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David Neumark
Elizabeth T. Powers

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David Neumark
Public Policy Institute of California,
NBER and IZA Bonn

Elizabeth T. Powers
University of Illinois at Urbana-Champaign

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
Email: iza@iza.org

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ABSTRACT

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The Supplemental Security Income (SSI) program in the United States creates incentives for potential aged recipients to reduce labor supply prior to becoming eligible, and our past research finds that older men likely to be eligible for SSI at age 65 reduce their labor supply in the years immediately before the age of eligibility. However, given the dramatic supplementation of SSI benefits in some states, a migration response to these benefits cannot be dismissed, and migration that is associated with SSI benefits can lead to bias in estimates of the effects of SSI benefits on labor supply; depending on retirement and migration behavior, the disincentive effects can be overstated or understated. Migration responses to SSI benefits are also important in their own right, as another instance of the potential problem of "welfare magnets." We fail to find any statistically significant evidence that older individuals likely to be eligible for SSI in the near future, or already eligible for SSI, are more likely to move from low benefit to high benefit states. These findings are robust to the use of a number of different comparison groups to try to capture the state-to-state migration patterns that exist independently of a response to SSI. The evidence indicates that labor supply disincentive effects of SSI do not stem from migration behavior that could, in principle, spuriously generate these findings.

JEL Classification: J14, J22, I38, R23

Keywords: SSI, labor supply, migration

Corresponding author:

David Neumark
Public Policy Institute of California
500 Washington St., Suite 800
San Francisco, CA 94111
USA
Email: neumark@ppic.org

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I. Introduction

The Supplemental Security Income (SSI) program provides a safety net for the elderly, blind, and disabled. The federal program sets its own maximum benefit levels, and many states supplement these benefits, in some cases substantially. The SSI program is means-tested and imposes both income and asset limits. As such, those on the margin of eligibility for the elderly component of the program—which is the focus of our paper—face incentives to reduce labor supply (or earnings) prior to becoming eligible. Our past research has consistently found evidence that older men likely to be eligible for SSI at age 65 reduce their labor supply in the years immediately before the age of eligibility (Neumark and Powers, 2000, 2003/2004, and 2005).[1,2] This research on the effects of means-tested income support programs on labor supply of the elderly complements a much wider body of research on similar effects of welfare programs for younger individuals and families (e.g., Moffitt, 1992; Blank, 2002).

In this paper, we consider another question concerning the effects of the SSI program that also follows naturally from a similar question asked with regard to other welfare programs. Specifically, for the first time we take up the question of migration responses to SSI. The migration question is of interest in its own right, as "welfare magnets" are an important issue in the welfare literature (e.g., Moffitt, 1992; Blank, 1988). In addition, the migration question is of interest because migration that responds to SSI benefits, and is potentially related to employment, might affect the estimation of the effects of SSI on labor supply.

Our previous research has studied the effects of the SSI program on the labor supply of older men as they approach age 65—the SSI eligibility age for the aged program—using a variety of data sets including the Survey of Income and Program Participation (SIPP), the Current Population Survey (CPS), and social security administrative data. The evidence indicates that among those likely to be eligible for the program, and hence likely to be responsive to the income and asset limits for eligibility, higher state SSI supplements are associated with lower labor supply. The causal mechanism that we presume to

[1] In contrast, these incentives are far less likely to play a role for participants in the disabled and especially the blind components of the program, which we generally ignore in this paper.
[2] Both the asset and income limits also create incentives to reduce saving near the age of eligibility; evidence that SSI also leads to dissaving among likely eligibles approaching age 65 is reported in Neumark and Powers (1998).

underlie this association is that higher state supplements make SSI more attractive, and therefore create stronger incentives to adjust labor supply so as to be eligible for SSI. In addition, we have shown that the eligibility for early social security retirement benefits at age 62 enhances the labor supply disincentives of SSI beginning at age 62; at this age, potential future SSI recipients can replace income lost from reducing labor supply by taking early social security retirement. This occurs without any long-lasting early retirement "penalty," since this penalty is negated once the household gets on SSI at age 65 (Powers and Neumark, 2003 and 2005). Correspondingly, our work on labor supply finds evidence of the sharpest labor supply effects for 62-64 year-olds.

A key assumption in this past work is that state of residence—which is critical because it determines the SSI benefit an individual faces—is treated as fixed. We have ignored the possibility that individuals migrate in part in response to SSI benefits. Given the dramatic supplementation of SSI benefits in some states, a migration response to these benefits cannot be dismissed. For example, as of January 2000, the maximum federal benefit was $512 for individuals and $769 for couples (both individuals aged 65 or over). In that same year the maximum individual (couple) benefit—including both federal and state benefits—was $874 ($1,297) in Alaska, $747 ($1,094) in Connecticut, and $692 ($1,229) in California. Especially for older individuals—for whom migration may be more likely around the time of retirement—migration that is associated with SSI benefits can lead to bias in estimates of the effects of SSI benefits on labor supply; depending on retirement and migration behavior, the disincentive effects can be overstated or understated, as explained later.

The issue of the responsiveness of migration to SSI benefits is also important in its own right, without reference to its implications for bias in the estimates of labor supply disincentive effects. In the welfare literature, there is a long-standing concern that potential program eligibles migrate to states with higher benefits (Moffitt, 1992, provides a review, and newer contributions are Enchautegui, 1997; Levine and Zimmerman, 1999; and Meyer, 2000). The possibility of this migration response is potentially important politically. A high response makes generous benefits less attractive to policy makers, by increasing the costs and diffusing the benefits to those who, at least initially, are not state residents.[3] At its worst, a perceived high migration response on the part of policymakers can set off what has been termed a “race to the bottom” as states cut benefits in the hope that other states will pick up the burden of income support for the poor (e.g., Brueckner, 2000; Figlio, et al., 1999). Similar issues can arise, of course, with respect to migration across international borders, depending on the eligibility of international immigrants for government benefits in the host country.

To study the migration response to SSI and its implications for labor supply estimates, in this paper we use data from the 1980, 1990, and 2000 Decennial Censuses of Population, with which we can study both labor supply and migration. Changes in SSI benefits over time, and more importantly variation in state supplements to SSI benefits, can be used to infer the effects of SSI on behavior of those nearing eligibility for the program, or those already eligible for the program. We first briefly use the data to replicate our earlier analyses of the disincentive effects of SSI on labor supply. Then, because the Census data include information on inter-state moves in the previous five years, we examine the relationships between SSI benefits and migration and whether they lead to biased estimates of the effects of the SSI program on labor supply.

II. The SSI Program

The SSI program was enacted by the U.S. Congress in 1972 and begun in 1974. Prior to that year, three separate programs—established in the original Social Security Act of 1935 and its 1950 amendments—provided means-tested assistance for individuals who were either ineligible for social security or whose social security benefits were inadequate: Old-Age Assistance, Aid to the Blind, and Aid to the Permanently and Totally Disabled. Federal law established only broad guidelines for these three programs, and states had great flexibility in setting program rules and benefit levels (including assessing needs on a case-by-case basis). SSI was established to provide a uniform income floor and common eligibility requirements, while states were free to supplement SSI payments subject to their own eligibility

[3] This concern led some states to try to offer lower welfare benefits or tighten time limits for individuals who recently moved into a state, but such efforts were declared unconstitutional (under the 14th amendment to the U.S. Constitution) in 1999 in the Saenz v. Roe decision (Meyer, 2000).

rules, with minor exceptions noted below. The SSI program comprises a substantial potential source of income for the elderly poor. Federal SSI, when combined with Food Stamps, brings an elderly household's resources close to the federal poverty line.

States can choose an optional benefit supplement. They can administer the supplemental program themselves, or choose to have the Social Security Administration administer their supplemental programs as long as they maintain the same eligibility rules as the federal program (except that they are allowed to exclude additional items from income in determining eligibility for the state supplement). As already noted, the state supplementation can be quite dramatic. Table 1 lists the maximum state supplements and federal benefits for individuals and for couples (paid when both individuals are aged 65 or over), for the years covered by our Census data.[4]

SSI is a means-tested program. As a consequence, benefits are reduced by income from other sources, including social security, and by financial resources. Twenty dollars per month of unearned, non-transfer income, \$65 of earned income, and one-half of earnings exceeding \$65, are disregarded in computing the SSI benefit.[5] The disregards are not indexed for inflation, nor are they differentiated by household type (couple or individual).[6] The federal benefit is reduced by one-third for filing units living in the household of another, and states are free to vary supplements according to living arrangements. We do not consider differentiation in benefits by living arrangement (but we do differentiate benefits by filing unit type). In most cases, the monthly SSI benefit is determined by the formula:

[4] Aside from states simply choosing higher SSI supplements, Congress initially imposed mandatory supplements to ensure that in no state would citizens already in state programs receive lower benefits in the federal program than they had previously received under the state program. However, because these mandatory supplements did not apply to individuals first becoming eligible for SSI in 1974 or later, and we study behavior of individuals approaching the age of eligibility for SSI, we need not account for them. In addition, some constraints were imposed on states' ability to alter supplemental payments; in 1976 Congress mandated that states pass along cost-of-living increases in federal benefits (to avoid states cutting their benefits to offset these increases). See U.S. Social Security Administration (2002) and U.S. Committee on Ways and Means (2000).

[5] In addition, there are exclusions for certain home energy and support and maintenance assistance, Food Stamps, most federally-funded housing assistance, state assistance based on need, one-third of child support payments, and income received infrequently or irregularly.

