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The Effect of Means-Tested Income Support for the Elderly on Pre-Retirement Saving: Evidence from the SSI Program in the U.S.

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## ABSTRACT

We attempt to draw inferences about potential behavioral responses to means-tested income support for the elderly by examining the effects on saving of the Supplemental Security Income (SSI) program for the aged in the U.S. Part of the SSI program provides payments to the poor elderly, thus operating as a means-tested public retirement program. The federal government sets eligibility criteria and benefit levels for the federal component of the program, but many states supplement federal SSI benefits substantially. We exploit the state-level variation in SSI benefits to estimate the effects of SSI on saving. We use data from selected waves of the 1984 Survey of Income Program Participation (SIPP). We find evidence that high SSI benefits reduce saving among households with heads who are approaching the SSI eligibility age and who are likely participants in the program.

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## I. Introduction

The elderly are one of the exceptional groups in American society with access to a significant cash safety net, a means-tested program called Supplemental Security Income (SSI). Although the potential disincentives posed by means-tested income-support programs for other populations (such as AFDC) have received considerable attention from both researchers and politicians, the potential disincentives posed by such programs for the elderly have largely escaped attention. In particular, the SSI program may create disincentives for saving as individuals approach the age of eligibility, because of both asset and income tests. Given that public policy often attempts to ensure that elderly individuals accumulate sufficient wealth to support their retirement--for example, Social Security and ERISA--it would be ironic if we also have policies in place that discourage saving among the more disadvantaged elderly population that is potentially eligible for SSI. However, there is no empirical evidence on whether SSI actually leads to discernible disincentives to save.[1]

In this paper, we examine the effects of SSI for the aged on saving.[2] The federal government sets eligibility criteria and benefit levels for the federal component of the program, specifying maximum benefit levels for couples and individuals that are reduced by income from other sources, including Social Security benefits and Disability Insurance. (The first $20 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 income influence both eligibility for and potential payments under SSI. Financial resources also affect eligibility. For example, as

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[1]There may also be work disincentives prior to the age of eligibility for SSI for the aged (see Neumark and Powers, 1997).

[2]The SSI program also provides benefits to the blind and disabled irrespective of age. We do not consider this component of the program in most of the analysis in this paper.

of 1985 individuals with over $1600 in countable assets, and couples with over $2400 in countable assets, were ineligible.[3] In September 1984 (corresponding roughly to the time period covered by our data), there were 1.55 million persons receiving SSI payments who were eligible because of age (1995 Green Book).[4]

In addition to the federal component of the program, states may supplement federal SSI benefits. For example, in January 1985 the maximum federal benefit was $325 for an individual, and $488 for a couple. The highest state benefit was in California, which resulted in a maximum combined benefit of $504 for an individual, and $936 for a couple.[5] In December 1985 the average federal benefit paid was $146 for individuals, and $232 for couples, and the average state supplements were $97 and $257, respectively (Kahn, 1987), with 39 percent of SSI recipients receiving state supplements.

We exploit the state-level variation in SSI benefits to estimate the effects of SSI on saving. We use data mainly on male householders from the 1984 panel 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 saving across states and across different types of individuals. We find evidence that high SSI benefits reduce saving among households with heads approaching the age of SSI eligibility, and who are likely to

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[3]Kahn (1987) and McGarry (1996) discuss the definition of countable assets and many other details of SSI.

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

[5]If states choose to administer the SSI program, they are also free to set their own eligibility criteria such as asset limits. However, variation in these criteria is generally minor (Social Security Administration, 1985).

end up participating in the program.[6] This evidence is robust to many (but not all) of the changes in the sample, specification, or definition of variables that we consider. Overall, then, we view the evidence as providing a relatively consistent picture of dissaving effects induced by the SSI program.

## II. The Data and Empirical Approach

### *The Data*

We use a sample drawn from the SIPP, which when weighted serves as a nationally-representative sample of households. The SIPP attempts to gather detailed and reliable data on income and welfare program use that are impractical to collect in the larger Current Population Surveys. 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. However, questions about wealth holdings refer to the last day of the month preceding the sample month. This paper uses the first (1984) panel of the SIPP, which covers the period from October 1983 through July 1986. This panel exceeds the size of the next-largest panel by 27 percent. It also contains more complete measures of financial assets and liabilities than other SIPP panels.

Our analysis focuses primarily on male householders (including males living alone). In our primary sample, 85 percent of these males are currently married. Since wealth and saving are usually interpreted as household-level variables, our sample therefore covers most men and a large proportion of women. We also report limited evidence on a considerably smaller subsample of female householders, most of whom (86 percent) are not currently married.[7]

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[6]For an analysis of the effects on saving behavior of asset limits in various types of welfare programs, see Powers (1995 and forthcoming).

[7]The SIPP identifies "householders" (or "reference persons") as individuals in whose name the home is owned or rented. We seek to identify those who can reasonably be viewed as

Dependent variables for the analyses are SSI participation (for those aged 65 and over), and saving and wealth measures (for those aged 60-64). The dummy variable for SSI participation for those aged 65 and over is based on participation at any time during wave 4. Saving is measured as the change in net wealth excluding housing, from wave 4 to wave 7.[8] This includes all financial assets plus non-housing property minus liabilities. Although liabilities are not counted against wealth in determining eligibility for SSI, because debts can be paid off prior to applying for SSI net wealth is the most appropriate measure of assets with respect to which eligibility would be determined.[9] Corresponding to the definition of counted wealth under SSI, we exclude housing.[10] Our wealth measure includes IRA and Keogh accounts, but we do not have information on other defined contribution plans.

Each household 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. Appendix Table A1 summarizes the variation in state supplemental benefits as of January 1985, taken from the 1985 Green Book.[11] Demographic variables in the analysis include race (black or

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heads of their family unit. To avoid selecting those who might be less likely to be classified as heads of household based on other criteria, we selected only records on householders. In the case of a married couple owning a house jointly, either the husband or wife could be listed as the householder, but we find relatively few married female householders.

[8]The assets and liabilities topical modules from which the data are drawn do not contain imputation flags to identify those observations for which wealth was imputed. We did, however, discard all observations in which the householder was not interviewed in at least one wave. The SIPP weights are adjusted to account for this non-response.

[9]The qualitative results were similar using gross rather than net wealth; the results were if anything somewhat stronger, as a consequence of smaller standard errors.

