Prospects for Social Security Reform

Edited by Olivia S. Mitchell, Robert J. Myers, and Howard Young

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Frontispiece: Special Treasury securities, stored in a federal government filing cabinet in West Virginia, represent $700 billion in Social Security Trust Fund assets. Photo: Jeff Baughan.

Chapter 10
Means Testing Social Security

David Neumark and Elizabeth Powers

Worldwide population aging has brought into sharp focus several controversial features of social security systems. For instance, it is often thought that the "contributions" a worker makes to the social security system are not clearly and positively linked to the benefits ultimately received.[1] Indeed, the complexity of the benefit calculation in the U.S. social security system makes it unlikely that most workers understand the relationship between their next dollar of payroll tax contributed and their ultimate benefit. This may explain why many younger workers express the belief that the present system is unsustainable and they will receive no retirement benefit. When workers believe that most or all of their social security payroll deduction is net tax, this has work disincentives. In turn, such disincentives may hurt economic growth and impose efficiency costs on the economy (Kotlikoff and Sachs 1997; Feldstein 1997). Another potential growth-inhibiting feature of large unfunded social security systems is their pay-as-you-go nature. It is often argued that such a system supplants self-financed retirement saving with intergenerational transfers, thereby reducing the capital stock (Kotlikoff 1979). A further criticism of a pay-as-you-go system is that it has generated excessive redistribution from younger to older cohorts (Kotlikoff 1992; Auerbach et al. 1992).

Despite these criticisms, most experts acknowledge that there is one goal many public pension systems around the world have achieved: they have prevented many elderly households from sliding into poverty. In the United States, the poorest 40 percent of elderly and disabled households receive 80 percent of their income from social security, and social security benefits lift 6 percent of the population above official poverty lines (Aaron 1997). Consequently, there is little support for eliminating public retirement income support systems entirely, at least in the United States and other developed nations. On the other hand, if reforms are required, many believe that cutting benefits for the relatively affluent would be a useful way to achieve system solvency. In this chapter we explore the pros and cons of converting

social security from a universal entitlement program to a program based on individual need in old age: that is, "means-testing" social security benefits.

If social security benefits were fully means tested, then a retiree's benefit would be reduced in accordance with his post-retirement income, as well as his (real and/or financial) asset holdings. Partial means tests might reduce benefits as income rose, but could ignore assets (or vice versa): alternatives might take only some components of income or assets into account when determining benefits. For example, the Concord Coalition advocates a social security "affluence test" that would cut benefits 10 percent for retired households with income between $40,000 and $49,000, and would increase the reduction by 10 percentage points for each additional $10,000 of income, to a maximum of an 85 percent reduction in benefits.[2] This approach is a dramatic departure from the current system, in which benefits depend positively on past income, while (aside from the "retirement test," discussed below) they are taxed lightly, if at all. A means-tested program might also act as a "safety net" or "bottom tier" of a multi-tier retirement system, for example, in conjunction with a defined contribution public system (Mitchell and Zeldes 1996).[3]

Examining the Current System

It is natural to ask whether the present social security system already incorporates any means-test-like features, since expansion of such features could afford a natural way to create a broader means-tested system. Before 1990, social security benefits in the United States were reduced by $1 for every $2 of earned income above an exempt amount. Recently, benefits have been reduced at a more generous rate of $1 for every $3 of earned income.[4] A penalty on labor income, or "retirement test," is clearly necessary for a system intended as a retirement income support, whether it is means-tested or not. That the retirement test is not intended as a means test is evident from the fact that no other types of income directly reduce social security benefits. Since 1983, some retirees' benefits have been subject to the federal income tax; the proceeds are returned to the Old Age Survivors and Disability Insurance (OASDI) trust fund. But fewer than a quarter of all beneficiaries pay this tax, and only the most affluent retirees "return" a significant share of their benefits. Only at household income categories exceeding $50,000 does the tax burden exceed 10 percent of benefits received.[5] Further, asset holdings do not affect benefits at all, except to the extent that asset income or realized capital gains might ultimately contribute towards the application of the federal tax to social security benefits.

For these reasons, we believe it is safe to say that a social security program in which benefits were targeted to needy recipients through a means test would look quite different from today's system. The simplest imaginable system would be similar to the Supplemental Security Income (SSI) pro-

gram that has two separate tests, one for income and the other for assets (we discuss this program at length below). If income exceeds a given amount, no benefits are received. If income is below the limit but positive, benefits are reduced (perhaps dollar-for-dollar). If assets exceed a fixed limit, no benefits are received (alternatively, a benefit reduction rate might be applied to the excess).[6] This simple scheme could be made more complex by applying different limits or benefit reduction rates to different types of earnings (for instance, treating earned income more harshly to preserve something like a "retirement test"); or by exempting some assets (like an owner-occupied home).

A more sophisticated scheme would unify the treatment of income and assets. By applying a formula to annuitize wealth stocks (i.e., to convert a stock of wealth into an equivalent income flow), benefit reduction formulas could be applied to wealth as they are to income.[7] If society expects recipients to expend all resources prior to their death, then the entire stock of real and financial wealth might be subject to the annuitizing formula. Again, however, one could apply different weights to different types of income and annuitized wealth, or possibly exclude some assets from the annuitization formula.

The primary debate in the United States has been over whether moderate changes that preserve the existing social security system will do, or whether a privatized or partially privatized system would be superior. The argument over whether the gains from a privatized system are real or illusory is fairly complex (Mitchell and Zeldes 1996). The relative merits of means testing, on the other hand, appear straightforward. Most obviously, means testing, by restricting who can receive benefits, or by offering only small benefits to all but the "neediest" elderly, could dramatically shrink the size of the social security system, and hence enable the payroll tax to be greatly reduced. It is the distortionary impact of this tax (an inefficiency that increases with the square of the tax) that is viewed as the most troublesome component of the current system by many (Kotlikoff and Sachs 1997; Feldstein 1997). If it caused the social security system to shrink dramatically, means testing would greatly reduce the redistribution from younger to older generations that has been the subject of so much recent criticism. In particular, even under a pay-as-you-go system, means testing would halt redistribution from current workers with moderate lifetime incomes to the currently-affluent elderly. Finally, benefits would be determined by actual post-retirement resources and not by one's earnings history, so that benefits intended to help low-income households would not be paid to well-off retirees.

