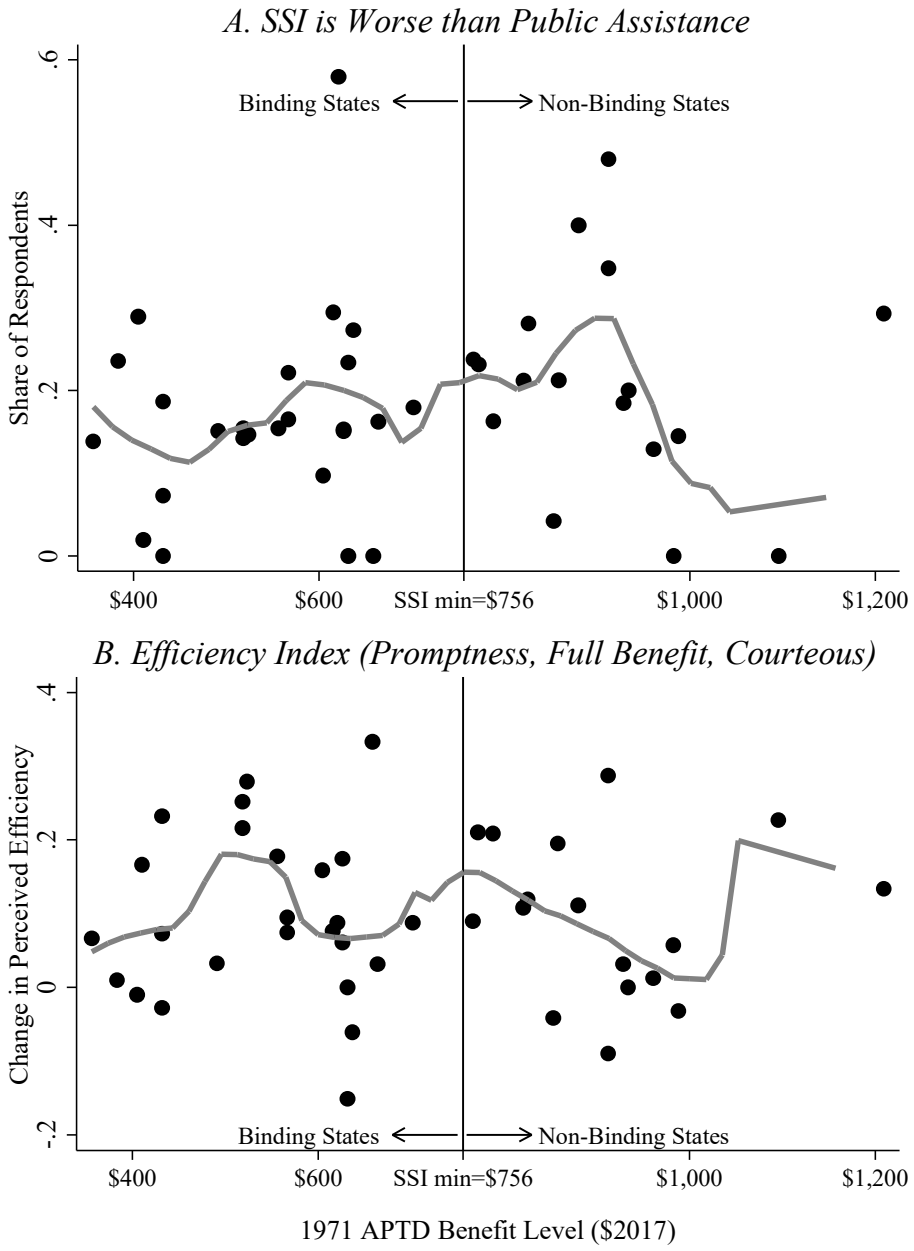
X. RESULTS APPENDIX

Figure A1. Work-Limiting Disability Rates in 1970 and Changes from 1970-1980



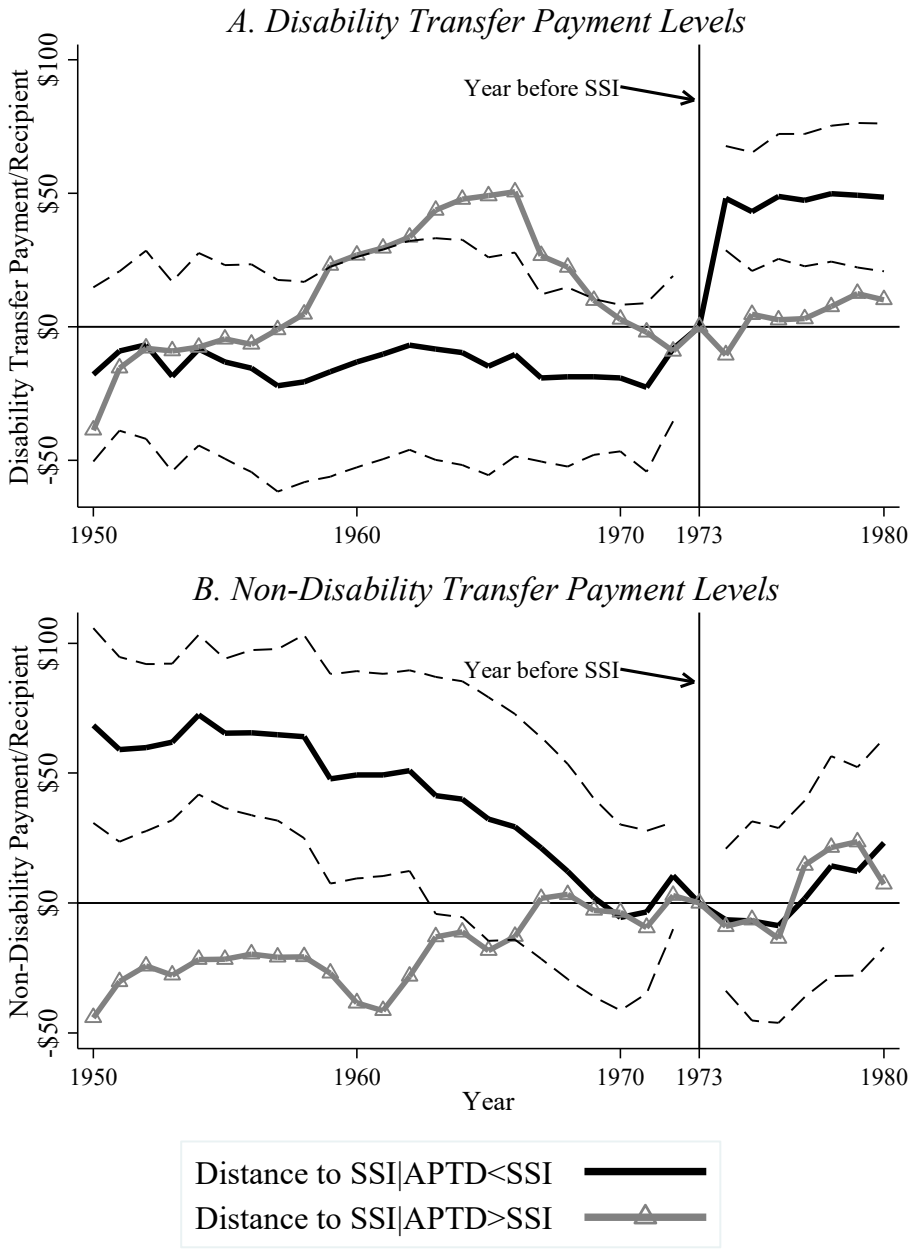
Notes: The figure plots self-reported work-limiting disability rates from the 1970 Census and the change in disability rates from 1970 to 1980 against the 1971 APTD benefit level. In 1970 respondents were asked "Does this person have a health or physical condition which limits the kind or amount of work he can do at a job?" In 1980 respondents were asked "Does this person have a physical, mental, or other health condition which has lasted for 6 or more months and which… Prevents this person from working at a job?" Gray lines are nonparametric regression estimates using an Epanechnikov kernel with bandwidth of 30.