[10]Vehicles are fully exempt from the SSI asset test if they are required for medical reasons or employment (and are exempt up to $4500 of market value regardless). We therefore exclude the value of respondents' cars, although we have no way to verify whether the first condition was met. However, the results were very similar when vehicles were included.

[11]There are some caveats to the benefits reported in the Green Book. First, in two states (California and Wisconsin) SSI recipients are denied food stamps, and the cash value of food

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).

Descriptive statistics for the sample of 60-64 year-olds on whom we focus most of our attention are reported in column (1) of Table 1. Descriptive statistics for individuals aged 65 or older, disaggregated by their current SSI participation, are reported in columns (2) and (3).[12] As is commonly found in data on financial wealth, median wealth is quite low for many elderly households. For SSI participants aged 65 and over, it is $0, while for non-participants the median is $24000 but the 25th centile is only $2080.[13]

*The Empirical Approach*

We are interested in estimating the effects of the potential receipt of SSI benefits on saving (denoted S). Two factors influence the potential value of SSI benefits: the level of 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,

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stamps for which they would be eligible is instead added to SSI benefits, possibly overstating SSI benefits in these states. However, subtracting the maximum food stamp benefit from the reported SSI benefit in these states did not change the findings. Second, for two states (Connecticut and Illinois) benefits are decided on a case-by-case basis, and the maximum benefit reported in the Green Book is simply an estimate provided by a state official. To avoid problems with measurement error we also recomputed the estimates excluding observations from these two states; the results were unchanged. Finally, for three states (Minnesota, Vermont, and Washington) benefit levels vary by location, and the maximum reported is either for metropolitan areas, in which benefits are presumably higher, or the Green Book explicitly states that the highest level is reported. Again, the results were unaffected by dropping these three states.

[12]Unfortunately, we cannot distinguish those who entered the program for the aged after their 65th birthday from those who entered the program for the disabled at an earlier age.

[13]Median wealth is lower than reported in Poterba, et al. (1994), who also use SIPP data. However, they measure wealth of elderly individuals and their spouses (if present), whereas we measure total household wealth, and focus only on male householders. The latter difference may be particularly important, as elderly individuals who are not householders and elderly women generally have lower wealth. For example, for female householders aged 60-64, median household wealth is 3799, and the 25th centile is 0, compared with the considerably higher numbers reported for male householders in column (1).

to be most likely to respond by reducing saving. In contrast, a married college graduate is very unlikely to become eligible for SSI, whether he resides in a state with high or low benefits. For a number of reasons, only relatively older individuals are likely to respond significantly to the incentives posed by the SSI program. First, the asset limit rules do not preclude rapid dissaving near the age of eligibility (although they do prohibit asset transfers). Second, younger individuals are more likely to maintain higher asset levels for precautionary reasons, and only to run them down near the age of eligibility. Third, given stochastic influences on wealth and earnings, older workers can form better predictions of post-retirement assets and income. Finally, we suspect that workers pay more attention to the potential receipt of SSI benefits as they approach the age of eligibility. In most of the analyses, therefore, our empirical strategy is to focus on 60-64 year-olds, identifying individuals who are relatively likely to be eligible for SSI ("likely participants"), and estimating the effects of SSI on saving from the relationship between state supplemental SSI benefits and saving for these individuals.

We begin by examining individuals over age 65, to identify characteristics associated with a high likelihood of SSI participation. We then distinguish among workers aged 60-64 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.[14] With this variable in hand, the simplest strategy would be to use only the subsample of likely participants, and to estimate an equation of the form:

$$(1) \qquad S_{ij} = \gamma \cdot Supp_{ij} + \epsilon_{ij}\,, \; i \in \{Part = 1\}\,,$$

where $Supp_{ij}$ is the maximum state-level SSI benefit available to the individual, i indexes

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[14]Because financial resources will be one of the endogenous variables we study, we predict participation based on largely exogenous characteristics, and not financial resources of the elderly. Not surprisingly, the latter are strongly related to SSI participation (McGarry, 1996).

individuals, and j indexes states.[15,16] In this specification, $\gamma$ is identified from differences in saving between likely participants in states with low (or no) SSI supplements, and states with high supplements. If SSI reduces saving of likely participants, we should find $\gamma < 0$.

In equation (1), individuals in states without state supplementation of SSI benefits, but with characteristics associated with SSI receipt, serve as the control group with which to compare the behavior of individuals in states with state supplementation, and with characteristics associated with SSI receipt. However, average saving in a state may be correlated with SSI benefits for reasons independent of program incentives. Thus, rather than relying on estimates of equation (1), we use a difference-in-difference estimator that identifies the effects of SSI from the difference--between states that do and do not supplement SSI--in the difference in saving between individuals likely to participate in SSI and individuals unlikely to participate in SSI. For example, individuals in states that supplement SSI generously may save more irrespective of their likelihood of participation in SSI, in which case estimates of equation (1) would suggest that SSI increases saving. However, if the shortfall between the saving of likely participants and unlikely participants is larger in states that supplement SSI than in states that do not supplement SSI, the difference-in-difference estimator will yield estimates indicating that SSI decreases saving. In particular, we include both likely participants and unlikely participants in the sample, and estimate

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[15]The maximum benefit is higher for married couples than individuals. Couples with a spouse below the age of 65 are only eligible for the individual benefit. (Benefits are usually reduced if the recipient lives in another person's household.) We use marital status to define the maximum benefit. Because this can generate some measurement error (as can measuring supplemental SSI benefits in some states), we also experimented with a dummy variable indicating generous benefits (exceeding 20 percent of the federal benefit) in a state, in contrast to the continuous specification we use for the results reported in the tables; the results were qualitatively similar.

[16]Demographic control variables are omitted in the exposition, but included in many of the specifications estimated.

an equation of the form

(2) $S_{ij} = \alpha \cdot Supp_{ij} + \beta \cdot Part_{ij} + \gamma \cdot Part_{ij} \cdot Supp_{ij} + \epsilon_{ij} \quad , \quad \forall i$ .