One drawback of means testing is its potential negative impact on saving. That is, when social security benefits depend in some way on one's level of financial assets (or the income these assets produce during retirement), some people might choose not to save, or to save very little. This view has been extremely influential in the current debate and appears to have pre-

cluded serious discussion of a means-tested reform option. For example, in an appendix to the report of the Bipartisan Commission on Entitlement and Tax Reform (1995), several experts endorsed the statement that "Means testing would tax . . . saving by cutting Social Security as income from private saving increases, conveying the message: Don't save or we will punish you for your frugality by denying you Social Security" (112). Similarly, the Technical Panel (1997) reported to the recent Advisory Council on Social Security that it "opposed means-testing social security benefits on the basis of other retirement income or accumulated wealth. To avoid loss of social security benefits, some workers might reduce their own retirement saving or persuade employers to shift compensation from pension contributions to earnings. Either response would lower savings and private retirement incomes" (6).

Equally damaging to the means testing case is the fact that new work disincentives may be created by a means test. Should social security benefits in a means-tested program be reduced or restricted on the basis of asset holdings at retirement, this would discourage the additional work needed to finance retirement savings in "ordinary" (non-tax-favored) savings, or in defined contribution private pension schemes. Income testing of benefits may also discourage work, per se. If social security benefits are reduced or eliminated when post-retirement income increases, then defined benefit pension payments or annuitized income from defined contribution plans would reduce public benefits. Depending on the exact rules of a means-tested program, workers might have an incentive to retire early. This is because additional private pension benefits might be partially or completely offset by reduced public benefits, and could even disqualify retirees from receiving any public benefits.

These possible negative effects of a means-tested social security policy may or may not outweigh its benefits. Despite the fact that means testing has been so resolutely rejected in the current debate, there is actually little direct evidence on the possible magnitude of the potential adverse effects. In the next section, we briefly discuss the current scant evidence on the likely impact of means testing public retirement income programs, and then present new evidence on the potential labor supply distortions from a means test, based on evidence from the United States.

Previous Evidence on Means Testing

An extensive literature seeks to estimate the effects of changing various parameters of the social security system on saving and labor supply behavior, relying on variation in program parameters across individuals to identify the effects of interest (e.g., Gustman and Steinmeier 1984, 1986, 1991; Burtless and Moffitt 1985). This literature may be fruitfully applied to assessing

changes to the existing system that preserve its basic structure, but it is less relevant to the means testing debate.[8]

One of the few rigorous analyses of the means-testing option appears in Feldstein (1987), who argued that "a means-tested program with benefits set at the optimal level may induce some utility-maximizing workers to save nothing. Although their resulting consumption in retirement would then be less than they would have chosen without a social security program, the utility value of the extra consumption during working years more than offsets the reduced consumption during retirement" (470). An overlapping generations model is employed to investigate the circumstances under which society would be better off under a universal pay-as-you-go system than a targeted (means-tested) pay-as-you-go system. In this model, workers receive earnings when young, and no income when old, but may save from first period income if they wish. Feldstein considers a particularly simple means test, where only zero-savers can collect benefits in old age. Not surprisingly, he finds that the relative advantages of a means-tested system depend critically on the responsiveness of private saving to the disincentive created by the means test. He concludes that an income test is the right "means" to use, but this finding relies on the assumption that income is exogenous. If it were not, one would expect efficiency losses to result from an income test as well.

Evidence from other countries is also germane. For instance, Australia's social security system has been means tested since its inception in 1909, although the design of the means test has varied greatly over the years.[9] The system's initial design was simple, with separately applied income and asset tests. In the 1960s, the asset and income means tests were "merged." A 10 percent rate of return was imputed on assets over a disregard level, and this imputed return was added to other income and affected benefits through the income test. Controversy over the treatment of assets culminated in the asset test being dropped altogether in 1976; by 1985, however, an asset test was reintroduced in an effort to rein in program costs.[10] Under the new rules, the participation rate dropped to 65 percent of people of pensionable age, from 74 percent in 1983. Currently the Australian old-age benefit reflects an average wage replacement rate of only around 20 percent. Therefore, while many elderly families in Australia receive some benefit despite the means tests (the all-time high was 78 percent in 1978), the "full" benefit is low, and many receive even lower benefits due to the means test.

The notion that asset tests could discourage saving had been a concern of Australian policy makers since the system's inception. For example, the asset test was already liberalized to exempt the home in 1912, because of the view that "taking it into account penalized thrift and discouraged home ownership" (Schultz et al. 1991: 224). However, the controversy continued. "During the postwar years . . . the main argument voiced against means

testing related to the impact [on] . . . the propensity of individuals to save. Means testing was seen as a major disincentive to saving. It was argued that there was little point in saving for retirement if, upon reaching the age-pension eligibility age, the retiree was denied a pension because of the means test" (225). The income test has also been frequently changed. Over the years gifts from children, asset income, and capital gains income have all been the subject of policy changes.

Considering this lengthy and varied experience with means testing, the lack of econometric evidence on the effect of means testing on Australians' work and retirement saving patterns is somewhat surprising. One explanation may be that the system's complex and inconsistent treatment of various types of income and assets is perceived to create a greater problem with "tax avoidance" than with changes in real activity like work and consumption. For example, when assets were not tested prior to 1986, retirees apparently shifted wealth into no- or low-interest accounts to escape the income test.[11] Loopholes probably affected the post-retirement composition of assets or income more than the real pre-retirement behavior that generated the resources in the first place.

An Alternative Approach to Studying Means Testing

In the remainder of this chapter we attempt to draw inferences about the potential behavioral responses to means testing social security by examining the effects of the United States Supplemental Security Income (SSI) program for the aged. Here we offer evidence on the effects of an income- and asset-tested retirement program on the pre-retirement labor supply of potential recipients. In our analysis, we exploit the state-level variation in SSI benefits to estimate the effects of SSI on labor supply. We use data on male-headed households from waves 4 and 5 of the Survey of Income Program Participation (SIPP), covering individuals in the 1983–1986 period, to estimate the effects of state SSI supplements via a difference-in-difference approach that controls for variation in labor supply behavior across states and across different types of individuals. In related work (Neumark and Powers, forthcoming) we examine the effect of variation in SSI benefits on saving of those approaching the age of eligibility for benefits, finding that higher benefits reduce pre-retirement saving among likely program participants.[12]

In our view, the SSI program for the aged has many parallels to the type of means tested social security program that might emerge from social security reform process. Part of the SSI program provides payments to the poor elderly (aged 65 and over).[13] The federal government sets eligibility criteria and benefit levels for the federal component of the program. The federal government also specifies maximum benefit levels for couples and individuals, which are reduced by income from other sources, including social security retirement and disability benefits. (The first $20 per month of non-

means-tested transfer income, the first $65 of earned income, plus one-half of remaining earnings, are disregarded in reducing SSI benefits.) Thus, other sources of retirement income influence both eligibility for SSI and the size of potential benefits. Financial resources also affect eligibility. For example, as of 1985 (corresponding roughly to the time period covered by our data) individuals with over $1,600 in countable assets, and couples with over $2400 in countable assets, were ineligible.[14] In 1984 there were 1.55 million persons receiving SSI payments who were eligible because of age (Committee on Ways and Means 1995).[15]

Though the federal government sets eligibility criteria and benefit levels, states may also supplement federal SSI benefits. For example, in January 1985 the maximum federal monthly benefit was $325 for an individual, and $488 for a couple. The highest state benefit was in California, with a maximum combined monthly benefit of $504 for an individual and $936 for a couple.[16] In December 1985 the average federal monthly benefit paid was $146 for individuals and $232 for couples, and the average state supplements were $97 and $257, respectively, with 39 percent of SSI recipients receiving state supplements (Kahn 1987).