Figure A2. The Effect of SSI's Benefit Floor on Payments per Recipient in Disability and Non-Disability Transfer Programs



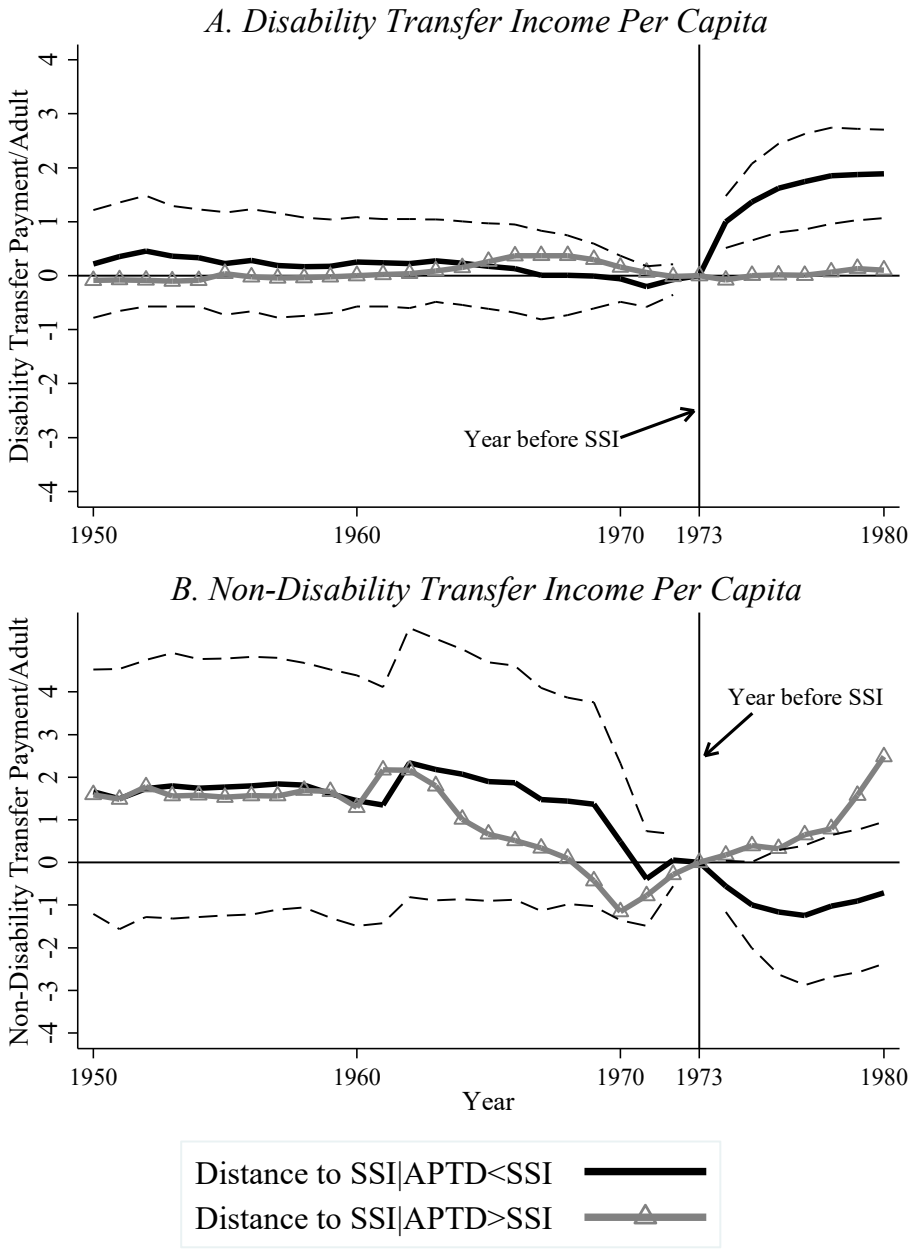
Notes: The figure plots the share of recipients who report that SSI is "worse" than APTD/AB in panel A and the change from 1973 to 1974 in an index of responses to questions about whether benefits are "always" paid promptly, "always" paid in the full amount, or whether caseworkers are "courteous". The figure also includes nonparametric regression estimates using an Epanachnikov kernel with bandwidth of 30.

Figure A3. The Effect of SSI's Benefit Floor on Payments per Recipient in Disability and Non-Disability Transfer Programs