In this specification $\alpha$ is identified from variation in saving of unlikely participants in states with different levels of SSI supplementation, and $\beta$ is identified from variation in saving between likely participants and unlikely participants in states that do not supplement SSI. Finally, $\gamma$ is identified from the difference between the saving of likely participants in states with high vs. low supplementation of SSI, relative to the difference between the saving of unlikely participants in the two types of states, which is the variation we want to use to obtain the difference-in-difference estimate. Including both likely and unlikely participants along with the Supp variable in equation (2) can be thought of as analogous to introducing fixed state effects capturing the relationship between policy and the distribution of Y that arises for reasons other than responses of the likely participants; because we have a single cross section, we model these state effects in a more restrictive fashion than introducing state dummy variables.[17]

The discussion to this point is framed in terms of saving. However, there are two reasons why it is also of interest to look at wealth held by individuals approaching the age of eligibility for SSI. First, SSI eligibility depends on the level of assets held. Thus, one might argue that if dissavers in states with higher SSI supplements have higher wealth despite their dissaving, then the dissaving effects may stem from sources other than the incentive effects of SSI. In fact, this argument is not necessarily correct, since an individual may go on SSI after age 65. Thus, for example, a 64 year-old likely participant in a state with a high SSI supplement may be dissaving

[17]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. Many states' supplements are fixed (nominally) from year to year, and most states have only small changes over longer periods (Green Book 1984, 1985, 1986, 1990, 1991).

relatively more rapidly in order to become eligible for SSI, but, because he does not intend to hit the asset limit until some age beyond 65, may still have relatively higher wealth at age 64 than a likely participant in a state with no SSI supplement. The second reason to look at asset levels may be measurement error in these data, which could be exacerbated in the differenced variable used to define saving; of course, if the measurement error has some persistence over time, it could be reduced rather than exacerbated in the saving variable. Regardless, equation (2) is also estimated using asset levels.

Finally, to assess the robustness of the evidence on saving, the wealth-level specification is augmented to estimate changes in wealth between ages 60-62 and ages 63-64, providing alternative estimates of the effects of SSI on saving. In particular, we estimate an equation of the form

$$(3) \quad W_{ij} = \alpha \cdot Supp_{ij} + \alpha' \cdot Supp_{ij} \cdot Age6364_{ij} + \beta \cdot Part_{ij} + \beta' \cdot Part_{ij} \cdot Age6364_{ij}$$
$$+ \gamma \cdot Part_{ij} \cdot Supp_{ij} + \gamma' \cdot Part_{ij} \cdot Supp_{ij} \cdot Age6364_{ij} + \epsilon_{ij} \ ,$$

where W is wealth and Age6364 is a dummy variable indicating age 63 or age 64.[18] In this specification, $\gamma$ captures the effect of SSI supplements on the wealth of 60-62 year-olds, while $\gamma'$ captures the differential effect on 63-64 year-olds (always using the difference in differences between likely participants and unlikely participants in states with high and low supplements to identify the parameters). Thus, $\gamma'$ identifies the effect of SSI on changes in wealth of likely participants. However, because this estimate uses cross-cohort information on levels of wealth to infer saving effects, rather than direct information on saving, it may be less compelling.[19]

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[18]We use wave 7 (end-of-period) wealth since we are interested in corroborating whether the estimated saving effects are reflected in wealth levels "after" the dissaving occurs.

[19]Attanasio (1993) presents evidence on differences in saving profiles across cohorts of older individuals.

## III. Results

### *SSI Participation*

The descriptive statistics in columns (2) and (3) of Table 1 provide demographic and other information for men aged 65 and over, classified by whether or not they are actual SSI participants. Participants are much more likely to have less than a high school education, and virtually guaranteed not to be college graduates. They are also more likely to be black, and never married, divorced, widowed, or separated. Finally, not surprisingly, SSI participants are more likely to ever have been authorized for food stamps, and to have been authorized for food stamps for more years cumulatively.

The table also reveals differences in state SSI supplementation in the states of residence of participants versus non-participants. In general, participants are less likely to reside in states that supplement SSI, presumably because SSI recipients are concentrated in poorer states that are less likely to supplement benefits. However, participants are more likely to reside in states with generous SSI supplements; for example, the table shows that recipients are more likely to reside in states whose SSI supplements exceed 20 percent of federal benefits (all such figures refer to maximum benefits), and the average maximum benefit in states in which participants reside is somewhat higher than the average benefit facing non-participants.

Column (4) of Table 1 reports estimates from a probit model for SSI participation. The estimates indicate that many of the univariate differentials with respect to SSI participation apparent in columns (2) and (3) continue to hold in the multivariate analysis. Specifically, blacks, those with less than a high school education, and those who never married, are significantly more likely to participate in SSI. In addition, those with a more substantial history of food stamp use are more likely to participate. Finally, because SSI benefits themselves may affect participation, the

maximum available benefit is also included in the probit, and has a positive and statistically significant coefficient estimate,[20] indicating that the generosity of state SSI supplements affects program participation.

The probit estimates are used to predict probabilities of subsequent SSI participation for those aged 60-64, to construct the Part variable in equations (1)-(3). For most of the results we present, we define Part based on the 90th centile. 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, which seems like a reasonable way to define "likely participants." It may seem preferable to use a higher cutoff, to isolate the effects of SSI on those who are likely to participate in SSI with much greater certainty. However, doing this entails some offsetting influences. On the one hand, the higher probability of participation should induce more dissaving. On the other hand, those with higher probabilities may not engage in as much dissaving, since they may have little accumulated wealth; they may, in fact, not have to engage in any dissaving at all. In other words, the effects of SSI on the extremely poor may be of little interest, if they do not save anyway. Rather, the most interesting question may be whether higher SSI benefits generate dissaving among those with some chance of going on SSI, but who might otherwise accumulate assets above the limit.

*Descriptive Statistics on Saving and Wealth*

Table 2 reports some simple descriptive statistics on saving and wealth of 60-64 year-olds

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[20]If the benefits were excluded, then given that benefits are positively associated with participation, the probability of participation would be underestimated in high benefit states and overestimated in low benefit states. On the other hand, some care must be exercised in interpreting the estimates of $\gamma$ and $\gamma'$. These coefficients are identified from variation in supplemental SSI benefits. In principle, however, a change in benefit levels could also change the classification of an individual as a "likely participant." Because we do not incorporate changes in this classification 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.

that provide information on the magnitudes and sample sizes involved, and illuminate the potential dissaving effects of SSI. The table also presents some crude difference-in-difference estimates that clarify the statistical experiment and highlight some estimation issues.