Empirical Analysis

To derive empirical estimates, we use a sample of male heads of households drawn from the SIPP, which when weighted serves as a nationally-representative sample of households.[17] Households are interviewed every four months (each four-month interval is referred to as a "wave") for two to three years. Most questions are asked retrospectively about the previous four months. This paper uses the first (1984) panel of the SIPP, covering the period from October 1983 through July 1986.

Several measures of labor force activity are used as dependent variables for the analysis. A binary employment variable is used that equals one if the individual reports positive hours of work in the first month of wave 4. Actual hours of work in that month are also studied. We also examine effects on years of covered social security employment (a variable collected in a special wave 5 topical module), which provides a longer-term perspective. Finally, because family labor supply and the wages earned by each family member may influence post-retirement income and wealth, we also examine some results regarding the possible impact of the SSI program on monthly family earned income in wave 4; this also gives us a convenient measure of aggregate total family labor supply.

Descriptive statistics for the sample of men aged 40–64 for whom we estimate labor supply effects appear in Table 1. Of this group, 83 percent of the group is employed in wave 4, and weekly hours of work average 37. Average years of covered social security employment are just under 23 years for this group, and average monthly family earnings are $2,628. Each house-

TABLE 1. Descriptive Statistics and Determinants of SSI Participation

	Descriptive Statistics, Male Household Heads Aged 40–64	*Probit for SSI Participation, Male Household Heads Aged 65+*
Employed, wave 4	.83	–
Hours, wave 4	37.03 (19.45)	–
Years of covered Social Security employment, wave 5	22.88 (13.80)	–
Family earnings, wave 4	2628.39 (2107.47)	–
Maximum state SSI supplement >20% of federal benefit	.20	.022** (.007)
Maximum SSI benefit, individual	365.49 (58.43)	–
Maximum SSI benefit, couple	566.00 (137.83)	–
Currently authorized for food stamps	.04	.019 (.013)
Years authorized for food stamps	.06 (.65)	.015** (.003)
Less than high school	.26	.05** (.01)
Some college	.17	−.01 (.02)
College graduate	.24	−.004 (.02)
Black	.08	.024** (.009)
Never married	.03	.04** (.01)
Divorced/widowed/ separated/spouse absent	.11	.007 (.007)
N	4560	1787

Source: Authors' calculations, 1984 SIPP. Column 1 reports means, with standard deviations in parentheses. Maximum SSI benefit is combined federal and state, obtained from the 1985 Green Book (Committee on Ways and Means 1985), and is based on current marital status. Classification of states providing supplements higher than 20 percent of the federal benefit is based on whether the supplement for either an individual or a couple exceeds this amount. Column 2 reports partial derivatives of the participation probability, with standard errors based on probit coefficients in parentheses; SSI benefits are measured in hundreds of dollars. High school graduates are the reference category. ** indicates statistically significantly different from zero at the 5-percent level, and * at the 10-percent level. All estimates are weighted.

hold is assigned a maximum state SSI benefit based on household composition (whether the household is comprised of an individual or a couple) and state of residence. For the empirical analysis, we create a variable that equals one for observations in states in which the benefit exceeds 20 percent of the federal maximum (for either individuals or couples), and zero for states that do not supplement the federal benefit; we exclude observations from the intermediate states.[18] In addition to the high benefit states, states such as Hawaii, Maine, Oregon, and Utah paid very low supplements. Twenty percent of our sample resides in states with state SSI supplements exceeding 20 percent of the federal benefit.[19] Average maximum combined (federal and state) benefits are $365 for individuals, and $566 for adults.

Demographic variables in the analysis include race (black or non-black), marital status (married spouse present, never married, and ever married), and education (less than high school, high school graduate, some college, and college graduate). As explained below, we also require estimates of the probability of participation in SSI at or after age 65. We obtain these estimates by studying the determinants of SSI participation among those aged 65 or over, based on participation of the male at any time during wave 4.

We are interested in estimating the effects of the potential receipt of SSI benefits on various labor supply measures, denoted Y. Two factors influence the potential value of SSI benefits: the level of the benefits, and the likelihood of receiving them. Thus, for example, we might expect a person with characteristics associated with low permanent income (such as low education), in a state with high SSI benefits, to experience the greatest labor supply disincentives. In contrast, a white, married college graduate is extremely unlikely to be eligible for SSI, whether he resides in a state with high or low benefits. Thus, to estimate the effects of SSI, we focus on variation in labor supply behavior associated with high SSI benefits for those with a relatively high likelihood of eligibility.

We therefore begin by identifying exogenous characteristics associated with likely future SSI receipt. By studying workers over age 65, we can identify characteristics associated with a high likelihood of SSI participation. We then distinguish among workers under age 65 based on these characteristics, defining a dummy variable "Part" to equal one for likely participants (based on a chosen threshold for the estimated probability of participating upon reaching age 65), and zero otherwise.[20]

Perhaps the simplest test for effects of SSI on labor supply would be to estimate a regression equation of the form:

$$(1) \qquad Y = \alpha \cdot \text{Part} \cdot \text{Age4049} + \beta \cdot \text{Part} \cdot \text{Age5059} + \gamma \cdot \text{Part} \cdot \text{Age6064} + \delta + \eta \cdot \text{Age5059} + \theta \cdot \text{Age6064} + X\psi + \epsilon,$$

where the age variables are dummy variables for the indicated ranges, and the sample includes individuals aged 40–64. The matrix X includes demo-

graphic controls. This can be thought of as a "federal experiment"; state benefit levels are not used to introduce variation among likely participants in incentives to go on SSI. For reasons discussed below, we do not rely on this test, but it is useful in understanding the empirical procedure we use.