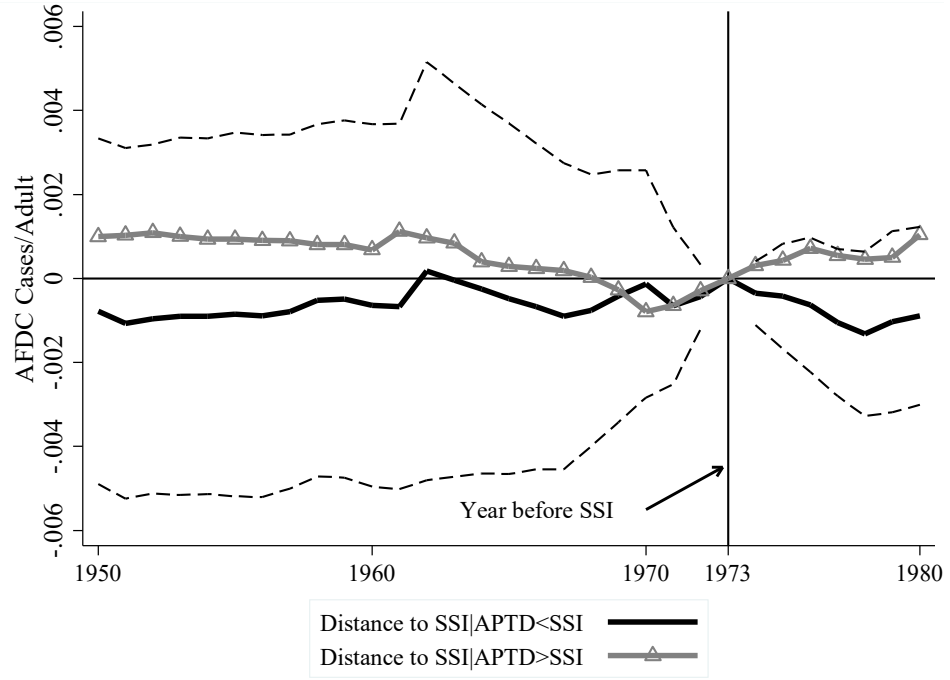
Notes: See notes to figure 8. The outcome is the average payment per recipient in disability and non-disability programs.

Figure A4. The Effect of SSI's Benefit Floor on Payments per Capita in Disability and Non-Disability Transfer Programs



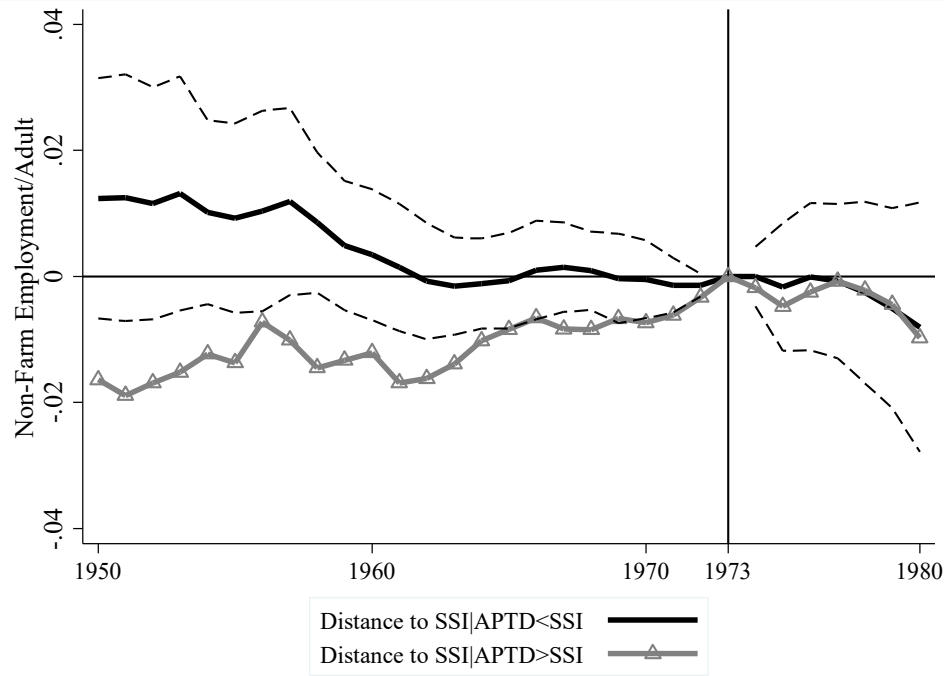
Notes: See notes to figure 8. The outcome is the per-capita payment in disability and non-disability programs.

Figure A5. The Effect of SSI's Benefit Floor on AFDC Cases per Adult



Notes: See notes to figure 8. The outcome is the number of AFDC cases per adult.

Figure A6. The Relationship of SSI's Benefit Floor on Non-Farm Employment Per Adult



Notes: See notes to figure 8. The outcome is the non-farm employment per adult.