[6] While some states vary their disregard amounts from the federal level, it proved difficult to incorporate this information given the idiosyncratic ways in which different disregards are applied and the detailed knowledge about income sources needed to assign them appropriately.

$$(1)\quad \text{SSI benefit} = \text{Guarantee} - \tfrac{1}{2}\,\text{Max}\{\text{earned income} - \text{Min}\{\text{earned income}, \$65\}, 0\}$$
$$- \text{Max}\{\text{unearned income} - \text{Min}\{\text{unearned income}, \$20\}, 0\}$$
$$- \{\text{means-tested transfer income}\}.$$

The guarantee is the benefit amount paid when there is no other income. Earned income refers to the current earnings of the SSI receiving unit. Unearned income includes income from private pensions, public pensions such as social security, interest income, and the like. Means-tested transfer income (e.g., Veterans Benefits) offsets SSI income dollar-for-dollar and none of it is disregarded. These deductions for other income are first applied to the federal benefit amount. When the computed SSI benefit is positive, the filing unit is eligible for the federal program. If there is any excess income, it is deducted from the state supplemental payment (U.S. Social Security Administration, 1994, pp. ii-iii), and the unit only receives a state benefit.

Finally, a limitation on financial resources is imposed via an asset limit, above which the individual or couple is ineligible for any benefit. As of 2000 the federal limits were $2,000 for individuals and $3,000 for couples.[7] As noted above, when states administer their own programs they have flexibility to set their own limits on and exclusions for income and assets. In practice few states have different rules, and when they do the differences are typically minor (U.S. Social Security Administration, January 2000).

The SSI program is large. In December 2000 there were 1.3 million recipients in the aged component of the program (overall, 2 million SSI recipients are aged 65 and over). About half of the 1.3 million aged-program recipients receive a federal benefit only and about half receive a positive state supplement. Total payments for the aged program in 2000 were approximately $4.8 billion (overall SSI spending was $31.6 billion), about 75 percent of which was in the form of federal benefits, and the remainder state supplements (U.S. Social Security Administration, 2001).

[7] The following assets are excluded from the limit: the prospective recipient's car, home, and the land the home is on; life insurance policies with a face value of up to $1,500; burial plots for the individual and immediate family members; and up to $1,500 in burial funds for the individual and his or her spouse.

III. Labor Supply

Disincentive Effects of SSI

We have fully developed the theoretical framework demonstrating why SSI creates disincentives for labor supply elsewhere (Neumark and Powers, 2000), so here we present only an overview. We focus on a simple two-period model in which agents make their labor supply decision in the first period and retire (and potentially become eligible for SSI) in the second, and there is no saving. Briefly, SSI generosity reduces work effort both infra- and infer-marginally. First, consider the infra-marginal effects of a small increase in the SSI guarantee on the agent's optimal choice. If he will be a non-participant, his work effort is unaffected by SSI benefit generosity. If he will participate in SSI, there are two cases: either his private income will exceed the SSI disregard amount, or it will not. If his non-SSI retirement income will be below the disregard amount, additional work leads to both increased first period consumption and second period (retirement) income, and one can readily show that under standard conditions, optimal work hours are decreasing in the guarantee. If income will exceed the disregard, then the only incentive to work is to maintain period one consumption, because additional private income in period two will be taxed away at a 100% rate by the SSI program. Since SSI cannot be received in the first period, the benefit G cannot influence the labor supply choice.

Because SSI creates a segmented budget constraint, one must also consider how changes in SSI generosity affect the labor supply choice infer-marginally. In Neumark and Powers (2000), we illustrate how an increase in the guarantee shifts both SSI-participation segments of the budget constraint out relative to the segment of the budget constraint for non-participants, encouraging SSI participation and reducing work. In addition, an increase in the guarantee widens the range of work hours that produce no additional retirement income due to SSI policy, further discouraging work.[8]

[8] When saving is allowed and SSI also has an asset test, the arguments developed above imply that saving to increase asset income post-retirement will be discouraged. Clearly if asset tests discourage saving they will also discourage the additional work needed to raise the stock of assets that would normally be drawn down during retirement. Moreover, assets that are drawn down prior to retirement can be used to offset reduced labor supply. Thus, with saving in the model the disemployment incentives of SSI are likely enhanced.

Empirical Approach

Our past research (Neumark and Powers, 2000, 2003/2004, and 2005) has focused on estimating the labor supply disincentive effects of SSI. Details are provided in those papers (with the most recent one paralleling most closely what we do here). Here we only describe our approach briefly, in sufficient detail to make clear how migration might affect the labor supply estimates.

Our equation of interest models pre-eligibility labor supply as a function of SSI benefits available upon retirement (at age 65). But we are also interested in isolating the effect of benefits for those likely to be eligible for SSI—and therefore potentially responsive to SSI benefit levels. Because we estimate models for labor supply prior to eligibility for SSI, we do not observe actual SSI participation for those for whom we want to study labor supply, but instead can only predict those likely to participate. We do this by estimating probit models for SSI participation for men aged at least 65, and using the estimates coupled with characteristics of those under age 65 to assign a value of the likelihood of future SSI participation, categorizing those with a predicted probability of participation above a particular cutoff as "likely participants," and the others as "unlikely participants."[9]

In estimating the effects of SSI supplements on pre-eligibility labor supply, we use the real maximum monthly federal plus state SSI benefit for individuals or couples. We focus on individuals aged 62-64, for three reasons. First, given stochastic influences on earnings and wealth, older workers can form better predictions of post-retirement income. Second, we suspect that workers pay more attention to the potential receipt of SSI benefits as they approach the eligibility age. And third, the eligibility for early social security retirement benefits at age 62 likely enhances the labor supply disincentives of SSI beginning at age 62 (Powers and Neumark, 2003 and 2005), consistent with our earlier findings that labor supply disincentive effects were sharpest for 62-64 year-olds (Neumark and Powers, 2000).

We could begin with a simple difference estimator implemented for 62-64 year-old likely participants, relating a labor supply measure (Y), to the combined federal and state supplement (SSI), and

[9] Research in the welfare literature has categorized individuals into treatment or control groups based on a single exogenous characteristic (e.g., family structure or education). Our framework can be viewed as broadening this approach to allow a multitude of exogenous factors to determine control and treatment group categorizations.

a vector of control variables denoted X (including education, race, marital status, a dummy variable indicating that spouse is younger than the other by more than three years, the state unemployment rate in the corresponding year, and year dummy variables). However, because there may be unmeasured sources of labor supply differences across states that are correlated with SSI, we use difference-in-differences (DD) and difference-in-difference-in-differences (DDD) estimators that introduce control groups to capture this state variation in labor supply. One potentially important source of variation in labor supply associated with but not caused by SSI variation is other state policies that might affect labor supply (or migration) and that might be correlated with SSI generosity.

Our first approach is to use data on younger individuals in the same state who are also likely participants, assuming that the state-level labor supply differences—perhaps stemming from variation in other policies—are common to likely participants across age groups. In particular, we use a DD estimator that identifies the effect of SSI on labor supply from the difference between the coefficient of SSI for 62-64 and 60-61 year-olds. (We denote this estimator DD-CS, because it relies on cross-sectional variation in SSI benefits.) This estimator, though, will yield misleading estimates if the slope of the age profile of labor supply is different in states with higher supplements, incorrectly attributing this difference to an effect of SSI benefits. One way to net out differences in age profiles of labor supply is to introduce unlikely participants, assuming that they have the same age profile of labor supply as the likely participants in the same state but are unresponsive to SSI benefits, and using a DDD estimator (denoted DDD-CS). In this case, we use the sample of all 60-64 year-olds and identify the effect of SSI from the extent to which the difference in Y between 62-64 year-old and 60-61 year-old likely participants, relative to the difference between 62-64 year-old and 60-61 year-old unlikely participants, varies with the state SSI benefit.

A potential limitation of this DDD estimator concerns the validity of using unlikely participants to control for variation in the age profile of labor supply for the likely participants, as the unlikely participants may be quite different from likely participants, and also quite heterogeneous themselves. With repeated cross-sections with variation in state-level benefits across time, however, we can avoid this, constructing a DD estimator that relies only on using 62-64 year-old likely participants—but in different years—as controls. To do this, we augment our model to include state dummy variables, and re-estimate the model for 62-64 year-old likely participants. In this case, we identify the effects of SSI from the differences in labor supply changes over time across states with different changes in SSI benefit generosity. We prefer this DD estimator because it uses observations on exactly the same types of individuals—likely participants aged 62-64—to obtain a control group. (We denote this estimator DD-TS, because it relies on time-series variation in SSI benefits.) Because this estimator does not use younger likely participants as a control group, estimated labor supply effects of SSI are not contaminated by possible differences across states in age profiles of labor supply.

Nonetheless, there is the possibility of changes over time in labor supply that are spuriously correlated with time-series changes in benefit generosity—perhaps because of other policy changes. In that case, we again want to introduce a control group that exhibits the same time-series changes in labor supply as the older likely participants, but for which these changes are attributable to factors other than SSI. Paralleling the earlier discussion, we can use younger likely participants in the same state, implementing a DDD estimator (DDD-TS) that uses 60-61 year-old likely participants as well, identifying the effects of SSI from the difference in the change in labor supply of older versus younger likely participants associated with changes in SSI benefits. An important advantage of this estimator relative to the DDD-CS estimator is that it does not rely at all on using data on unlikely participants, a group which, as we noted above, may not be an ideal control group.