The estimates of α, β, and γ indicate differences in labor supply between individuals likely to be eligible for SSI, and those unlikely to be eligible. For example, γ measures the behavioral difference between 60–64-year-olds likely to be eligible and those unlikely to be eligible ($Y|_{Part=1,Age6064=1} - Y|_{Part=0,Age6064=1}$), and $(\gamma - \alpha)$ measures the difference in the change in Y from ages 40–49 to ages 60–64 between likely participants and non-participants ($[Y|_{Part=1,Age6064=1} - Y|_{Part=1,Age4049=1}] - [Y|_{Part=0,Age6064=1} - Y|_{Part=0,Age4049=1}]$).[21] If SSI reduces the labor supply of likely participants, we should find at least one of two effects: $\gamma < 0$, because likely participants are predicted to work less; or $(\gamma - \alpha) < 0$, because the work disincentive on likely participants becomes more evident as they approach the age of eligibility for SSI.

One might also expect, a priori, that estimates of γ and $(\gamma - \alpha)$ — rather than β and $(\beta - \alpha)$ — are of greatest interest and are more likely to reflect disincentive effects because, for a number of reasons, the influence of SSI should be strongest for workers nearing the age of eligibility. First, given stochastic influences on earnings and wealth, older workers can form better predictions of post-retirement income. Second, workers may pay more attention to the potential receipt of SSI benefits as they approach the age of eligibility (paralleling findings in Mitchell 1988). We therefore focus on estimated effects for individuals aged 60–64, but also report effects for those aged 50–59.

There are competing reasons to prefer using estimates of γ and of $(\gamma - \alpha)$ (and similarly for β) to draw inferences about the effects of SSI. One reason to prefer estimates of γ is because $(\gamma - \alpha)$ is identified from differences in behavior across cohorts, which requires the assumption of constant behavior across cohorts, or at least that cross-cohort differences are the same across states; in contrast, identification of γ does not require such assumptions. On the other hand, looking at $(\gamma - \alpha)$ effectively uses workers aged 40–49 in the state as a control group, because it identifies the effects of SSI from changes in the behavior of 60–64-year-olds relative to this group. There may be state-specific differences in behavior of likely participants in a state regardless of their age, stemming from economic conditions, tax policy, or other income-support programs. As a consequence, changes in behavior with age, rather than levels, may provide more reliable information. We report both types of estimates, but focus more on the changes with age captured in $(\beta - \alpha)$ and $(\gamma - \alpha)$.

Of course, while estimates of α, β, and γ, in equation 1 could represent the effects of the SSI program, the estimates are based on a rather tenuous identifying assumption — namely, that in the absence of the program the

labor supply behavior of men (or families) with characteristics associated with SSI participation would be the same as those of other men. That is, the group for which Part = 0 (for example, highly educated men) serves as the "control group" for the estimation of the effects of SSI. It is well known, however, that age profiles of labor supply differ systematically with variables such as education, marital status, and race. This may arise because health is correlated with these characteristics, or because they affect optimal retirement decisions. In addition, if likely participants are most dependent on current income for consumption, a comparison with unlikely participants would bias the results against finding negative effects of SSI benefits on labor supply. Because the variables that determine Part are systematically related to labor supply for reasons that may have nothing to do with SSI, estimation of equation 1 will lead to biased estimates of the effects of SSI.

The empirical strategy we follow instead, therefore, is to exploit state-level variation in the provision of SSI benefits. Individuals in states *without* state supplementation of SSI benefits, but *with* characteristics associated with SSI receipt, serve as a much more compelling control group with which to compare the behavior of individuals in states *with* state supplementation and *with* characteristics associated with SSI receipt. At the same time, state-specific labor supply profiles may also differ in ways that are correlated with SSI benefits. Therefore, we want to identify the effects of SSI from *relative differences* between the behavior of likely participants and unlikely participants in supplement and non-supplement states, effectively using the unlikely participants to control for these state differences. For example, older high school dropouts (who are much more likely to be SSI participants) may work less, or decrease their labor supply more as they age, compared with more-educated workers. But the difference-in-difference framework only infers an effect of SSI if the difference between the labor supply of older high school dropouts and older more-educated workers is larger in states that supplement SSI than in states that do not.

An intuitive way to think about the approach is to imagine dividing the sample into states that generously supplement SSI, and states that do not supplement SSI. We then estimate equation 1 only for the subset of states that do not supplement SSI, and a similar equation

$$\text{(2)} \qquad Y = \alpha' \cdot \text{Part} \cdot \text{Age4049} + \beta' \cdot \text{Part} \cdot \text{Age5059} + \gamma' \cdot \text{Part} \cdot \text{Age6064} + \delta' + \eta' \cdot \text{Age5059} + \theta' \cdot \text{Age6064} + X\psi + \epsilon'$$

for the states that supplement SSI. Estimates for these two subsamples provide the relevant difference-in-difference estimates. For example, focusing on 60–64-year-olds, γ' and γ each measure the difference in behavior between likely and unlikely participants within a particular type of state (i.e., high and low supplement states). Here γ serves as the "baseline" difference in behavior of those with characteristics associated with participation in SSI,

and therefore $(\gamma' - \gamma)$ captures the effect of SSI supplements on the labor supply of likely participants. Similarly, $(\gamma' - \alpha') - (\gamma - \alpha)$ captures the effect of SSI on changes in labor supply with age, based on the difference in behavior between likely and unlikely participants in high supplement states versus low supplement states.

Note that, as discussed above, this framework allows the age profile of labor supply for a whole state population (captured in δ, η, and θ, or δ', η', and θ') to differ for states that do and do not supplement SSI. This is potentially important to control for biases that might arise from a relationship between policy and other sources of variation in the level or age profile of Y in a state (e.g., if states in which older individuals have a higher propensity to work also tend to offer more generous SSI supplements). Rather than estimate equations 1 and 2 using separate state samples, we obtain difference-in-difference estimates from an interactive specification estimated for the pooled sample of states:

$$
\begin{aligned}
(3)\quad Y = {} & \alpha\cdot\text{Part}\cdot\text{Age4049} + \beta\cdot\text{Part}\cdot\text{Age5059} + \gamma\cdot\text{Part}\cdot\text{Age6064} + \delta + \eta\cdot\text{Age5059} \\
& + \theta\cdot\text{Age6064} + \alpha'\cdot\text{Part}\cdot\text{Age4049}\cdot\text{Supp} + \beta'\cdot\text{Part}\cdot\text{Age5059}\cdot\text{Supp} \\
& + \gamma'\cdot\text{Part}\cdot\text{Age6064}\cdot\text{Supp} + \delta'\cdot\text{Supp} + \eta'\cdot\text{Age5059}\cdot\text{Supp} \\
& + \theta'\cdot\text{Age6064}\cdot\text{Supp} + X\psi + \epsilon
\end{aligned}
$$

where "Supp" is a dummy variable for states with generous SSI supplements. In this specification the estimate of γ' is the difference-in-difference estimate of the effect of SSI on Y for those aged 60–64, and the estimate of $(\gamma' - \alpha')$ measures the effect on the change in Y from ages 40–49 to ages 60–64.[22,23] The pooled regression lets us easily assess the statistical significance of our difference-in-difference estimates, since the estimates are obtained from a single estimation.