Prior to looking for effects of SSI on saving or wealth of likely participants, it is worth asking whether households in this relatively disadvantaged segment of the population have any financial assets that would have to be run down to satisfy the asset test. We know from existing work (e.g., Poterba, et al., 1994) that median financial assets of households with heads near age 65 are quite low. If asset holdings are also very low at age 60, SSI is unlikely to have much effect; in contrast, if asset holdings are higher at age 60, SSI may help to explain low asset holdings near retirement. Panel A of Table 2 presents medians, 25th centiles, and 75th centiles for the dependent variables used in the empirical analysis, in particular saving and wealth for those aged 60-64, and wealth for the two subgroups aged 60-62 and 63-64, as well as the proportions with positive wealth. The data are further broken down by likely and unlikely participants. The figures reveal that median wealth of likely participants is in fact very low, equal to or very near zero (while median wealth of unlikely participants is above $20000). However, a high proportion of likely participants aged 60-64 (.45) have positive financial wealth, and the 75th centile of household wealth is $12299. Thus, a sizable fraction of likely participants have wealth that could be run down to satisfy asset tests. More importantly, when the data are broken down by separate age groups, we see that the 75th centile of household wealth is quite a bit lower for those aged 63-64 ($4999) than for those aged 60-62 ($12299), pointing to some asset decumulation over this age range. These figures do not, of course, address the issue of the effects of SSI, since they do not introduce state-level variation in benefits. But they do point to the plausibility of dissaving effects among part of the population that is likely to become eligible for SSI.

Panel B presents crude difference-in-difference estimates of the effects of SSI. These estimates are crude because they do not exploit variation in state SSI supplementation, which is substantial, and because no other control variables are included. Thus, these calculations should be interpreted as illustrative of the research strategy we employ, whereas more reliable estimates appear in later tables. The first row considers saving. The two left-hand entries indicate that likely participants in supplement states actually have slightly higher saving ($95) than likely participants in non-supplement states ($0). However, the right-hand entries indicate that unlikely participants in supplement states have considerably higher saving ($3223) than unlikely participants in non-supplement states ($732). Thus, the difference-in-difference estimate of the effect of state SSI supplementation is -2396. Note that the negative estimate arises because of the difference in saving between unlikely participants in supplement and non-supplement states, rather than a difference among likely participants. The next row in Panel B repeats this analysis for wealth. Here also the effect is identified from the difference in wealth between unlikely participants in supplement and non-supplement states, leading to a large difference-in-difference estimate of -16443.

These figures highlight the role of the difference-in-difference estimator. For saving, for example, without differencing relative to unlikely participants, the estimated effect of SSI supplementation on saving would be positive (95). The significant role played by differences among unlikely participants (the "control sample") may cast some doubt on the estimated effects of SSI. In our view, any evidence is stronger if it turns out that the "main effects" (those obtained in the first or simple difference, for the "treatment sample") are the same as those obtained from the difference-in-difference estimate. On the other hand, the motivation for difference-in-difference estimation is that there may be differences across states in the treatment sample that are

correlated with but not caused by the policy being studied, and the control sample--in particular the difference in this sample across states where the policy is and is not implemented--accounts for these differences.

Nonetheless, as Panel B of Table 2 illustrates in a particularly striking fashion, when the difference-in-difference estimate is driven by the control sample, we have to ask whether a plausible estimate results, or whether instead the estimate may be driven by differences across states among unlikely participants that are unrelated to the policy. In the case of wealth, in particular, given that likely participants have low median wealth, an effect of SSI on wealth at the median that exceeds $10000 is not plausible. Because of this concern, below we also present evidence using an alternative control sample that ought to have wealth levels in the same range as the levels of wealth among 60-64 year-old likely participants. In addition, following all of the regression estimates we provide decompositions that show the reader how much of the difference-in-difference estimate comes from the main effects. Finally, we note that for the saving specifications for which results follow, in contrast to Table 2 a sizable fraction of the difference-in-difference estimate generally comes from the treatment sample, bolstering our confidence in the estimated effects of SSI on saving.

*Effects of SSI on Saving and Wealth*

In Table 3 we turn to the regression equations for the difference-in-difference estimation of the effect of SSI on saving, using the direct saving measure. Column (1) reports estimates of equation (2), with no other control variables added. Given the extreme outliers in the saving (and wealth) data, as reflected in the mean vs. median comparisons in Table 1, we report median regressions throughout.[21,22] The estimated effect of SSI benefits is negative (-19.56) and

[21]OLS regressions yielded extremely imprecise estimates, whereas robust (Huber) regressions yielded similar estimates to the median regressions. For example, for the

statistically significant. To provide a better interpretation of the results, below the estimates we report a decomposition of the difference-in-difference estimate. We translate the estimates into the effects of a $100 increase in yearly SSI benefits. For this specification, most of the difference-in-difference estimate comes from the control sample, as the estimated differential between unlikely participants in the two types of states is 161, for a difference-in-difference estimate of 163.[23] It may seem that a $163 reduction in saving is an overly large response to a $100 increase in benefits. But changes in saving primarily influence the intertemporal pattern of consumption, and may impose relative minor costs on individuals.

There is a potentially important source of misspecification in the estimates in column (1) of Table 3. Generally, states with high SSI supplements also offer relatively generous benefits in other transfer programs, including, of course, SSI for the disabled. These other programs may be available not only to the elderly, but also to younger men, in particular the disabled. As a consequence, we may be detecting dissaving effects among those below age 65 that are a response to programs other than SSI for the aged. In addition, it is possible that older individuals in our sample who have health problems are spending down their assets to become eligible for Medicaid,

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specification in column (1), the OLS estimate of the likely participant × state supplement coefficient ($\gamma$ in equation (2)) was -83.21, with a standard error of 124.40. The robust regression estimate (unweighted) was -27.41, with a standard error of 15.48.

[22]As explained above, the estimates are based on a chosen threshold for the predicted probability of participation in SSI. Although the use of a predicted probability leads to some understatement of the standard errors, in our view the far greater source of uncertainty is the threshold chosen. As a result, below we present estimates using an alternative threshold for the probability of SSI participation. Given uncertainty over the threshold (as well as the use of a predicted probability), statistical inferences should be treated cautiously.

[23]The estimated effect in the first row of -19.56 translates into an estimated effect of a $100 increase in benefits of -$163 because the monthly benefit is used in the regression estimate. Multiplying 100/12 by -19.56 yields the estimate of -163.

which unlike Medicare covers nursing home care.[24] In this case, we may again be detecting effects unrelated to general means-tested income support for the elderly. To better isolate the incentive effects of SSI for the aged, and in particular to eliminate spurious correlations that may arise through relationships between disability and income-support or other programs available to the younger disabled, we proceed to drop from the sample individuals with any self-reported work-impairing disability.