IV. Migration

Clearly, evidence on potential migration responses to state variation in SSI benefits directly informs the "welfare magnet" debate. In addition, endogenous migration decisions may impact the estimation of labor supply disincentive effects of SSI. In particular, all of the estimators described in Section III assign individuals the maximum SSI supplement in the state in which they currently reside. However, given that individuals can migrate from state to state, estimation based on this assignment may be biased if migration is associated with changes in maximum SSI benefits.

Implications of Migration for Estimation of Labor Supply Disincentive Effects

There are two principal channels by which migration can lead to biased estimates of labor supply disincentive effects of SSI. First, consider migration after reaching the age of eligibility for SSI. In particular, suppose that there is some tendency for individuals to move to higher benefit states if they go on SSI. Assuming that these moves are in large part anticipated, so that agents are responding to benefit levels in their destination state, then when we estimate a model for labor supply as a function of SSI benefits, we are using the wrong SSI benefits measure, as it is the benefit in the destination state a few years later to which the individual should be responding. As suggested above, the most likely kind of mobility we would expect to be associated with SSI benefits is moves of likely participants from states with low supplements to states with high supplements. Individuals planning to make such a move may have a strong incentive to reduce labor supply prior to the age of eligibility despite presently residing in a state in which the SSI supplement is zero, which would tend to bias the estimated disemployment effect of SSI toward zero. Thus, the mismeasurement of the SSI benefit to which some individuals respond in making their labor supply decisions suggests that post-eligibility migration likely leads to understatement of the labor supply disincentives of SSI, hence strengthening the evidence we do find of such effects.[10] Regardless, though, whether or not migration of those likely to be eligible for SSI is sensitive to SSI benefit levels is of direct interest.

A second type of migration pattern is potentially more problematic with regard to our past evidence from estimating labor supply effects of SSI. Specifically, especially among those with lower incomes and hence more likely to be eligible for SSI, retirement prior to the age of eligibility for SSI is common, more so in light of the incentives to take early social security retirement for those who will go on SSI. Among those who retire before age 65, some may move to states with higher SSI benefits in preparation for receiving the higher benefits at age 65. This is different from the previous case, because

[10] The same conclusion holds if individuals going on SSI tend to move to low benefit states, because then their labor supply does not respond to the higher SSI benefit in their state of residence. Thus, this case can be interpreted as paralleling classical measurement error, where we measure with error the relevant SSI benefit level to which individuals respond, using the current benefit level instead of that in the destination state. Of course the reality is more complicated than classical measurement error, both because many of the estimators use the supplement in interactions as well as main effects, and because there is a discrete number of possible values that the change in benefits can take.

now we accurately measure the SSI benefits to which individuals may be responding, given that they have already moved. For these individuals, though, an estimated negative relationship between SSI benefits and labor supply is generated not because the higher benefits reduced labor supply, but rather because of the joint determination of retirement and migration, with those who chose to retire—perhaps irrespective of SSI benefits—subsequently also choosing to migrate to high benefit states. Thus, this pattern of migration suggests bias in the direction of overstating the labor supply disincentive effects of SSI.

Estimating Migration Responses

From the point of view of the welfare magnet debate, we are interested in migration that could occur either before or after the age of eligibility for SSI, since either type of migration pattern could influence the number of beneficiaries a state might expect as a result of variation in their SSI supplement. Similarly, the preceding discussion indicates that evidence of pre- or post-eligibility migration is informative—in different ways—about how migration impacts estimates of the labor supply effects of SSI.

Our framework for estimating how migration responds to SSI benefit levels parallels that in the literature on migration and welfare benefits—most recently Gelbach (2004). In all cases, we estimate models for the change in benefits—most importantly isolating those changes associated with a move. Paralleling the labor supply estimators discussed earlier, we use specifications that identify the effect for likely participants in the age group of interest relative to a comparison group that might exhibit similar state-to-state migration patterns, but which is less likely to exhibit migration patterns in response to SSI benefits. We should emphasize that we are not, in this paper, conducting a completely general analysis of the potential impact of SSI benefits on migration, but are instead more focused on whether there is evidence for the types of migration patterns discussed above that might bias the estimated disincentive effects of SSI on labor supply.

The Census data from 1980, 1990, and 2000 that we use to study labor supply also have information on state of residence five years earlier, to which we match states' SSI benefit information for that period. We first focus on post-eligibility migration. To correspond most closely to the 62-64 year-olds on which we focus in estimating labor supply effects, we look at migration of current 67-69 year-olds, who are five years older.[11] We also study pre-eligibility migration directly, by estimating the equations described below substituting 62-64 year-olds for 67-69 year-olds; the logic of our estimators carries over completely. We begin by restricting attention to those initially living in states that do not supplement federal SSI benefits, and ask whether those most likely to participate in SSI exhibit a tendency to move to states with higher SSI benefits. Later, we look at those living in the most generous states, and ask whether these individuals tend to retain high benefits by choosing to stay where they are. Thus we look at migration from both perspectives, as a problem of whether to move and a problem of whether to stay.

Specifically, in the case of "whether to move," we look at those aged 67-69 in the Census year, and ask whether they have made migratory moves that increase their SSI benefits. We do this for samples including migrants and non-migrants, as well as samples of migrants only. The latter are most useful for trying to detect a migration response, whereas the former are more useful for trying to gauge whether there is any detectable migration response in the larger sample for which we estimate labor supply disincentive effects. As Gelbach (2004) points out, it is the estimates from the full sample that are of most interest in answering whether migration biases researchers' estimates of other behavioral responses to government benefits. On the other hand, since we pool data from multiple Census years, non-migrants can also be responding to SSI benefit levels, for example by choosing to remain in a state that has raised its benefits.

The simplest estimator we use is a difference-in-difference estimator estimated for 67-69 year-old and 60-61 year-old likely participants. The 60-61-year-olds serve as the control group, to capture

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[11] For this group, we can also identify actual SSI participants, rather than just likely participants. But paralleling the argument in Meyer (2000), if we condition on participation in year 't', then representation in the sample of participants may be influenced by migratory moves. For example, all else the same, those who moved to high benefit states are more likely to appear in the sample than those who remained in low benefit states, simply because more people are eligible for some SSI benefits when benefits are higher. That is, individuals who were initially in low benefit states and stayed in a low benefit state are underrepresented relative to those who were initially in low benefit states but moved to a high benefit state. Thus, conditioning on likely participation in a way that does not depend on SSI benefits—as we do—rather than on actual participation, avoids this endogenous sample selection. For the analysis of pre-eligibility migration below, this is not an issue, as our only option is to use likely participation.

common state-to-state migration patterns for older individuals or families that would lead to changes in benefits—such as general migration to California, which supplement benefits generously. But only if this pattern is stronger for 67-69 year-olds will this be interpreted as a migration response to SSI. We do not want to simply use those a couple of years younger than those aged 67-69 as of the Census year, as these individuals may have also already made a retirement-related move upon either reaching the age of eligibility for SSI (ages 65-66) or for early social security retirement (ages 62-64). Likely participants aged 60-61 are less likely to have made moves for either of these reasons, yet may still tend to capture the migration patterns exhibited by residents of the same state who are older and likely to participate in SSI.

Specifically, we define the dependent variable, $\Delta BEN_{ist,t\text{-}5}$, as the change in maximum benefits (individual or couple, depending on how the individual is classified) based on state of residence, between five years ago and the Census year. We estimate the specification:

(2) $\Delta BEN_{ist,t\text{-}5} = \zeta + \alpha \cdot OLD_{ist} + X_{ist}\psi + YEAR_{it}\rho + \varepsilon_{ist}$.

In this specification, α identifies whether there is a migration response to SSI benefits among 67-69 year-olds, because it measures whether there is a change in benefits that differs for 67-69 year-olds relative to 60-61 year-olds. We noted earlier that for non-migrants a change in benefits can arise because a state's benefits changed. However, these changes common to everyone in a state do not identify α because of the 60-61 year-old comparison group. Similarly, any changes in federal benefits between a pair of years will be captured in the year effects. We label this first difference-in-difference estimator DD1.

A potential problem with DD1 is that the age patterns of migration may be different for those aged 67-69 and those aged 60-61 for reasons unrelated to SSI. One way to address this possibility is to use an alternative difference-in-differences estimator that uses unlikely participants of the same age (67-69) as the comparison group. In this case the equation we estimate is:

(3) $\Delta BEN_{ist,t\text{-}5} = \zeta + \alpha \cdot PART_{ist} + X_{ist}\psi + YEAR_{it}\rho + \varepsilon_{ist}$.

In this case, α identifies the migration response to SSI by asking whether likely participants are more likely to make transitions to higher benefit states than are unlikely participants in the same age group. We label this second difference-in-difference estimator DD2. But DD2 also has a potential pitfall, because migration patterns of the more affluent unlikely participants may differ for reasons unrelated to SSI. For example, they may be more likely to migrate to higher-cost areas in retirement (again, California is a good example), and these higher cost areas may happen to have higher benefits.