Results for SSI Participation

The probit estimates for SSI participation among men aged 65 or older appear in column 2 of Table 1. One would expect that more generous SSI benefits also increase the likelihood of participation; this is confirmed in the table, as the estimated coefficient on the generous state SSI supplement variable is positive (.022) and strongly significant. Education, race, and marital history variables are also strongly associated with SSI participation. High school dropouts are significantly more likely than high school graduates to be SSI participants, with a differential of 5 percentage points. Black and never-married men are also considerably more likely to be on SSI. Finally, to capture unobservables related to low permanent income, and possibly also unobserved heterogeneity in the propensity to participate in income-support programs, information on food stamp enrollment is in-

cluded in the probit; the number of years authorized, in particular, is strongly positively associated with SSI participation.[24,25]

We use these probit estimates to generate predicted probabilities of participation in SSI for those aged 40–64, which are used in turn are used to construct the variable Part that is used in labor supply models like equation 3. For most of the specifications we estimate, we define Part to equal one when the predicted probability is at or above the 90th centile of the distribution of the predicted probabilities, and zero otherwise. Based on the proportion of participants among those aged 65 and over, approximately one-half of those above this centile should end up on SSI. Selecting which threshold to use presents a trade-off. On the one hand, a high threshold isolates the effects of SSI on those who are most likely to participate, for whom labor supply disincentives may be stronger. On the other hand, those with higher probabilities may need to work more to finance current consumption, since they tend to be poor, or may initially work sufficiently little that disincentive effects are difficult to detect. We therefore also present evidence based on higher and lower thresholds.

Results for Labor Supply

Before proceeding to investigate the impact of SSI on labor supply, we present estimates of the age profiles for the alternative dependent variables measuring labor supply for our sample of men aged 40–64 in Panel A of Table 2. The equations in each column also include controls for education, race, and marital status. For each of the four dependent variables—employment, hours, years of covered social security employment, and family earnings—there are substantial declines with age, particularly for those aged 60–64. The decline in years of covered social security employment with age must reflect cohort effects, because it is a cumulative measure of labor supply.

In Panel B of Table 2, we proceed to the difference-in-difference estimates of equation 3 to estimate the effects of SSI on labor supply.[26] The first two rows contain estimates of β' and γ' for the various dependent variables. Recall that these coefficients measure the differences—for individuals at ages 50–59 and 60–64 respectively—between the behavior of likely participants and unlikely participants in states with generous SSI supplements, using the difference in behavior between likely and unlikely participants in non-supplementing states as a "control." In all four columns the estimate of γ' is negative, consistent with the hypothesis that the pre-retirement labor supply of older likely participants in generous states is relatively lower. However, none of these estimates are significantly different from zero. There is even less evidence of negative labor supply effects from the estimates of β', which are positive and significant in the 5- or 10-percent level for the three direct labor supply measures.

TABLE 2. Estimates of Age Profiles and Difference-in-Difference Estimates of Effects of SSI on Labor Supply, 90th Centile of Participation Probabilities Used to Define Likely SSI Participants, Male Household Heads Aged 40–64

Estimator	*Employed Probit*	*Hours Tobit*	*Years of Covered Soc. Sec. Employment*	*Family Earnings Tobit*
A. Age profiles of labor supply measures				
Age 50–59	−.10**	−5.90**	2.76**	−152.69**
	(.01)	(.70)	(.54)	(69.91)
Age 60–64	−.27**	−21.12**	−6.25**	−1526.84**
	(.01)	(.91)	(.70)	(90.73)
B. Difference-in-difference estimates of Effects of SSI				
Effect of SSI on 50–59 year-olds (β')	.093*	12.14**	6.70**	78.24
	(.055)	(4.04)	(3.08)	(390.48)
Effect of SSI on 60–64 year-olds (γ')	−.062	−5.38	−3.03	−36.22
	(.071)	(5.92)	(4.55)	(567.95)
Effect of SSI on 50–59 year-olds relative to 40–49 year olds ($\beta' - \alpha'$)	−.074	.21	3.59	−1067.38*
	(.083)	(5.69)	(4.32)	(551.44)
Effect of SSI on 60–64 year-olds relative to 40–49 year olds ($\gamma' - \alpha'$)	−.229**	−17.31**	−6.14	−1181.84*
	(.095)	(7.17)	(5.48)	(689.67)
Log likelihood	−1122.9	−11554.1	−10659.4	−23432.0

Source: Authors' calculations, 1984 SIPP. There are 4560 observations in Panel A, and 2940 observations in Panel B. See Table 1 for details. Specifications in Panels A and B also include dummy variables for education, race, and marital status. Specifications in Panel B correspond to equation (3) in the text. The specifications also include dummy variables for age (50–59 and 60–64), a dummy variable for states with maximum benefits for either individuals or couples exceeding 20% of the federal benefit, interactions between the "high supplement" variable and age dummy variables (for ages 50–59 and 60—64), and interactions between the age dummies for all ranges (40–49, 50–59, and 60–64) and a dummy variable for "likely participants." The coefficients reported in the table are from the variables interacting age, "high supplement," and "likely participant." The "likely participant" dummy variable is define to equal one for observations for which the predicted probability of participation in SSI, using the estimates from column 2 of Table 1, exceeds .0799, which is the 90th centile of the weighted distribution of estimated probabilities. Observations from states that supplement SSI, but with supplements below 20%, are excluded. ** indicates statistically significantly different from zero at the five-percent level, and * at the 10-percent level. All estimates are weighted.

We also report estimates of $(\beta' - \alpha')$ and $(\gamma' - \alpha')$, which describe the changes relative to individuals aged 40–49. In this case, there is considerably stronger evidence of labor supply disincentive effects of SSI. In particular, the estimated drop-offs from ages 40–49 to 60–64 are statistically significant at the 5-percent level for employment and hours, and at the 10-percent level for earnings. Furthermore, looking at the drop-offs from ages 40–49 to 50–59, two of the four estimates are negative (one significant at the 10-percent level), and there are no longer significant positive effects. This evidence, and the differences compared with the estimates of β' and γ', suggest that state-specific differences in labor supply mask life-cycle changes induced by SSI.[27]

There is a potentially important source of misspecification in the Panel B estimates. This arises because states with high SSI supplements also offer relatively generous benefits in other transfer programs, including, of course, SSI for the disabled. These programs may be available not only to those eligible for SSI for the aged, but also to younger men, in particular the disabled. As a consequence, we may see labor supply effects among those below age 65 that are a response to programs other than SSI for the aged. To better isolate the incentive effects of SSI for the aged, and in particular to eliminate spurious relationships that may arise through relationships between disability and income-support programs available to the younger disabled, we re-estimate the equations from Panel B of Table 2 excluding from the sample individuals with any self-reported work-impairing disability.