Column (2) reports estimates for the same specification as in column (1), for this more restrictive subsample.[25] The estimated effects of SSI on saving become stronger and remain statistically significant. The stronger negative effects may occur because spend-downs for health-related problems occurred earlier and therefore do not appear as strong for 60-64 year-olds with health problems. The estimates imply that a $100 increase in annual benefits reduces saving by $281 among likely participants in this age group. The decomposition of the difference-in-difference estimate indicates that nearly one-half (.45) of this effect is identified from lower saving among likely participants in high-supplement states vs. non-supplement states, which bolsters the causal inference of a dissaving effect of SSI.

The next two columns consider some robustness checks. First, column (3) uses the same sample, but adds demographic controls (the same ones introduced in the probit estimates reported in Table 1). This has relatively little effect on the estimates, except that the proportion of the difference-in-difference estimate that comes from the likely participant comparison rises to .73. Second, the figures on SSI benefits in Appendix Table A1 indicate that California pays

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[24]Federal Medicaid regulations first allowed states to drop asset tests in 1988, and many states did so (Gruber and Yelowitz, 1997). But this change comes after our sample period.

[25]Among those with a reported disability, 14 percent report transfer income from a means-tested program. When we deleted an additional handful of observations with such transfer income but no reported work-impairing disability, the results were very similar.

extraordinarily high SSI benefits. In addition, of course, California has a major concentration of sample members, and a large immigrant population that may have different saving behavior. Thus, it is conceivable that results for California drive the results discussed so far. To avoid undue influence of these observations, in column (4) we add variables that let the saving of both likely and unlikely participants in California differ from those of the rest of the sample, consequently identifying the effects of SSI from variation in benefits (and saving) for the other states. In this particular case, these additional controls are not jointly significant, and the point estimates are largely unchanged, although the estimated dissaving effect of SSI becomes statistically insignificant.[26]

Finally, to this point we have focused on a dependent variable--saving--that is hypothesized to be affected by the incentives posed by SSI. Generally, we have found evidence of the hypothesized dissaving effect. It is conceivable, however, that other sources of differences in saving or the accumulation of wealth underlie these results. One way to check this is to look at a source of wealth that should not be affected by SSI, and verify that--using our estimation strategy--it is not affected. Thus, in column (5) we look at the change in housing equity, a variable that might be correlated with other changes in wealth, but should not be influenced by SSI since housing is excluded from countable wealth. As the table shows, there is no evidence that higher

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[26]We also considered potential problems from measuring features of state SSI programs. As mentioned earlier, some states choose to administer their own programs, in which case they can set their own eligibility criteria. We reestimated the specification in column (3) for the subset of states with federal administration of SSI (based on information in Kahn, 1987), to ensure that we are looking at states with federal administration and therefore identical asset limits. This is potentially important because in a state with asset limits that differ from the federal limits, one can be eligible for the state but not the federal benefit, or vice versa (McGarry, 1996). As a result, for states with different asset limits it is difficult to identify the appropriate maximum benefit level. However, the estimates for this subsample were qualitatively similar to those for the full sample.

SSI supplements are associated with the same kinds of declines in housing wealth that we find looking at non-housing wealth.[27] In fact the estimated effect, while insignificant, is positive.

As discussed earlier, it is also of interest to look at results using data on wealth levels, rather than saving.[28] Panel A of Table 4 reports results for the same specifications used in Table 3, with wealth as the dependent variable. The coefficient estimates in the first row of columns (1)-(4) are negative in three of four cases, consistent with SSI benefits lowering wealth held by 60-64 year-old likely participants. However, only one of these estimated coefficients is statistically significant. In addition, the results of decomposing the wealth effects indicate that the difference between likely participants in high-supplement vs. non-supplement states never contributes to an estimated negative effect of SSI on wealth. Thus, on grounds of either statistical significance, or the desirability of estimated "main effects" having at least the same sign as the difference-in-difference estimates, the evidence that SSI reduces levels of wealth among 60-64 year-olds is weak. The greater contribution of the control sample to the difference-in-difference estimate when we examine wealth may reflect a greater role of unobservable or unmeasured differences across states; we suspect that such unobservables are more likely to bias results for wealth, which reflects long-term asset accumulation, than for saving measured over a relatively short period. This is another reason the saving specifications may provide a better statistical experiment.

However, the results in Panel A do not imply that likely participants in high-supplement states are not running down their wealth to a greater extent than those in low-supplement states; those in high-supplement states may simply be starting from sufficiently higher wealth levels such

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[27]Note that here we report robust regression estimates rather than median regression estimates, because of the large spike at zero in the distribution for the change in housing wealth.

[28]Of course, the rules of the SSI program may allow conversion of financial assets into other forms of non-countable wealth (such as housing, some durable goods, etc.). Thus, literally speaking, we are mainly estimating the effects on financial and (non-housing) property wealth.

that the average over the 60-64 age range is not much lower. Thus, Panel B reports estimates of equation (3), which allows us to infer the effect of SSI on the change in wealth from ages 60-62 to 63-64. We caution, however, that the sample sizes for which the effects are now estimated become quite small, as the treatment group of interest includes 63-64 year-old likely participants in states that supplement SSI. In columns (1)-(4) the estimated effect of SSI is negative. The estimated effect is marginally significant in each case, and statistically significant when the California controls are included (as well as in column (1)); tests of joint significance indicate that the specification with these additional controls is preferred. The decomposition indicates that in column (4), although not the other columns, the difference-in-difference estimate comes from the main effects; in fact, the unlikely participants push the estimate in the opposite direction.[29] Finally, the estimates in column (5) again indicate no effects of SSI on housing wealth. To this point, then, the evidence points to dissaving effects of SSI as older individuals approach the age of eligibility, although the evidence is strongest and most compelling when we look directly at saving data as the dependent variable.[30]

Table 5 provides additional assessment of the robustness of these findings. Columns (1)-(3) focus on what we regard as perhaps the most important assumptions or specification decisions underlying the estimated effects of SSI, namely the definition of the treatment or control sample. First, we redefine the treatment sample by using a lower threshold for the probability of participating in SSI, specifically using the 75th rather than the 90th centile as the cutoff. The prediction here is actually ambiguous. On the one hand, those with a lower probability of

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[29]We report average annual saving/dissaving for comparability to Table 3.