To address this possibility, we use a difference-in-difference-in-differences (DDD) estimator that uses 67-69 year-old and 60-61 year-old likely participants and unlikely participants. This estimator is:

$$(4) \qquad \Delta BEN_{ist,t-5} = \zeta + \alpha \cdot PART_{ist} \cdot OLD_{ist} + \gamma \cdot OLD_{ist} + \delta \cdot PART_{ist} + X_{ist}\psi + YEAR_{it}\rho + \varepsilon_{ist}.$$

In this specification, α identifies a migration response only from the difference in the change in benefits experienced by older versus younger likely participants, relative to the change experienced by older versus younger unlikely participants—or equivalently, only from the difference in the change in benefits experienced by older likely versus unlikely participants, relative to younger likely versus unlikely participants. Thus, this DDD estimator simultaneously accounts for differences in migration patterns by age that can plague DD1, and differences by likely participation status that can plague DD2.[12]

As noted earlier, we estimate specifications (2)-(4) for the full sample (of the appropriate groups based on age and likely participation), and for migrants only. In addition, we take these specifications one step further. Specifically, whether someone moves to a state with higher benefits may well depend upon whether there is such a state in close proximity, because of moving costs, a desire to remain near family and friends, etc. We therefore construct an indicator for each person of whether they could receive a non-trivial state supplement (exceeding 20 percent of the federal benefit) in a bordering state.[13] We then add interactions of all of the variables indicating either likely participation or being in the older group (PART and OLD), as well as their interaction (PART·OLD) in the DDD specification, with a

[12] We do not estimate specifications including state dummy variables, unlike in the labor supply case, because we do not estimate migration-related specifications with benefits as a right-hand side variable.

[13] Gelbach (2004) does something similar, but for a fixed radius, because in some regions, such as New England, distances between states are small and one might be more likely to move to a non-bordering state. But Massachusetts and Connecticut both supplement generously and at least one borders all of the other New England states, with the exception of Maine. Thus, using a fixed radius approach does not appear useful here. We did confirm, however, that defining Maine to also have a bordering state with generous supplements (since it is separated only slightly from Massachusetts) had no bearing on the results.

dummy variable (BORD) indicating a bordering state with generous SSI supplementation. For example, corresponding to DD1 we have:

(2') $\Delta BEN_{ist,t-5} = \zeta + \alpha' \cdot OLD_{ist} \cdot BORD_{ist} + \alpha \cdot OLD_{ist} + \beta \cdot BORD_{ist} + X_{ist}\psi + YEAR_{it}\rho + \varepsilon_{ist}$.

Evidence of a significant (presumably positive) estimate of α' would indicate that there is a stronger migration response for those who can obtain a higher SSI benefit in a nearby state. The combined estimate (α' + α) captures the migration response for older likely participants who face a bordering state with generous SSI supplements. We therefore report estimates of (α' + α) in the tables, which reveal whether, in comparison to the estimates from the earlier specifications, there is a detectable migration response for those for whom a migration response is less costly because of the proximity of a state with higher benefits. We similarly augment equations (3) and (4) to incorporate the interactions with BORD and report the estimated migration response for those living in states bordering states with generous SSI supplementation. Specifically, we estimate

(3') $\Delta BEN_{ist,t-5} = \zeta + \alpha' \cdot PART_{ist} \cdot BORD_{ist} + \alpha \cdot PART_{ist} + \beta \cdot BORD_{ist} + X_{ist}\psi + YEAR_{it}\rho + \varepsilon_{ist}$,

and

(4') $$\Delta BEN_{ist,t-5} = \zeta + \alpha' \cdot PART_{ist} \cdot OLD_{ist} \cdot BORD_{ist} + \alpha \cdot PART_{ist} \cdot OLD_{ist} + \beta \cdot BORD_{ist} + \gamma' \cdot OLD_{ist} \cdot BORD_{ist} + \gamma \cdot OLD_{ist} + \delta' \cdot PART_{ist} \cdot BORD_{ist} + \delta \cdot PART_{ist} + X_{ist}\psi + YEAR_{it}\rho + \varepsilon_{ist},$$

for which we also report estimates of (α' + α).

Finally, to this point we have described specifications estimated for those initially in states that do not supplement SSI benefits, for whom we might observe a migration response in the form of likely SSI participants moving to higher benefit states. Equivalently, we can start by restricting attention to those in high benefit states, and see whether likely participants are less likely to move to states with lower benefits. We do this by restricting attention to those in states in which the SSI supplement exceeds 20 percent of the federal supplement. Using specifications (2)-(4), a migration response to SSI benefits would again predict positive estimates of α, as likely participants would be less likely to make a move that entails a reduction in SSI benefits. Given that most states do not exceed the 20 percent supplementation criterion, for this subsample we do not estimate the augmented specifications taking account of benefits in bordering states.

V. Data

We use Census data from the 5% Public Use Microdata Samples (PUMS) for 1980, 1990, and 2000. These are stratified samples of the population, created by subsampling the full Census sample that received the long form questionnaire; the long form went to approximately 19.4% of all housing units in 1980, 15.9% in 1990, and 15.8% in 2000. These files include information about labor supply in the previous calendar year—specifically whether the individual worked at all, and if so how many weeks. We examine results using both labor supply measures.[14] We focus on males aged 60 and older that fall into one of two categories: heads of household (married and unmarried); and (2) married males who are not coded as head of household. We do not include unmarried males who are not coded as head of household. Sample members aged 65 and older are used to estimate the model for predicting the probability of SSI participation, based on whether they received income from SSI in the previous calendar year. Those aged 60-64 are used to estimate the labor supply models as well as some of the migration models. We also use 67-69 year-olds for some of the migration models.

To the Census data we append information on SSI benefits at the state level. Through 1983 SSI benefits were set each July, and afterwards they were set in January. For the sample aged 65 and older that we use to predict SSI participation, we use marital status and the wife's current age to assign the benefit level for either individuals or couples. For the sample aged 60-64, for whom we will both predict SSI participation after age 65 and model contemporaneous labor supply, we assign the couple benefit if the individual is married and his wife is no more than three years younger than he. In assessing the benefits of SSI participation, we assume the individual considers the couples benefit level if his wife is sufficiently close in age (three years or fewer) that he is likely to face the higher benefit level for most of the prospective retirement period. Because the Census labor supply measures refer to the previous

[14] There are also indicators of employment and hours worked in the week prior to the survey, but we do not use these because as shorter-term measures they are likely to be noisier and less likely to reflect longer-term behavioral responses. In prior work (Neumark and Powers, 2005) with Current Population Survey data we found stronger evidence of labor supply disincentive effects of SSI with annual as opposed to survey week labor supply measures.

calendar year, we use the SSI benefits available for that year (these are as of January in 1989 and 1999 and as of July in 1979). Nominal maximum state supplements for the years we study are reported in Table 1. Looking first at columns (1)-(3) for individual benefits, and (4)-(6) for couple benefits, the table reveals that there has been and remains considerable variation in state supplements. Alaska, California, and Massachusetts have the highest benefits over the long haul (and Connecticut for most of the sample period, although at first it decided benefits on a case-by-case basis), while numerous states have rather trivial supplements. The table also shows that there have been relatively few sharp changes in state supplements, but that changes are common. Recall that even though the state benefit is often a small share of the federal benefit, many SSI participants receive only a state supplement or a state supplement plus only a fraction of the federal benefit, because of the benefit reductions for other sources of income.[15]

Unfortunately, SSI participation is measured inconsistently across Census years. As Table 2 shows, in 2000 there was a question referring to SSI income exclusively. But in 1980 and 1990 the question also included AFDC or other public assistance or public welfare payments. In 2000, there was a separate question for public assistance or welfare payments. We suspect the problem of other welfare payments is not too serious, because we are studying older individuals. As the last column of Table 2 suggests, SSI participation of men aged 65 and older in 1999 was 3.28 percent, while another 0.62 percent reported some other form of public welfare or assistance. As shown in the last two rows of the table, for 1980 and 1990 the administrative data on SSI participation indicates participation nearly as high (in 1989) or higher (in 1979) than does the Census information that includes other forms of public assistance. This, again, suggests that there is not a great deal of non-SSI welfare for our sample. Given the inconsistency, though, throughout the paper we use the data two ways: first, using the cleanest possible measure in each year, which entails using the SSI-only question in 2000; and second, using the combined SSI plus public assistance measure in each year. The latter approach, while prone to more measurement error, is

[15] We report the maximum benefit and use it in the empirical analysis, because we do not know the SSI payments that are actually received by individuals and because these would be endogenous with respect to labor supply. Also, our analyses use individuals under age 65, for whom actual benefits are not defined because the age of eligibility has not been reached. Depending on the question being asked, using the maximum benefit instead of the actual benefit a person expects could bias the estimates. In particular, with the data we have, we can ask how SSI policy variation in the form of the maximum benefit affects labor supply, and we can characterize migration responses to state SSI policy as reflected in maximum SSI benefits.

preferable on grounds of consistency across the years. Moreover, keep in mind that the data on SSI participation are only used to predict who is a likely participant, so whatever the precise measure in the Census, we are really most interested in predicting who is likely to have very low resources and be eligible for SSI. It may not be critical to measure SSI participation perfectly in order to pick out those observations that face a higher likelihood of eligibility and hence may be prone to respond to the incentive effects posed by SSI, especially given that our estimates do not have a structural interpretation.

VI. Results

Replication of Labor Supply Results

We begin by providing information on the sample and on likely SSI participants. The first two columns of Table 3 report descriptive statistics for the full samples of 62-64 and 60-61 year-old men. Both labor supply measures reveal significant drop-offs after age 61; for example, the employment rate falls from 75.6 to 62.2 percent. Estimates of the probit models used to predict SSI participation, which in turn are used to select a group of likely SSI participants for analysis, are presented in columns (3) and (4), for the two different ways of defining SSI receipt; the estimates have been transformed into marginal effects. There is no way of knowing whether the individual was originally a participant in the aged component of the SSI program. In principle it would be desirable to drop those with a disability—who might be receiving SSI under the disabled or blind components of the program—from the analysis. But information on disability is not elicited in a consistent manner across the three Census years.