Estimates for this subsample appear in Panel A of Table 3. The results for ages 60–64 are now more fully consistent with the labor supply disincentive effects of SSI. As before, the estimates of $(\gamma' - \alpha')$ are negative, and estimates for employment and hours are both significant at the five-percent level. For this sample, though, the estimates of γ' are also negative, and significant at the 5- or 10-percent level for employment and hours. In contrast, the estimates of β' are no longer positive and statistically significant. Thus, once we exclude individuals whose behavior is most likely to be influenced by other programs whose generosity is potentially correlated with supplemental state benefits for SSI for the aged, state-specific differences in labor supply measures at all ages are less important, and the estimated effects of labor supply are more similar whether or not we use the 40–49-year-olds as a control group.[28]

Next, we consider problems that arise in measuring features of state SSI programs. In particular, as mentioned earlier, some states choose to administer their own programs, in which case they can set their own eligibility criteria. Therefore we further restrict the sample to include only the subset of states with federal administration of SSI, to ensure that we are looking at states with federal administration and therefore identical benefit formulas and asset rules (see Appendix Table A1). This is potentially important because in states with policy features that differ from the federal ones, one can

TABLE 3. Difference-in-Difference Estimates of Effects of SSI on Labor Supply, Excluding Individuals with Work-Impairing Disability, and States with State-Administered SSI Programs, Male Household Heads Aged 40–64

	Employed	*Hours*	*Years of Covered Soc. Sec. Employment*	*Family Earnings*
A. Excluding individuals with work-impairing disability				
Estimator	Probit	Tobit	Tobit	Tobit
Effect of SSI on 50–59 year-olds (β')	.013 (.05)	6.10 (3.93)	2.31 (3.00)	−312.87 (436.36)
Effect of SSI on 60–64 year-olds (γ')	−.118* (.07)	−12.96* (6.24)	−4.94 (4.79)	−107.44 (693.68)
Effect of SSI on 50–59 year-olds relative to 40–49 year olds ($\beta' - \alpha'$)	−0.63 (.08)	1.17 (5.49)	4.52 (4.18)	−1131.49* (611.21)
Effect of SSI on 60–64 year-olds relative to 40–49 year olds ($\gamma' - \alpha'$)	−.194** (.09)	−17.88** (7.33)	−2.73 (5.61)	−926.06 (815.46)
B. Also excluding states with state-administered SSI programs				
Effect of SSI on 50–59 year-olds (β')	.031 (.06)	8.84 (5.06)	6.71 (4.04)	−464.10 (553.54)
Effect of SSI on 60–64 year-olds (γ')	−.132* (.08)	−12.13 (7.49)	−3.23 (6.01)	−4.84 (820.36)
Effect of SSI on 50–59 year-olds relative to 40–49 year olds ($\beta' - \alpha'$)	−.073 (.10)	3.15 (7.43)	5.24 (5.93)	−1581.19* (822.08)
Effect of SSI on 60–64 year-olds relative to 40–49 year olds ($\gamma' - \alpha'$)	−.236** (.12)	−17.82* (9.24)	−4.70 (7.40)	−1121.93 (1019.90)

Source: Authors' calculations, 1984 SIPP. There are 2478 observations in Panel A, and 1284 observations in Panel B. Specifications correspond to those in Panel B of Table 2. See notes to Table 2 for additional details.

be eligible for the state but not the federal benefit, or vice versa,[29] making it difficult to identify the appropriate maximum benefit level. As reported in Panel B of Table 3, the estimates are very similar to those in Panel A. In particular, the estimates of γ are negative for each of the labor supply measures (and significant at the 10-percent level for employment). The estimates of $(\gamma' - \alpha')$ are negative and significant at the 5- or 10-percent level for employment and hours. Given the considerable reduction in the sample size, coupled with little change in the point estimates, our last analysis reverts to the sample in Panel A of Table 3.

The evidence to this point indicates that generous supplemental state SSI benefits reduce the labor supply of older men. In any difference-in-difference estimation, it is instructive to ask which differences identify the effect. In particular, in our case we estimate the effects of SSI from the difference between the labor supply of likely participants and unlikely participants in high supplement states, relative to the same difference in low supplement states. Thus, the estimates of γ' can be identified largely from a difference in labor supply between likely participants in the two types of states ("main effects"), largely from a difference in labor supply between unlikely participants in the two types of states, or some combination. Similarly, the estimates of $(\gamma' - \alpha')$ can be identified largely from differences in the drop-off in labor supply from ages 40–49 to ages 60–64 among likely participants in the two types of states ("main effects"), or more from differences among unlikely participants. Whether or not the estimates reflect main effects does not render the empirical procedure valid or invalid (if it did, there would be little reason to use difference-in-difference estimation), but may bear on the interpretation of the estimates. In particular, researchers are more likely to place confidence in the results if they show up in the main effects, and are not driven primarily by differences in behavior among unlikely participants in high and low supplement states, which could arise from omitted state-specific influences on the behavior of unlikely participants.

Table 4 provides evidence on this question, by reporting the decomposition of the difference-in-difference estimates into the differences in behavior for the likely participants and the differences for the unlikely participants. Especially for the direct employment and hours labor supply measures, most (65 to 78 percent) of the estimated effects come from the comparison between the likely participants. Indeed, for employment and hours the estimates of the differences in the drop-off from ages 40–49 to 60–64 between likely participants in the two types of states are statistically significant at the 5- or 10-percent level.[30]

Conclusion

Means testing of social security is one way to resolve the pending social security imbalance, but it has been largely dismissed as a real policy alterna-

TABLE 4. Decompositions of Difference-in-Difference Estimates of Effects of SSI on Labor Supply

	Employed	*Hours*	*Years of covered Social Security employment*	*Family Earnings*
Levels, 60–64				
a. Likely participant, high supplement state - likely participant, no supplement state	−.07	−8.03	−2.76	627.11
b. Unlikely participant, high supplement state - unlikely participant, no supplement state	.04	3.04	1.60	616.13**
Difference-in-difference estimate (a. − b.)	−.11	−11.06*	−4.36	10.97
Percentage of estimate from likely participant comparison	.65	.73	.63	—
Difference, 60–64 vs. 40–49				
a. (as above)	−.15*	−14.13**	−1.69	−597.13
b. (as above)	.05	4.08	1.74	315.53
Difference-in-difference estimate (a. − b.)	−.20**	−18.20**	−3.42	−912.66
Percentage of estimate from likely participant comparison	.75	.78	.49	.65

Source: Authors' calculations, 1984 SIPP. Estimates are based on the specifications and sample in Panel A of Table 3, although excluding the demographic controls. See notes to Tables 2 and 3 for additional details.

tive. This chapter gathers some existing evidence and offers new information required to evaluate how such policy might influence behavior. We draw inferences about the potential consequences of means testing social security by studying the effects of SSI for the aged, which currently operates as a sort of means-tested retirement program. State-level variation in generosity of supplemental SSI payments is used to identify the effects of SSI on labor supply as men approach the age of eligibility for the program, studying a sample of male household heads. We find evidence that SSI discourages work among men nearing the age of eligibility, as one might anticipate given the way the SSI program penalizes post-65 income and assets. While the relevance of these results for a social security program would depend on the exact parameters of a means-tested program, we believe that our evidence implies that the cautious approach that has been taken in the past to the means-testing option is justified.