[30]If among those individuals who are likely participants, those who save relatively less face higher mortality, there is a bias against finding that high SSI benefits reduce saving, as only the higher savers among the likely participants survive (see Jianakoplos, et al., 1989, and Menchik, 1993).

participation are less likely to pay attention to the incentives created by SSI, since they are less likely to be eligible. On the other hand, they may have more dissaving to do to become eligible. Despite this ambiguity, these results help to assess the robustness of the findings. Second, we redefine the control sample to exclude those most dissimilar to the likely participants, dropping those whose predicted probability of SSI participation is below the 25th centile. This may help to reduce the influence on the difference-in-difference estimates of observations with relatively high wealth, although the use of median regression should render isolated observations non-influential. Finally, to take this concern one step further, we redefine the control sample to be likely participants aged 50-59, rather than unlikely participants aged 60-64. It is possible that relatively likely, but younger participants are a more appropriate standard of comparison than relatively unlikely, but older participants. On the other hand, it is possible that individuals respond to SSI incentives in their 50's, in which case the estimates can be interpreted as the change in behavior that ensues in the 60's.

Regardless of which of these alternative treatment or control samples we use, the results continue to indicate dissaving effects of SSI when we look at saving directly, or when we infer effects on saving from changes in wealth. Furthermore, when we use the 50-59 year-old, likely participant control sample, we also find statistically significant evidence that higher SSI benefits are associated with lower wealth holdings at ages 60-64, or equivalently with sharper reductions of wealth from ages 50-59 to ages 60-64, although again the negative estimate comes from the unlikely participants. This, of course, parallels the evidence in the other columns that higher SSI benefits result in sharper reductions in wealth over the 60's.

Finally, the estimates in column (4) of Table 5 report results from our main specification

for the sample of female householders.[31] The saving results are similar to those for men, indicating a statistically significant dissaving effect. For this sample, as well, a sizable proportion (.38) of the difference-in-difference estimate of the effect on saving comes from the treatment sample. For women, as for men, we find a negative effect of SSI benefits on the level of wealth held at ages 60-64, although this estimate is insignificant; however, in contrast to men, for women the effect comes from the comparison between likely participants. Finally, for women the wealth data do not yield evidence of wealth declines from ages 60-62 to ages 63-64, although, as indicated by the much larger standard error, the sample is too small to make any precise statement. Thus, in general, the results for women mimic those for men, especially for the direct measure of saving.[32]

## IV. Conclusion

The goal of this paper is to draw inferences about the effect of SSI for the aged on pre-retirement saving. SSI for the aged effectively operates as a means-tested retirement income program, in that eligibility depends on financial resources in the form of assets and income. As a consequence, more generous SSI benefits may induce less saving or more dissaving at ages near retirement. We use state-level variation in generosity of supplemental SSI payments to identify the effects of SSI, studying primarily a sample of male householders. We find relatively consistent evidence that SSI reduces the saving of men nearing the age of retirement, which is the anticipated effect of a means-tested retirement program. We also find similar evidence for female householders. For both men and women, the evidence is stronger when we look at direct data on saving, rather than inferring saving effects from cross-cohort differences in wealth. While the

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[31]The SSI participation probit was also reestimated for this sample; the estimated effects were similar to those for men.

[32]We also find no effect of SSI on changes in housing wealth, paralleling the findings for men.

overall findings are consistent with dissaving effects of SSI, we think that more definitive policy conclusions require further replication and analysis with these and other data sources, especially given the problematic nature of measuring wealth (Smith, 1995).

In addition to providing direct evidence on the effects of the SSI program, this evidence may also bear on proposals to means test Social Security. For example, a means-tested program might act as a "safety net" or "bottom tier" of a multi-tier retirement system in conjuction with a defined contribution public system. Our research tends to suggest that a means-tested retirement income program could have detrimental effects on private saving among part of the population. We think that one must be very cautious in applying our findings to the Social Security reform debate, because the SSI program serves a poor population with relatively low lifetime labor market attachment, whereas Social Security, even if means tested, would likely continue to serve a higher-income population with greater labor market attachment. Nonetheless, our findings point to the potential usefulness of proposals such as a "demogrant" (Mitchell and Zeldes, 1996), which provides a fixed, guaranteed (but small) public pension payment that is not means tested, as part of Social Security privatization.

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Table 1: Descriptive Statistics and SSI Participation Probit Estimates, Male Household Heads

| | | Age 65 and Over | | Age 65 and Over |
|---|---|---|---|---|
| | | SSI | SSI | SSI Participation |
| | Age 60-64 | Participants | Non-participants | Probits |
| | (1) | (2) | (3) | (4) |
| Saving (Δnet wealth excluding housing) | | | | |
| Mean | 11472.2 | -1094.4 | 7944.9 | ... |
| | (153132.5) | (26499.6) | (185057.8) | |
| Median | 1400 | 0 | 207 | ... |
| Total net wealth excluding housing | | | | |
| Mean | 85481.9 | 1340.8 | 76417.6 | ... |
| | (229127.1) | (32231.4) | (217076.0) | |
| Median | 21520 | 0 | 24000 | ... |
| 25th centile | 767 | 0 | 2080 | ... |
| State supplements SSI benefits | .56 | .43 | .54 | ... |
| Maximum state SSI supplement > 20% of federal benefit | .19 | .25 | .20 | ... |
| Maximum SSI benefits, individual | 363.9 | 372.4 | 364.9 | |
| | (56.0) | (71.2) | (58.0) | .007 |
| | | | | (.002) |
| Maximum SSI benefits, couple | 561.8 | 595.3 | 563.0 | |
| | (131.7) | (178.3) | (134.1) | |
| Less than high school | .36 | .88 | .50 | .04 |
| | | | | (.01) |
| High school graduate | .30 | .08 | .26 | ... |
| Some college | .16 | .02 | .10 | -.00 |
| | | | | (.02) |
| College graduate | .18 | .01 | .13 | -.02 |
| | | | | (.02) |
| Black | .07 | .23 | .06 | .026 |
| | | | | (.009) |
| Never married | .03 | .11 | .04 | .04 |
| | | | | (.01) |
| Divorced/widowed/ separated/spouse absent | .12 | .27 | .18 | .01 |
| | | | | (.01) |
| Ever authorized for food stamps | .05 | .31 | .03 | .025 |
| | | | | (.014) |
| Years authorized for food stamps | .07 | 1.65 | .06 | .012 |
| | (.64) | (3.56) | (.54) | (.003) |
| N | 675 | 98 | 1886 | 1984 |

In columns (1)-(3) unless otherwise noted means are reported with standard deviations in parentheses. Data are from waves 4, 5, and 7 of the 1984 SIPP panel. Saving is measured from wave 4 to wave 7, and wealth from wave 7. There are somewhat fewer observations for saving than for wealth; the latter are reported in the last row. Maximum SSI benefit is combined federal and state benefit, obtained from the 1985 Green Book, and is based on current marital status; see notes to Appendix Table A1 for details. In column (4) partial derivatives of the participation probability are reported, with standard errors in parentheses. The effect of a $100 change in monthly benefits is reported. All estimates are weighted.