In the SSI participation model, not surprisingly, the real value of the SSI benefit is positively associated with SSI participation. The magnitude of the estimate implies that a $100 increase in monthly benefits (about a 24 percent increase relative to the sample mean) raises the participation probability by about 0.0064, a 14 percent increase relative to the participation rate of men aged 65 and over of about 0.046, implying an elasticity of 0.58. We would expect that variables positively related to lifetime income or wealth, as well as current earnings opportunities, would be negatively associated with SSI participation. This is borne out in the negative effect of education, and the positive effect of black and the state unemployment rate. Similar to what we found in SIPP data, we find that through 11 years of schooling higher education is associated with a lower likelihood of SSI participation; participation is lower for those with 12 or more years of schooling, although the marginal effect of education beyond high school is minimal. This was apparent in a specification with much more-detailed education controls, and is captured in the specification reported here (and used throughout) by including an interaction between years of education and a dummy variable for less than a high school education, as well as a dummy variable for a high school education or more. In addition, given that never married men typically earn less than divorced, widowed, or separated men, who in turn earn less than married men, net of other controls (Korenman and Neumark, 1991), it is not surprising that we find that never married men are considerably more likely to participate in SSI, and divorced, widowed, or separated men somewhat more likely, relative to married men.

We use the estimates of this model to predict the probability of SSI participation of 60-64 year-olds, so that we can identify likely participants.[16] For most of our analyses, we use a cutoff of the 90th centile of the distribution of predicted probabilities of SSI participation to identify these likely participants. As reported in the last row of columns (3) and (4), the 90th centile of the distribution is a about a 0.07 probability of participation. This is, of course, a relatively low probability, but there are no doubt many determinants of SSI participation that are unobserved to the researcher, so that many individuals at this predicted probability have a much higher participation probability in fact.

Regression results from implementing each of the alternative estimators described in Section III are reported in Table 4.[17] The two panels (A and B) differ in how SSI participation was coded in the

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[16] To keep the "treatment" and "control" groups comparable, when we predict these probabilities we net out the SSI supplement, so that individuals in states with different supplements but with identical characteristics (as captured in the other controls) are assigned the same probability of participation. (For the same reason, we do not include fixed state effects in the participation probits, since these would reflect in large part cross-state variation in SSI benefits.) This netting out of the SSI supplement means that one of the potential problems posed by migration related to SSI benefits—that individuals live in one state currently but are responding to SSI benefits in a state to which they plan to migrate—does not create a problem in predicting probabilities of SSI participation. But it remains a problem, of course, for the labor supply equation.

[17] Linear probability models with robust standard errors are used for the employment equation, to ease interpretation. Estimated partial derivatives from probit models were virtually identical. In all estimations beginning in this table we report standard errors clustered on cells defined by state, year, age group, and likely participation. We define cells based on the latter two characteristics as well as state and year because, effectively, the combination of benefits, age, and likely participation is what defines the policy variation. Bertrand, et al. (2004) discuss the problem posed by serially correlated errors in panel data when treatments are highly persistent. This is

probit used to identify likely participants. The first two rows of each panel report estimates that rely on cross-sectional (cross-state) variation in SSI benefits. In the DD-CS estimates, which use the 60-61 year-olds as a control sample, all four estimates are negative, although none statistically significant. Using instead the cross-sectional estimator that compares the differences between 62-64 year-old and 60-61 year-old likely participants to the differences between 62-64 year-old and 60-61 year-old unlikely participants (DDD-CS), the estimates are centered around zero.

The final two rows of each panel in Table 4 report the results from the estimators using the time-series variation in SSI benefits within states, which we have argued are preferable because they do not rely on comparisons with unlikely participants. The DD-TS estimator using only the 62-64 year-old likely participants yields negative estimates in all four cases, although estimates are insignificant in three out of four cases. But the DDD-TS estimator, which might be viewed as giving the strongest identification of SSI effects on labor supply, yields estimated coefficients that are very similar in magnitude but more strongly significant; that is, the stronger evidence from the DDD-TS estimator results mainly from smaller standard errors rather than from different coefficient estimates.

Using the estimates for employment in the last row of Panel B, the estimated magnitude implies that a \$100 increase in monthly SSI benefits (or a \$1,200 increase in annual benefits) reduces the employment rate by 0.012. Based on the figures in Table 3, this implies an elasticity of −0.08.[18] For hours, the same increase in benefits reduces hours by 0.605, implying a similar elasticity of −0.09.[19] All told, then, although the Census data yield weaker evidence of labor supply disincentive effects than the other data sources we have studied, such evidence nonetheless emerges from what we view as the most reliable estimator.

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particularly problematic in panels with yearly data, and treatments that are, for example, dummy variables that are first zero and then (in some cases) always one. In our context, this is less relevant, because the observations are 10 years apart (suggesting little serial correlation), and the treatments vary less systematically over time. We experimented with their proposed solution by clustering only on state, age group, and likely participation (not year). The standard errors were sometimes larger and sometimes smaller, but often not very different.

[18] This is smaller than the elasticity we found using CPS data (Neumark and Powers, 2005). Aside from the different coding of SSI participation, the CPS data also have information on Food Stamp use, which we use to help predict SSI participation on the theory that it contains information on longer-run resources and also on the individual's willingness to tolerate the stigma effect of participation in these types of programs.

[19] To capture changes relative to 60-61 year-olds, we calculate these elasticities using the means for likely participants aged 60-61 that are reported in Table 3.

Migration Responses to SSI Benefits

Next, we turn to the key new evidence that this paper presents, on whether either post-eligibility or pre-eligibility migration responds to SSI benefit levels. Table 5 reports the analyses for post-eligibility migration.[20] As for labor supply, the estimates are reported for the two different ways of measuring SSI receipt in estimating the likelihood of SSI participation. This table (and the next) focuses on those initially in low benefit states that do not supplement federal SSI benefits.

The estimates of the first difference-in-differences model (DD1), in the first row of each panel, indicate that 67-69 year-olds classified as likely participants are if anything more likely to experience a decline in the maximum SSI benefits available in their state of residence, relative to similar 60-61 year-olds. However, although the point estimates are negative, none are statistically significant. This is true for the full sample (columns (1) and (2)), as well as movers only (columns (3) and (4)). These results are inconsistent with SSI recipients moving, post-eligibility, to states with higher supplements. Note, also, that in every case the estimate identified for those in states with high supplement bordering states is less negative, which is the direction of change in the effect that we would expect from the augmented specifications distinguishing these individuals.[21]

For the second such estimator (DD2), which uses only 67-69 year-olds but uses unlikely participants as the comparison group, the evidence is quite similar. In both the upper and lower panels the first three estimates are negative and statistically insignificant. In column (4) the estimates are positive. Note that these estimates are perhaps the cleanest test of the migration hypothesis, since they focus on movers only and identify the effect for those initially residing in a low benefit state but with a bordering state that supplements SSI generously (equation (3')). Nonetheless, the estimates are well below statistical significance.

[20] Note that the samples in columns (1) and (3), which include both movers and non-movers, are smaller here than for the labor supply estimation in Table 4. This occurs for three reasons. First as noted in Table 1, Connecticut decided their SSI supplements on a case-by-case basis in some years, including some of the five-year lags needed for the migration specifications. Second, we have to drop those who migrated from outside the United States. And most important, the migration question was not coded for the full Census sample in 1980 (U.S. Department of Commerce, 1980, p. 8); this accounts for most of the sample size differences.

[21] Perhaps they are more likely to make a move that does not entail benefit erosion. Of course if we found the expected migration response entailing a positive estimate of α, we would expect the estimate of $(\alpha + \alpha')$ to be larger.

Finally, the third row of each panel reports the DDD estimates. Here, there is no evidence of migration to states with higher SSI benefits, as most of the estimates are negative, and all are statistically insignificant. The one positive coefficient, for the full sample with bordering states with high benefits, is very small (0.160 to 0.199); again, though, note that we obtain the more positive (or less negative) estimates when focusing on those with high supplements in states bordering their origin states.

Overall, then, the results reported in Table 5 reveal no evidence of a post-eligibility migration response to SSI benefits. This particular type of migration response would lead, if anything, to understatement of the estimated labor supply response to SSI. Thus, potentially more interesting is pre-eligibility migration, since this can generate spurious evidence of labor supply disincentive effects of SSI. To address this form of migration, Table 6 reports results for migration among 62-64 year-olds.

The results reported in Table 6 again, however, fail to reveal any evidence of a migration response to SSI benefits. In the first two columns, which use the full sample (movers and non-movers) of 62-64 year-old likely participants and the appropriate comparison group, most of the estimates are negative, and the few that are positive are very small. In the subsample of movers, the estimates using DD2 yield larger positive coefficient estimates that represent non-trivial benefit changes, but none of the estimates exceed their standard error. Thus, on numerous grounds including both statistical significance and for most estimates the magnitudes and even signs of the effects, there is again no basis for concluding that there is evidence of a migration response to SSI benefits.

Finally, Table 7 looks at those initially in states that supplement federal SSI benefits generously. In this case, a positive effect of likely participation on the change in benefits would be more likely to come about from choosing to stay in a high benefit state, relative to the comparison group. This implies that we are still looking for positive estimates of α in equations (2)-(4) as evidence of a migration response. Columns (1) and (2) present the results for the post-eligibility response among 67-69 year-olds, and columns (3) and (4) present the results for the pre-eligibility response among 62-64 year-olds. Looking first at the full sample of movers and non-movers, in columns (1) and (3), there is no evidence of a migration response, most of the estimates are negative, and the few positive ones are small in magnitude and relative to their standard errors.