These results must be used cautiously both as evidence on the effects of SSI, and as evidence on the likely consequences of means testing social security. With respect to the evidence on the effects of SSI, we emphasize that these are the first estimates of which we are aware of the effects of SSI on pre-retirement labor supply, and additional analyses should be done using other data sets. With respect to using our findings regarding SSI to infer the likely consequences of a means-tested social security program, there are three issues that must be kept in mind. First, it is always possible that any new program might be sufficiently different from SSI that our results would not generalize. Second, if the alternative to the means-tested program—for example, a partially-privatized component of a two-tier program—either operates or is perceived to operate very differently from the current social security system, behavioral responses to a means-tested program might differ from the responses to SSI that we estimate. Finally, the SSI program serves a poor population, whereas social security, even if means tested, would be likely to continue to serve a higher-income population for which behavioral responses might differ.

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Notes

1. For some U.S. workers (e.g., relatively low-earning spouses in two-earner families) it is literally true that an additional dollar of payroll tax contributed will not increase the old-age benefit at all.

2. The Concord Coalition advocates "affluence testing" all entitlement programs, not just social security.

3. In the 1970s, Milton Friedman proposed means testing social security (Friedman and Cohen 1972).

4. The exempt amount for couples was $13,500 in 1997.

5. The base of the tax includes one-half of social security income. The source for the 1995 figures is Congressional Budget Office projections reported in the 1994 Green Book (Committee on Ways and Means 1994).

6. The Concord Coalition plan is thus an income-tested, but not asset-tested, scheme.

7. This is akin to the current federal approach to education finance, where a portion of parents' annuitized wealth holdings is added to their incomes to determine how much of their child's education they are expected to finance.

8. The recent social security "privatization" literature reflects this problem, tending to rely more on theoretical and simulation methods, as well as discussion of privatization schemes in other countries, most notably Chile. See for instance, Altig and Gokhale (1996), Arrau and Schmidt-Hebbel (1993), Feldstein (1995), Gustman and Steinmeier (1995), Kotlikoff (1995), Mitchell and Zeldes (1996), and Pennacchi (this volume).

9. The historical information in this section is drawn from Schultz et al. (1991).

10. By the standards of U.S. private saving, these asset limits were fairly high at $76,300 (in 1985 U.S. dollars) for couples who owned homes; and $150,000 for couples who rented (housing equity is excluded from the asset test).

11. The asset test was reintroduced in 1986 in large part as a response to the popular belief that this type of "evasion" was widespread.

12. Powers (forthcoming) examines the impact of the asset test applied in the AFDC program. Hubbard, Skinner, and Zeldes (1995) use a simulation model to assess the impact of asset tests associated with U.S. welfare programs collectively.

13. Unlike social security, there is no early retirement option in the SSI program. However, the SSI program also provides benefits to the blind and disabled irrespective of age, and is also linked to a health care program, Medicaid. We do not consider these components of the program in this paper.

14. Kahn (1987) discusses the definition of countable assets, and McGarry (1996) provides more details regarding the SSI program.

15. Zedlewski and Meyer (1989) estimate that about 30 percent of the elderly poor receive SSI benefits.

16. If states choose to administer the SSI program, they are also free to set their own eligibility criteria such as asset limits. However, many (but not all) states use the federal criteria (Social Security Administration 1985).

17. The SIPP actually identifies "householders," who are the individuals in whose name the home is owned or rented (also referred to as "reference persons"). In the case of a married couple owning a house jointly, either the husband or wife can be listed as the householder. The data set documentation provides no guidance as to who is classified as the householder in the case. To avoid selecting males who might be less likely to be classified as heads of household based on other criteria, we selected only records on male householders. In fact, the majority of those receiving SSI for the aged are single women (Kahn 1987), because of the greater life expectancy and lower earnings of women.

18. A continuous benefits measure would use all of the available information. On the other hand, all we know about individuals prior to age 65 is the maximum benefit available, not the benefit they would actually receive. In addition, the maximum SSI benefits reported in the Green Book (Committee on Ways and Means) are subject to error for some states. For three states (Minnesota, Vermont, and Washington) benefit levels vary by location, and the maximum reported is for metropolitan areas rather than a statewide level. Since none of these states is classified as paying benefits exceeding 20 percent of the federal level, this measurement problem does not result in misclassification when this particular threshold is used. Regardless of these considerations, the qualitative results are similar using a continuous measure of maximum benefits (including all states), or—as in the reported results—distinguishing states with generous supplemental benefits from states that do not supplement benefits. Appendix Table A1 discusses state supplemental benefit patterns.

19. We suspect that trivially small supplements have no effect on behavior. This was confirmed in unreported results using a zero threshold for the supplement dummy variable, for which the estimated effects were weaker but qualitatively similar than those reported in the paper.

20. We also experimented with using a continuous measure of the estimated probability of participation; the results were similar to those reported below.

21. The notation Y| . . . means the expectation of Y conditional on the information that follows the |.

22. For example, the difference-in-difference parameter capturing the effect of generous state SSI supplementation on the level of Y for likely participants aged 60–64 is

$$(Yl_{Supp=1,Part=1,Age6064=1} - Yl_{Supp=1,Part=0,Age6064=1})$$
$$- (Yl_{Supp=0,Part=1,Age6064=1} - Yl_{Supp=0,Part=0,Age6064=1})$$
$$= (\gamma + \delta + \theta + \gamma' + \delta' + \theta' - \delta - \theta - \delta' - \theta') - (\gamma + \delta + \theta - \delta - \theta) = \gamma'.$$

23. In principle, we could also use variation over time in state supplemental benefit levels, and hence use the earlier observations on likely participants in a state as a "control group." However, variation over time in state supplements is minimal, with many states staying fixed (nominally) from year to year, and most states having only small changes over longer periods (Committee on Ways and Means, various years).