Table 2: Descriptive Statistics and Crude Difference-in-Difference Estimates of Effects of SSI, Male Household Heads Aged 60-64

*A. Descriptive Statistics*

| | Overall | Likely participants | Unlikely participants |
|---|---|---|---|
| *Saving, Ages 60-64* | | | |
| 25th centile | -4406 | -500 | -5199 |
| Median | 1400 | 15 | 1850 |
| 75th centile | 18050 | 3700 | 19800 |
| *Wealth, Ages 60-64* | | | |
| 25th centile | 767 | -160 | 2601 |
| Median | 21520 | 0 | 26860 |
| 75th centile | 73600 | 12299 | 79804 |
| Proportion > 0 | .81 | .45 | .84 |
| *Wealth, Ages 60-62* | | | |
| 25th centile | 300 | -233 | 1050 |
| Median | 17998 | 0 | 22099 |
| 75th centile | 67599 | 12299 | 69794 |
| Proportion > 0 | .78 | .41 | .83 |
| *Wealth, Ages 63-64* | | | |
| 25th centile | 2500 | -160 | 5200 |
| Median | 31400 | 2 | 35420 |
| 75th centile | 100800 | 4999 | 59999 |
| Proportion > 0 | .84 | .52 | .87 |

*B. Difference-in-Difference Estimates of Effects of SSI, Using Medians*

| | | Likely participants | | Unlikely participants | | |
|---|---|---|---|---|---|---|
| | | States with SSI supplement | States without SSI supplement | States with SSI supplement | States without SSI supplement | Difference-in-difference estimate |
| *Saving, Ages 60-64* | | 95 | 0 | 3223 | 732 | -2396 |
| | N: | (35) | (29) | (320) | (291) | |
| *Wealth, Ages 60-64* | | 2 | 0 | 34443 | 17998 | -16443 |
| | N: | (38) | (30) | (330) | (314) | |

"Likely participant" indicates observations with predicted probability exceeding the 90th centile (.0663) of the weighted distribution of estimated probabilities of SSI participation, based on the probit estimates in Table 1. All estimates are weighted.

Table 3: Difference-in-Difference Estimates of Effect of SSI on Saving of Likely Participants, Male Household Heads Aged 60-64, Median Regressions

| | No control variables | Drop those with work-impairing disability | Add demographic controls | Add California controls | Housing wealth, robust regression |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Likely participant | -19.56** | -33.68** | -30.39** | -30.15 | 7.45 |
| × state supplement | (5.97) | (13.11) | (13.59) | (24.19) | (28.18) |
| Pseudo $R^2$ | .003 | .002 | .004 | .005 | ... |
| N | 675 | 520 | 520 | 520 | 520 |
| Joint significance of new controls (p-value) | ... | ... | .03 | .48 | ... |
| Decomposition of difference-in-difference estimate: | | | | | |
| a. Likely participant, high supplement state - likely participant, no supplement state | -2 | -127 | -186 | -97 | ... |
| b. Unlikely participant, high supplement state - unlikely participant, no supplement state (control sample) | 161 | 153 | 67 | 154 | ... |
| Difference-in-difference estimate (a. - b.) | -163 | -281 | -253 | -251 | ... |
| Proportion from treatment group | .01 | .45 | .73 | .39 | ... |

"State supplement" is the dollar value of the maximum state supplement. "Likely participant" indicates observations with predicted probability exceeding the 90th centile (.0663) of the weighted distribution of estimated probabilities of SSI participation, based on the probit estimates in Table 1. All specifications include the state supplement variable and a dummy variable for likely participants. Demographic controls for education, race, and marital status, in the same form as in the probits in Table 1, are included beginning in column (3). "California controls" include a dummy variable and interaction with "likely participant." Estimates significant at the five-percent level are denoted with a '**', and those at the ten-percent level with a '*'. All estimates are weighted except in column (5). Decompositions are evaluated for $100 increases in yearly state supplemental SSI benefits.

Table 4: Difference-in-Difference Estimates of Effect of SSI on Wealth of Likely Participants, Male Household Heads Aged 60-64, Median Regressions

*A. Levels Specification*

| | No control variables | Drop those with work-impairing disability | Add demographic controls | Add California controls | Housing wealth, robust regression |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Likely participant × state supplement | -61.71** (29.52) | 52.95 (54.80) | -60.83 (58.41) | -62.82 (114.84) | 61.51 (122.03) |
| Pseudo $R^2$ | .015 | .010 | .032 | .032 | .087 |
| N | 712 | 551 | 551 | 551 | 551 |
| Joint significance of new controls (p-value) | ... | ... | .00 | .83 | ... |
| Decomposition of difference-in-difference estimate: | | | | | |
| a. Likely participants | 1 | ... | 6 | 6 | ... |
| b. Unlikely participants | 516 | ... | 513 | 530 | ... |
| Difference-in-difference estimate (a. - b.) | -514 | ... | -507 | -523 | ... |
| Proportion from treatment group | <0 | ... | <0 | <0 | ... |