The results for movers, though, in this case give a somewhat different impression. For 67-69 year-olds, the estimates for all of the specifications, in both panels, are positive, substantively large, and in four out of six cases larger than their standard errors. Nonetheless, none are statistically significant. However, for the 62-64 year-olds the estimates are smaller, clearly insignificant, and negative in half the cases.[22]

Interpretation of Migration Responses

These results lead to a few tentative conclusions, regarding both whether there is a migration response to SSI, and whether this biases our estimates of the labor supply disincentive effects of SSI. First, strictly speaking not one of the estimates testing for a migration response to SSI benefits in Tables 5 through 7 finds evidence of a migration response. Second, from the perspective of estimating disincentive effects of SSI, and more generally from the perspective of policymakers concerned with SSI benefits playing the role of welfare magnet, none of the evidence points to substantive concerns arising from a migration response. The conclusion regarding the estimation of disincentive effects of SSI is reinforced by the fact that only for 67-69 year-olds do we ever really find point estimates indicative of a migration response to SSI. This is not the type of migration response that leads to overstatement of the labor supply disincentive effects of SSI; rather it is the pre-eligibility response that would most likely lead to such bias.

This conclusion with respect to bias in the labor supply estimates can be demonstrated more directly in one case. One of the types of bias in labor supply estimates stemming from a migration response occurs when retirement prior to age 65 is coupled with migration to a state with high benefits, leading to overstatement of the negative impact of SSI on labor supply. In some sense this is the more

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[22] Throughout, we have estimated separate models for subsamples of those initially in states without SSI supplements and those initially in states with generous supplements. We explored whether pooling the subsamples would increase the precision of the estimates, but the resulting standard errors turned out to be about the same magnitude (roughly speaking, an average of those for the two subsamples). If we were only increasing the sample size for populations for which the same parameters held, then we would expect reductions in standard errors roughly proportional to the increase in the square root of the sample size. But since there is no reason to believe parameters are the same for these two subsamples (which is why we estimate separate models), the standard errors could go up because of worse fit.

important type of bias, given that our past work ignoring migration finds evidence that SSI reduces labor supply. The bias in this case comes from the endogenous choice of labor supply and SSI. If, however, we instrument for SSI in the current state of residence with SSI in the state in which the person resided five years earlier (when we assume he was working), then we break or at least reduce the endogeneity between labor supply and SSI benefits.[23]

Results of this instrumental variables estimation are reported in Table 8. We first report the OLS estimates for the smaller sample for which we have data on state of residence five years earlier, and then report the instrumental variables estimates. As the table shows, in every case the results indicate that the OLS estimates of the effects of SSI are if anything biased upward rather than downward. For example, in the case of the DD-TS or DDD-TS estimators, which we prefer, stronger evidence of labor supply disincentive effects results from the instrumental variables estimation, although in most cases the changes in the estimated coefficients are small. This evidence runs counter to what we would expect if those retiring in the couple of years before age 65 are moving to high benefit states. If anything, it is more consistent with them moving to low benefit states (such as Florida, a magnet for retirees)—which is precisely what our evidence on migration suggests.[24]

More generally, we are interested in interpreting these estimates from the perspective of the behavioral question of whether there is a migration response to SSI benefits. Here, one might view the evidence as less informative than we might prefer. To fix ideas, consider the DDD estimates in column (1) of Table 5. The estimate of α in column (1) measures the *differential* change in benefits for older individuals who are likely participants, and therefore captures the greater tendency of likely participants to move to states with higher SSI benefits (precisely because they would benefit from the higher benefits). Because the standard error of the estimate is 0.962, an average change in benefits that was approximately \$1.57 (\$1.89) higher would be significant at the 10% (5%) level. Given that about four percent of the

[23] We are grateful to a referee for suggesting this approach.

[24] Recall that we also described bias from migration leading to understatement of labor supply disincentive effects, stemming from measurement error attributable to individuals moving to higher benefit states after retirement. However, one would not expect the instrumental variables procedure we use to correct for this type of measurement error, as SSI benefits in the state five years prior would likely reflect the same measurement error as SSI benefits in the current state.

sample moves, changes of this magnitude for the full sample would require that movers in the treatment group experience increases of these amounts divided by 0.04 to be significant, or $39 ($47).[25] On the other hand, one might argue that policymakers should care mainly about the full sample results estimated for both movers and non-movers, and their implied changes in benefits, since these give a sense of the changes in SSI benefits paid out as a result of migration.

In thinking about how informative the estimates are, one key question is whether it is plausible that those movers who can benefit from a more generous SSI program are likely to exhibit moving patterns that lead to relative increases of about $40 in state SSI supplements, relative to movers in the control group. Obviously, we cannot answer this question definitively, but framing it this way is helpful in thinking about how informative the estimates are. As shown in Table 1, for those eligible for individual benefits, there are between nine and 11 states (depending on the year) for which the state supplement exceeds $40, and it is often much larger. For those eligible for couples benefits, there are a few more such states. And these states are relatively geographically dispersed (e.g., CA, OK, CO, MN, NE, NV, WA), rather than, say, clustered in New England. Thus, if there is a migration response to SSI benefits, it seems plausible that we would have seen a response that is statistically detectable in these data given the precision of our estimates.

On the other hand, readers could certainly argue that they would like to be able to detect smaller changes as statistically significant before concluding that there is no migration response. For example, the estimates in column (2) of Table 7, indicating benefit changes in some cases exceeding $20, are arguably large enough to be substantively interesting to policymakers, yet are not statistically significant. To this, we offer three responses. First, the standard caveat—that failure to reject the null of no effect is not the same as concluding that there is no effect—applies. Second, in the other estimators (DD1 and DD2) the standard errors are considerably smaller, so we could detect effects roughly half as large, but we do not. And third, the majority of the estimates are the opposite sign from what we would expect if there

[25] Note, by the way, that if we instead begin with the estimates in column (3), for movers only, the standard error for the DDD estimator is 22.6, implying very similar sized effects for achieving statistical significance—$37 ($44) at the 10% (5%) level.

were a migration response to SSI. Thus, the problem is not that we consistently obtain point estimates consistent with a migration response, but which are small relative to their standard errors. Rather, the estimates appear largely centered around zero, so, hypothetically, if nothing changed but the standard errors were smaller, we would not reach the conclusion that there is the expected migration response to higher SSI benefits. And finally, even in the case of the larger estimates in Table 7, these are for movers, while—as suggested earlier—policymakers are likely more concerned with effects averaged over movers and non-movers.[26]

Finally, it is of interest to contrast these results with those of Gelbach (2004), who uses similar data and a similar approach, but studies migration of single mothers in response to welfare benefit levels (in the period prior to welfare reform). He finds stronger evidence of a migration response (in part because of more precise estimates, but more generally because of larger point estimates), but also concludes that the implications of this response for estimation of incentive effects of welfare benefits are likely trivial. More interestingly, he offers a very natural interpretation of why we might see a stronger response among single mothers. Simply put, migration responses may well depend on where one is in the life cycle, with those more likely to reap longer-term gains from higher benefits also more likely to move to higher benefit states. In fact, he finds evidence of this life-cycle difference in his sample of single mothers, based on age of children (as those with younger children can potentially obtain the higher benefits for a longer period). If we think about older SSI recipients, it is clear that although they can obtain benefits for the remainder of their lives, they may not have much expected longevity, especially taking account of their socioeconomic status as well as their age. Coupled with the likely greater cost and difficulty of moving, these life-cycle considerations could well dampen any migration response to SSI.

VII. Conclusions

When the generosity of income support programs varies across states, there is a natural concern over a migration response of those potentially eligible for the program. States contemplating more generous support need to consider the possibility that the costs of the program will go up not only because

[26] As a practical matter, we are using the largest data set possible to carry out these analyses.

of changes for current residents—including higher payments to those already residing in the state and participating, as well as increases in the number who remain eligible despite other sources of income. In addition, they need to be concerned with increased inflows of eligible participants from other states. Conversely, states might believe that cutting benefits will save more than just projected savings for current residents, if doing so also spurs out-migration of eligible participants. In addition to this direct motivation for looking at migration responses to benefit generosity, the estimation of behavioral responses to income support (and other programs) may be complicated when variation in benefits influences migration. In particular, we consider the impact of migration on estimates of the labor supply disincentive effects of SSI, which are predicted by theory, and of which we have found evidence in past research.

Specifically, we use Census data that permit both the estimation of labor supply responses and the examination of migration responses. We first re-examine the evidence on labor supply effects using these data, finding, again, evidence that more generous SSI benefits reduce the labor supply of individuals who are likely to be financially eligible for SSI and approaching the age of eligibility for the elderly component of the program. We then consider how migration responses to SSI benefit variation across states may influence estimates of labor supply effects that treat state of residence as fixed. The most troubling possibility is that individuals likely to become eligible for SSI, who retire before age 65 for reasons unrelated to the generosity of SSI payments, choose to move to higher benefit states prior to age 65. In this case, we would observe a negative relationship between SSI benefits and labor supply among likely participants, but it would not be generated by a labor supply response to SSI, but rather solely by a migration response. Finally, we present direct evidence on the migration response to SSI benefit variation.