24. Because the variable measuring years of food stamp receipt may reflect both age effects and cohort effects, we do not remove the effect of age on this variable in estimating the probability of participation for those under age 64 based on the probit estimates. That is, a 60-year-old with 10 years on food stamps might not have lower permanent income or a lower propensity to enroll than a 50-year-old with the same number of years on food stamps, if cohort effects have boosted food stamp usage among younger cohorts.

25. Because we assume financial resources are endogenous with SSI participation, we do not include them in our estimation of participation probabilities. Not surprisingly, financial resources are strongly negatively correlated with SSI participation (McGarry 1996).

26. Some care must be exercised in interpreting the estimates of β', γ', $(\beta' - \alpha')$, and $(\gamma' - \alpha')$ as effects of SSI on labor supply. Because supplemental benefit levels may influence participation, an indicator of the benefit levels was included in the participation equation. In principle, then, a change in benefit levels could also change the classification of an individual as a "likely participant." Because we do not allow such changes in estimating the effects of SSI, the estimates must be interpreted conditional on this classification not changing. Such an interpretation is likely to be valid for most of the observations in the data set, with the exception of those initially on the border line between being classified as a likely or unlikely participant.

27. In Appendix Table A2, we report estimates of the same specifications using the 75th and 95th centiles of the estimated probabilities of participation, rather than the 90th. As expected if the disincentive effects are stronger for more likely participants, the estimated effects of SSI are weaker when the 75th centile is used, and stronger when the 95th centile is used. This holds true for the estimated effects for 60–64 year olds. Using the 75th centile (in Panel A), the signs of the estimated effects are generally the same as for the 90th centile—in the direction of labor supply disincentive effects—although the estimated effects are not statistically significant. On the other hand, using the 95th centile yields uniformly larger estimated effects, with the estimates of $(\gamma' - \alpha')$ statistically significant at the 5- or 10-percent level for the three direct labor supply measures. However, the standard errors are quite a bit larger than when the 90th centile is used. Thus, results using the 90th centile are used in the remainder of the paper.

28. The restriction to those without a work-impairing disability reduces the sample size by 462. When we deleted an additional 83 observations with transfer income from any means-tested program, the results were very similar.

29. McGarry (1996) notes that this occurs frequently.

30. The estimates come from the same specification and sample used in Panel A of Table 3, although dropping the demographic controls because they are so strongly associated with use of SSI (see Table 2). As the second-to-last row in each panel of Table 4 shows, the difference-in-difference estimates were little changed by dropping the demographic controls.

APPENDIX TABLE A1. State SSI Supplemental Maximum Benefits, 1985

State	*Individuals*	*Couples*	*>20% of Federal Benefit*	*Federally-Administered*
Alabama				
Arizona				
Arkansas				Yes
California	179	448	Yes	Yes
Colorado	58	278	Yes	
Connecticut	172	119	Yes	
Delaware				Yes
Washington, D.C.	15	30		Yes
Florida				
Georgia				Yes
Hawaii	5	9		Yes
Illinois	35	34		
Indiana				
Iowa				Yes
Kansas				Yes
Kentucky				
Louisiana				Yes
Maine	10	15		Yes
Maryland				Yes
Massachusetts	129	202	Yes	Yes
Michigan	27	40		Yes
Minnesota	35	66		
Missouri				
Montana				Yes
Nebraska	61	89		
Nevada	37	74		Yes
New Hampshire	27	21		
New Jersey	31	25		Yes
New York	61	76		Yes
North Carolina				
North Dakota				
Ohio				Yes
Oklahoma	60	120	Yes	
Oregon	2			
Pennsylvania	32	49		Yes
Rhode Island	54	102	Yes	Yes
South Carolina				
Tennessee				Yes
Texas				
Utah	10	20		
Vermont	53	97		Yes
Virginia				
Washington	38	37		Yes
Wisconsin	100	161	Yes	Yes

Source: 1985 Green Book and Kahn (1987). Sample is restricted to states individually identified in the SIPP. The maximum federal benefits were $325 for individuals, and $488 for couples. Classification in column 3 is based on maximum benefit for either an individual or a couple. In California and Wisconsin, the cash value of food stamps is included in the supplement (Zedlewski and Meyer 1989). For a small number of individuals living with non-recipients or ineligible spouses, the maximum benefit is reduced.

APPENDIX TABLE A2. Sensitivity Analysis for Estimated Effects of SSI in Panel B of Table 2, Alternative Definitions of Likely SSI Participants, Male Household Heads Aged 40–64

Estimator	*Employed Probit*	*Hours Tobit*	*Years of Covered Soc. Sec. Employment Tobit*	*Family Earnings Tobit*
A. Using 75th centile of participation probabilities				
Effect of SSI on 50–59 year-olds (β')	.04	5.04	1.56	−254.33
	(.05)	(3.26)	(2.48)	(314.18)
Effect of SSI on 60–64 year-olds (γ')	−.05	−3.37	−2.03	−180.88
	(.06)	(4.57)	(3.49)	(435.30)
Effect of SSI on 50–59 year-olds relative to 40–49 year olds ($\beta' - \alpha'$)	−.01	1.57	3.61	−670.18
	(.07)	(4.54)	(3.45)	(437.26)
Effect of SSI on 60–64 year-olds relative to 40–49 year olds ($\beta' - \alpha'$)	−.01	−6.83	.02	−596.73
	(.08)	(5.62)	(4.28)	(537.00)
Log likelihood	−1129.8	11562.7	−10663.8	−23436.4
B. Using 95th centile of participation probabilities				
Effect of SSI on 50–59 year-olds (β')	.09	10.94**	4.72	−53.79
	(.07)	(5.44)	(4.19)	(534.45)
Effect of SSI on 60–64 year-olds (γ')	−.09	−14.10	−13.34**	−672.82
	(.10)	(8.66)	(6.70)	(849.79)
Effect of SSI on 50–59 year-olds relative to 40–49 year olds ($\beta' - \alpha'$)	−.08	−1.79	2.37	−747.88
	(.11)	(8.27)	(6.32)	(805.22)
Effect of SSI on 60–64 year-olds relative to 40–49 year olds ($\gamma' - \alpha'$)	−.26*	−26.84**	−15.69*	−1366.91
	(.14)	(10.72)	(8.24)	(1046.52)
Log likelihood	−1119.4	−11551.9	−10655.9	−23428.7

Source: Authors' calculations, 1984 SIPP. There are 2940 observations in Panels A and B. The 75th centile of participation probabilities (as well as the 80th and 85th centiles) is .0381; the 95th centile is .0962. See notes to Table 2 for additional details.

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