*B. Change from Ages 60-62 to Ages 63-64*

| | | | | | Median regression |
|---|---|---|---|---|---|
| Likely participant × state supplement × (age 63 or 64) | -199.26** (76.34) | -208.67 (129.03) | -138.84 (90.76) | -183.55** (93.04) | -94.97 (82.04) |
| Pseudo $R^2$ | .024 | .020 | .039 | .044 | .093 |
| N | 712 | 551 | 551 | 551 | 551 |
| Joint significance of new controls (p-value) | ... | ... | .00 | .00 | ... |
| Decomposition of difference-in-difference estimate: | | | | | |
| *Average annual saving:* | | | | | |
| a. Likely participants | 236 | 181 | 307 | -993 | ... |
| b. Unlikely participants | 900 | 877 | 769 | -381 | ... |
| Difference-in-difference estimate (a. - b.) | -664 | -696 | -462 | -611 | ... |
| Proportion from treatment group | <0 | <0 | <0 | >1 | ... |

The specifications in Panel B also include a dummy variable for age equal to 63 or 64, and interactions of this dummy variable with the state supplement and with a dummy variable for likely participants. "California controls" include a dummy variable and interaction with "likely participant" in Panel A, as well as an interaction with the age 63-64 dummy variable in Panel B. In Panel B, average annual saving is computed by dividing the change in wealth by 2.5, the average number of years between those aged 60-62 and those aged 63-64. Estimates significant at the five-percent level are denoted with a '**', and those at the ten-percent level with a '*'. All estimates are weighted. See notes to Table 3 for additional details.

Table 5: Robustness Analysis of Difference-in-Difference Estimates of Effects of SSI on Saving and Wealth of Likely Participants, Male Household Heads Aged 60-64, Median Regressions

| | Treatment sample: Predicted probability of SSI participation above 75th centile | Control sample: Predicted probability of SSI participation above 25th centile | Control sample: Likely participants aged 50-59 | Female household heads aged 60-64, original definitions of control and treatment samples |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *A. Savings* | | | | |
| Likely participant × state supplement | -22.61* | -37.24** | -16.76** | -22.96** |
| | (12.97) | (11.32) | (5.75) | (9.12) |
| N | 520 | 342 | 144 | 239 |
| CA controls (p-value) | .60 | .53 | .80 | .62 |
| Decomposition of difference-in-difference estimate: | | | | |
| a. Likely participants | -121 | -186 | -137 | -73 |
| b. Unlikely participants | 67 | 124 | 3 | 118 |
| Difference-in-difference estimate (a. - b.) | -188 | -310 | -140 | -191 |
| Proportion from treatment group | .64 | .60 | .98 | .38 |
| *B. Wealth* | | | | |
| Likely participant × state supplement | -39.32 | -52.80 | -35.73** | -38.33 |
| | (49.44) | (48.46) | (10.95) | (33.49) |
| N | 551 | 357 | 152 | 251 |
| CA controls (p-value) | .72 | .93 | .00 | .80 |
| Decomposition of difference-in-difference estimate: | | | | |
| a. Likely participants | 0 | 47 | 18 | -324 |
| b. Unlikely participants | 328 | 487 | 316 | -4 |
| Difference-in-difference estimate (a. - b.) | -328 | -440 | -298 | -319 |
| Proportion from treatment group | <0 | <0 | <0 | >1 |
| *C. Changes in Wealth from Ages 60-62 to Ages 63-64* | | | | |
| Likely participant × state supplement × (age 63 or 64) | -260.97** | -201.03** | ... | -166.00 |
| | (130.27) | (101.65) | | (347.09) |
| N | 551 | 357 | ... | 251 |
| CA controls (p-value) | .53 | .05 | ... | .03 |
| Decomposition of difference-in-difference estimate: | | | | |
| *Average annual saving:* | | | | |
| a. Likely participants | 104 | -1648 | ... | 145 |
| b. Unlikely participants | 1898 | 27 | ... | 698 |
| Difference-in-difference estimate (a. - b.) | -1794 | -1675 | ... | -553 |
| Proportion from treatment group | <0 | .98 | ... | <0 |

Specifications correspond to column (3) or (4) of Tables 3 and 4, with the California controls included when the p-value was .1 or less. In column (3) the "likely participant" variable included in the interactions refers to a dummy variable for age 60-64, rather than for a high probability of SSI participation, since the entire sample used for this specification consists of those with a high probability of participation. See notes to Tables 3 and 4 for details. Estimates significant at the five-percent level are denoted with a '**', and those at the ten-percent level with a '*'. All estimates are weighted.

Appendix Table A1: State SSI Supplemental Maximum Benefits, 1985

| State | Individuals | Couples | ≥ 20% of Federal Benefit |
|---|---|---|---|
| Alabama | 0 | 0 | No |
| Arizona | 0 | 0 | No |
| Arkansas | 0 | 0 | No |
| California | 179 | 448 | Yes |
| Colorado | 58 | 278 | Yes |
| Connecticut | 172 | 119 | Yes |
| Delaware | 0 | 0 | No |
| Washington, D.C. | 15 | 30 | No |
| Florida | 0 | 0 | No |
| Georgia | 0 | 0 | No |
| Hawaii | 5 | 9 | No |
| Illinois | 35 | 34 | No |
| Indiana | 0 | 0 | No |
| Iowa | 0 | 0 | No |
| Kansas | 0 | 0 | No |
| Kentucky | 0 | 0 | No |
| Louisiana | 0 | 0 | No |
| Maine | 10 | 15 | No |
| Maryland | 0 | 0 | No |
| Massachusetts | 129 | 202 | Yes |
| Michigan | 27 | 40 | No |
| Minnesota | 35 | 66 | No |
| Missouri | 0 | 0 | No |
| Montana | 0 | 0 | No |
| Nebraska | 61 | 89 | No |
| Nevada | 37 | 74 | No |
| New Hampshire | 27 | 21 | No |
| New Jersey | 31 | 25 | No |
| New York | 61 | 76 | No |
| North Carolina | 0 | 0 | No |
| North Dakota | 0 | 0 | No |
| Ohio | 0 | 0 | No |
| Oklahoma | 60 | 120 | Yes |
| Oregon | 2 | 0 | No |
| Pennsylvania | 32 | 49 | No |
| Rhode Island | 54 | 102 | Yes |
| South Carolina | 0 | 0 | No |
| Tennessee | 0 | 0 | No |
| Texas | 0 | 0 | No |
| Utah | 10 | 20 | No |
| Vermont | 53 | 97 | No |
| Virginia | 0 | 0 | No |
| Washington | 38 | 37 | No |
| Wisconsin | 100 | 161 | Yes |

List is restricted to states individually identified in the SIPP. The maximum federal benefits were $325 for individuals, and $488 for couples. Figures were taken from the 1985 Green Book. 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.