We fail to find any statistically significant evidence that older individuals likely to be eligible for SSI in the near future, or already eligible for SSI, tend to move from low benefit to high benefit states. These findings are robust to the use of a number of different comparison groups to try to capture the state-to-state migration patterns that exist independently of a response to SSI. These findings, and additional evidence on labor supply effects, imply that evidence of labor supply disincentive effects of SSI does not stem from migration behavior that could, in principle, generate these findings, and similarly does not indicate a detectable migration response among those who are likely to participate in SSI. While there is, similarly, no statistically significant evidence of a migration response among those who move, some of the estimates for this subsample are consistent with such a response, but the estimates are sufficiently imprecise that we cannot rule out a large range of possible parameters. Thus, one might conclude that the evidence is not necessarily inconsistent with the behavioral migration response to SSI benefits that economic theory might lead us to expect, but that other concerns about migration responses to SSI benefits—including both substantive welfare magnet concerns and concerns about bias in estimates of disincentive effects—are unfounded.

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Table 1: Variation in Monthly SSI Benefits Across States and Over Time

	Individuals			***Couples***		
	1979	1989	1999	1979	1989	1999
	(1)	(2)	(3)	(4)	(5)	(6)
State supplements						
AK	206	317	362	296	462	528
CA	148	234	176	348	563	450
CO	37	58	36	178	299	321
CT	NA	384	247	NA	551	343
DC	15	15	0	30	30	0
HI	15	5	5	24	9	8.8
ID	54	73	48	61	43	17
ME	10	10	10	15	15	15
MA	129	129	129	202	202	202
MI	34	30	14	51	45	28
MN	34	35	81	45	66	111
NE	87	38	27	94	60	13
NV	43	36	36	83	74	74
NH	29	27	27	20	21	21
NJ	23	31	31	12	25	25
NY	63	86	87	80	102	104
OK	79	64	53	158	128	106
OR	12	2	2	10	0	0
PA	33	32	27	49	49	44
RI	37	61	64	70	115	121
SD	0	15	15	15	15	15
UT	10	9	0	20	18	5
VT	39	60	55	50	109	103
WA	45	28	27	49	22	21
WI	93	103	84	150	166	132
WY	20	20	10	40	40	25
Federal benefits	208.2	368	500	312.3	553	751

Notes: Benefits shown are for the calendar year, for states with any supplements over the sample period. Illinois is omitted because the state decides the benefit on a case-by-case basis, as was also the case for Connecticut in some years.

Table 2: Comparing SSI vs. Other Public Assistance, and Disability Measures Across Censuses

	1980	1990	2000
Census question re: SSI income	Supplemental Security (SSI), Aid to Families with Dependent Children (AFDC), or other public assistance or public welfare payments -- Y/N and annual amount	Supplemental Security Income (SSI), Aid to Families with Dependent Children (AFDC), or other public assistance or public welfare payments -- Y/N and annual amount	Supplemental Security Income (SSI) -- Y/N and annual amount
Census question re: other public assistance income	See above	See above	Any public assistance or welfare payments from the state or local welfare office – Y/N and annual amount
Number and % of men 65 and older receiving SSI (Census)	N/A	N/A	17,835 3.28%
Number and % of men 65 and older receiving any public assistance, including SSI (Census)	22,410 5.77%	23,866 4.82%	21,132 3.90%
Number and % of 65 and older population receiving SSI benefits[1] (Administrative)	1,838,381 7.16%	1,484,160 4.75%	1,327,567 3.79%

Notes: Census data are from the 1980, 1990 and 2000 5% PUMS. Only records with unallocated SSI or public assistance income are included. Percent of records with allocated public assistance income is 14.6% in 1980, 18.1% in 1990, and 20.9% in 2000.

[1] Source: SSA Annual Statistical Supplement 2001. Denominator (total U.S. elderly population) from 1980, 1990 and 2000 U.S. Census counts. Note that the figures in this row refer to SSI receipt in the years 1980, 1990 and 2000, rather than 1979, 1989, and 1999, as in the rest of the table.

Table 3: Descriptive Statistics and Probit Estimates for SSI Participation: 1980, 1990, 2000 PUMS

	Descriptive statistics, all male heads of household		Probit for SSI participation[1]	Probit for SSI participation[2]	Descriptive statistics, likely participants[3]		Descriptive statistics, likely participants[4]	
	Ages 62-64	Ages 60-61	Ages 65+	Ages 65+	Ages 62-64	Ages 60-61	Ages 62-64	Ages 60-61
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed last year (%)	62.2	75.6			47.0	58.8	47.3	59.1
Usual weekly hours last year	25.0	31.8			18.4	23.8	18.6	24.0
Real maximum monthly federal plus state SSI payment (1982-84 $)	419.80	421.91	0.0066 (0.0001)	0.0063 (0.0001)	365.14	368.09	365.22	368.17
Education (years)	11.7	11.9			5.5	5.5	5.5	5.5
Education x less than high school education			-0.0067 (0.00007)	-0.0064 (0.00007)				
High school education or more			-0.0981 (0.0008)	-0.0939 (0.0008)				
Black	7.2%	7.4%	0.0277 (0.0008)	0.0272 (0.0008)	34.7%	35.7%	34.6%	35.7%
Never married	3.5%	3.5%	0.0419 (0.0014)	0.0392 (0.0014)	12.9%	12.6%	12.5%	12.2%
Divorced, widowed, or separated	11.7%	11.3%	0.0229 (0.0006)	0.0213 (0.0006)	32.0%	30.7%	31.6%	30.3%
Eligible for married couple benefit (married & wife age 65+)			-0.0145 (0.0005)	-0.0135 (0.0005)				
State unemployment rate (%)	5.1	5.1	0.0021 (0.0002)	0.0022 (0.0001)	5.6	5.6	5.6	5.6
Number of observations	402,035	280,957	1,377,883	1,381,619	41,312	26,624	41,280	26,571
Mean participation probability			0.0471	0.0453				
90th centile of predicted probability for 60-64 year-olds			0.0719	0.0706				

In columns (3) and (4), the marginal effects implied by the probit estimates are reported; standard errors are reported in parentheses, with t-statistics scaled to equal those in the probit estimation. The probit includes fixed year effects. In the probit estimation, the SSI benefit is divided by $100, so the marginal effect can be interpreted as arising from a $100 increase in monthly benefits. When computing predicted probabilities, the supplement variable was set to its sample mean. Men coded as "married, spouse present", but with no corresponding spouse record in the Census, were excluded from all analyses. The omitted marital status category is married heads with younger spouses.

[1] Includes other public assistance for all years.

[2] Includes other public assistance for 1980 and 1990, and SSI only in 2000.

[3] Likely participation derived from probit on individuals receiving SSI and other public assistance for all years.

[4] Likely participation derived from probit on individuals receiving both SSI and other public assistance for 1980 and 1990, and SSI only in 2000.

Table 4: The Effect of SSI Benefits on Employment and Hours of 62-64 Year-Old Men

	Employed last year (1)	Usual weekly hours last year (2)
A. SSI probit uses SSI only in 2000, public assistance or SSI in 1980 and 1990		
DD-CS	-0.003 (0.008) [N=67,851]]	-0.138 (0.358) [N=67,851]
Sample: 62-64 and 60-61 year-old likely participants		
Regressors: SSI·OLD, SSI, OLD, YEAR, X		
DDD-CS	0.002 (0.010) [N=682,992]	0.013 (0.430) [N=682,992]
Sample: 62-64 and 60-61 year-old likely and unlikely participants		
Regressors: SSI·PART·OLD, SSI, OLD, PART, SSI·OLD, SSI·PART, OLD·PART, YEAR, X		
DD-TS	-0.011 (0.009) [N=41,280]	-0.549 (0.328)* [N=41,280]
Sample: 62-64 year-old likely participants		
Regressors: SSI, STATE, YEAR, X		
DDD-TS	-0.008 (0.005)* [N=67,851]	-0.429 (0.198)** [N=67,851]
Sample: 62-64 and 60-61 year-old likely participants		
Regressors: SSI·OLD, SSI, OLD, STATE, YEAR, OLD·STATE, OLD·YEAR, X		
B. SSI probit uses SSI or public assistance in all years		
DD-CS	-0.005 (0.008) [N=67,936]	-0.222 (0.316) [N=67,936]
DDD-CS	-0.0003 (0.010) [N=682,992]	-0.076 (0.401) [N=682,992]
DD-TS	-0.012 (0.011) [N=41,312]	-0.525 (0.359) [N=41,312]
DDD-TS	-0.012 (0.005)** [N=67,936]	-0.605 (0.214)*** [N=67,936]

Linear probability estimates are reported for the employment equation. The SSI benefit is divided by $100, so the marginal effect can be interpreted as the effect of a $100 increase in monthly benefits. The SSI benefit is aligned with the labor supply measure, so that the benefit is for the year prior to the Census year. The vector of controls X includes education, race, marital status, and the state unemployment rate. Standard errors of the estimates are reported in parentheses, and are calculated clustering on cells defined by state, year, age group, and likely participation. '*', '**", and '***' indicate that the estimate is statistically significant at the ten-, five-, or one-percent level. All specifications use the continuous measure of SSI benefit, and the 90th centile cutoff for predicted probability of SSI participation.

Table 5: The Effect of SSI Eligibility on Change in Benefits in the Last Five Years of 67-69 Year-Old Men Originally in States without SSI Supplement

	All (1)	All (2)	Movers (3)	Movers (4)
A. SSI probit uses SSI only in 2000, public assistance or SSI in 1980 and 1990	α	α’+ α	α	α’+ α
DD1: equation (2)/(2’)	-0.305 (0.362) [N=31,526]	-0.233 (1.442) [N=31,525]	-12.563 (13.690) [N=630]	-4.786 (23.128) [N=629]
Sample: 67-69 and 60-61 year-old likely participants				
Regressors: OLD, YEAR, X (BORD·OLD, BORD in equation (2’))				
DD2: equation (3)/(3’)	-0.422 (0.540) [N=141,444]	-1.257 (1.881) [N=141,424]	-4.432 (13.527) [N=5,284]	8.337 (27.524) [N=5,264]
Sample: 67-69 likely and unlikely participants				
Regressors: PART, YEAR, X (BORD·PART, BORD in equation (3’))				
DDD: equation (4)/(4’)	-0.185 (0.962) [N=252,671]	0.199 (2.576) [N=252,630]	-12.173 (22.616) [N=10,001]	-6.430 (31.693) [N=9,960]
Sample: 67-69 and 60-61 year-old likely and unlikely participants				
Regressors: PART·OLD, PART, OLD, YEAR, X (BORD·PART·OLD, BORD·OLD, BORD·PART, BORD in equation (4’)				
B. SSI probit uses SSI or public assistance in all years				
DD1: equation (2)/(2’)	-0.295 (0.358) [N=31,851]	-0.246 (1.488) [N=31,850]	-10.583 (13.403) [N=647]	-3.778 (23.851) [N=646]
DD2: equation (3)/(3’)	-0.281 (0.517) [N=141,444]	-1.130 (1.936) [N=141,424]	-3.615 (12.946) [N=5,284]	11.449 (28.113) [N=5,264]
DDD: equation (4)/(4’)	-0.173 (0.962) [N=252,671]	0.160 (2.606) [N=252,630]	-10.034 (22.365) [N=10,001]	-5.121 (32.055) [N=9,960]

Sample includes men in states without SSI supplements in 1975, 1985, or 1995. Migration is measured from that year to the Census year. The vector of controls X includes education, race, and marital status. All specifications use the continuous measure of SSI benefit, and the 90th centile cutoff for predicted probability of SSI participation. The dependent variable is the change in benefits. BORD is a dummy variable equal to one if a bordering state has a state supplement for which the individual would be eligible (as an individual or couple depending on his classification) exceeding 20 percent of the federal benefit. For these estimates states without bordering states are dropped. Only individuals in the United States during the Census year and five years earlier are included. Standard errors of the estimates are reported in parentheses, and are calculated clustering on cells defined by state, year, age group, and likely participation.

Table 6: The Effect of SSI Eligibility on Change in Benefits in the Last Five Years of 62-64 Year-Old Men Originally in States without SSI Supplement

	All (1)	All (2)	Movers (3)	Movers (4)
A. SSI probit uses SSI only in 2000, public assistance or SSI in 1980 and 1990	α	α'+ α	α	α'+ α
DD1: equation (2)/(2')	-0.063 (0.408) [N=33,119]	-0.261 (1.472) [N=33,116]	-5.501 (14.290) [N=746]	-5.436 (21.689) [N=743]
Sample: 62-64 and 60-61 year-old likely participants				
Regressors: OLD, YEAR, X (BORD·OLD, BORD in equation (2'))				
DD2: equation (3)/(3')	0.087 (0.666) [N=161,122]	-1.309 (1.738) [N=161,102]	9.033 (13.125) [N=7,033]	15.745 (25.447) [N=7,013]
Sample: 62-64 likely and unlikely participants				
Regressors: PART, YEAR, X (BORD·PART, BORD in equation (3'))				
DDD: equation (4)/(4')	0.038 (0.972) [N=272,349]	0.231 (2.423) [N=272,308]	-1.626 (21.736) [N=11,750]	-0.291 (29.504) [N=11,709]
Sample: 62-64 and 60-61 year-old likely and unlikely participants				
Regressors: PART·OLD, PART, OLD, YEAR, X (BORD·PART·OLD, BORD·OLD, BORD·PART, BORD in equation (4')				
B. SSI probit uses SSI or public assistance in all years				
DD1: equation (2)/(2')	-0.041 (0.401) [N=32,913]	-0.239 (1.436) [N=32,910]	-3.632 (13.892) [N=770]	-5.371 (21.709) [N=767]
DD2: equation (3)/(3')	0.309 (0.641) [N=161,122]	-0.854 (1.898) [N=161,102]	10.608 (12.105) [N=7,033]	17.909 (25.463) [N=7,013]
DDD: equation (4)/(4')	0.068 (0.977) [N=272,349]	0.232 (2.558) [N=272,308]	-0.076 (21.482) [N=11,750]	0.414 (29.469) [N=11,709]

See notes to Table 5.

Table 7: The Effect of SSI Eligibility on Change in Benefits in the Last Five Years of 67-69 and 62-64 Year-Old Men Originally in States with SSI Supplement Exceeding 20% of Federal SSI Benefit

	67-69, All (1)	67-69, Movers (2)	62-64, All (3)	62-64, Movers (4)
A. SSI probit uses SSI only in 2000, public assistance or SSI in 1980 and 1990	α	α	α	α
DD1: equation (2)	-1.146 (17.422) [N=8,904]	22.663 (20.325) [N=301]	-1.275 (18.338) [N=9,522]	10.851 (16.985) [N=331]
Sample: 67-67/62-64 and 60-61 year-old likely participants Regressors: OLD, YEAR, X				
DD2: equation (3)	-0.267 (20.048) [N=52,142]	25.761 (19.520) [N=2,943]	-4.488 (21.017) [N=61,099]	-3.082 (15.767) [N=3,566]
Sample: 67-69/62-64 likely and unlikely participants Regressors: PART, YEAR, X				
DDD: equation (4)	-2.864 (28.817) [N=95,064]	18.614 (24.971) [N=5,210]	-0.569 (29.264) [N=104,021]	1.181 (20.727) [N=5,833]
Sample: 67-69/62-64 and 60-61 year-old likely and unlikely participants Regressors: PART·OLD, PART, OLD, YEAR, X				
B. SSI probit uses SSI or public assistance in all years				
DD1: equation (2)	-0.945 (17.222) [N=8,695]	22.972 (20.099) [N=304]	-0.704 (17.724) [N=9,217]	8.130 (16.910) [N=329]
DD2: equation (3)	3.586 (19.158) [N=52,142]	27.053 (19.217) [N=2,943]	-0.185 (19.727) [N=61,099]	-4.212 (15.720) [N=3,566]
DDD: equation (4)	-2.428 (28.234) [N=95,064]	19.193 (24.778) [N=5,210]	0.063 (28.596) [N=104,021]	-1.131 (20.697) [N=5,833]

See notes to Table 5. A state's supplement is considered above the 20% cutoff if the state supplement for which the individual would be eligible (as an individual or couple depending on his classification) exceeds 20 percent of the federal benefit.

Table 8: The Effect of SSI Benefits on Employment and Hours of 62-64 Year-Old Men

	OLS, IV sample	IV	OLS, IV sample	IV
	Employed Last Year (1)	Employed Last Year (2)	Usual weekly hours last year (3)	Usual weekly hours last year (4)
<u>A. SSI probit uses SSI only in 2000, public assistance or SSI in 1980 and 1990</u>				
SD	0.017 (0.009)* [N=35,435]	0.016 (0.009)* [N=35,435]	0.890 (0.380)** [N=35,435]	0.832 (0.355)** [N=35,435]
DD-CS	-0.004 (0.007) [N=57,691]	-0.005 (0.007) [N=57,691]	-0.120 (0.302) [N=57,691]	-0.193 (0.293) [N=57,691]
DDD-CS	0.001 (0.009) [N=576,411]	-0.0003 (0.008) [N=576,411]	0.009 (0.390) [N=576,411]	-0.059 (0.359) [N=576,411]
DD-TS	-0.016 (0.012) [N=35,435]	-0.021 (0.014) [N=35,435]	-0.641 (0.390) [N=35,435]	-0.925 (0.450)** [N=35,435]
DDD-TS	-0.007 (0.005) [N=57,691]	-0.009 (0.006) [N=57,691]	-0.399 (0.219)* [N=57,691]	-0.509 (0.236)** [N=57,691]
<u>B. SSI probit uses SSI or public assistance in all years</u>				
SD	0.016 (0.008)* [N=35,384]	0.015 (0.008)* [N=35,384]	0.860 (0.359)** [N=35,384]	0.813 (0.341)** [N=35,384]
DD-CS	-0.006 (0.007) [N=57,699]	-0.006 (0.007) [N=57,699]	-0.177 (0.293) [N=57,699]	-0.228 (0.281) [N=57,699]
DDD-CS	-0.001 (0.009) [N=576,411]	-0.001 (0.008) [N=576,411]	-0.047 (0.373) [N=576,411]	-0.093 (0.340) [N=576,411]
DD-TS	-0.016 (0.012) [N=35,384]	-0.020 (0.014) [N=35,384]	-0.635 (0.386) [N=35,384]	-0.862 (0.429)** [N=35,384]
DDD-TS	-0.009 (0.005)* [N=57,699]	-0.010 (0.006)* [N=57,699]	-0.495 (0.228)** [N=57,699]	-0.573 (0.241)** [N=57,699]

In the IV estimation, maximum SSI benefit in the state of residence five years ago is used as an instrument for the benefit in the current state of residence. The sample sizes are smaller than in Table 4 because those who immigrated from outside the country or with missing state of residence five years ago are dropped because the instrument is unavailable. In the case of interactions with variable Z, for example, the interactions of Z and the benefit in the state of residence five years ago and Z are used as instruments for interactions of Z and the current benefit. See notes to Table 4.