The Incentive Effects of Health-Targeted Social Insurance

by

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ABSTRACT

This thesis examines how the presence and structure of government disability insurance programs influence the labor market and health care decisions of potential participants. Chapter 1 studies the large liberalization of the Supplemental Security Income (SSI) for children in the 1990s and its effects on the propensity of low-income parents to identify health impairments in their children. The SSI expansion was followed by a substantial revision of the health status of low-income children by their parent. I find that increases in SSI benefits raise the likelihood that a family identifies a chronic mental impairment in their child. The discovery often leads to SSI receipt for the child and a reduction in the labor market activity of the parent. Increases in SSI benefits contribute to the movement of children into special education programs and the shift of school resources toward the treatment of the disabled.

The second chapter documents the role of state governments in identifying children eligible for SSI participation and measures the degree to which states attempt to shift public welfare recipients onto the SSI roles. States suffering from fiscal troubles were more likely to transfer children from Aid to Families with Dependent Children (AFDC) to SSI, and states that pay a higher proportion of AFDC benefits to their citizens were more likely to encourage this movement. This evidence is important for evaluating the consequences of the funding changes that accompany the recent welfare reforms.

Chapter 3, written with Jonathan Gruber, investigates the consequences of changing the initial screening process stringency for the Social Security Disability Insurance (DI) program on the labor force participation decisions of older men. We find that a rise in denial rates led to a statistically significant fall in labor force non-participation among 45-64 year old males. We also find that denial rate increases effectively targeted their incentive effects to more able individuals, according to an anthropometric measure of disability.

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Introduction

This thesis examines how the presence and structure of government disability insurance programs influence the labor market and health care decisions of potential participants. Chapter 1 studies the large liberalization of the Supplemental Security Income (SSI) for children in the 1990s and its effect on the propensity of low-income parents to identify health impairments in their children. The second chapter documents the role of state governments in identifying children eligible for SSI participation and measures the degree to which states attempt to shift public welfare recipients onto the SSI roles. Chapter 3 investigates the consequences of changing the initial screening process stringency for the Social Security Disability Insurance (DI) program on the labor force participation decisions of older men.

Changes in the administration of SSI for disabled children in 1990 transformed the program into a substantial cash welfare plan for low-income children. Chapter 1 investigates how disability benefit availability and generosity affect the probability that a family discovers a health problem in their child and moves onto the SSI rolls. First, I show that the liberalization of child SSI eligibility was followed by a substantial reevaluation of the health status of low-income children by their parents. Then, using variation in the net SSI cash benefit a family eligible for Aid to Families with Dependent Children (AFDC) can receive because of the interaction of the SSI and AFDC benefit schedules, I find that increases in SSI benefits raise the likelihood that a family identifies a chronic impairment in their child; this health condition is almost always a mental illness. The discovery often leads to SSI receipt for the child and a reduction in labor market activity of the parent. Increases in SSI benefits contribute to the movement of children into special education programs and the shift of school resources toward the treatment of the disabled. My results indicate that targeting government assistance based on disability status induces large changes in the behavior of low-income families.

The liberalization of the child SSI program in 1990 allowed many children receiving assistance from AFDC to join the SSI rolls instead. Due to differences in the federal government funding rules for these two programs, state governments had an incentive to encourage the transfer of children from AFDC to SSI. Chapter 2 calculates the financial gain to states of moving a child within the welfare system and presents empirical evidence that states do consider their own financial circumstances when shepherding children through welfare programs. States suffering from fiscal troubles are more likely to transfer children from AFDC to SSI, and states that pay a higher proportion of AFDC benefits to their citizens are more likely to encourage this movement. This evidence is important not only to understand the large growth in SSI in the early 1990s but also to predict state responses to the recent radical welfare reform.

Chapter 3, written with Jonathan Gruber, investigates the effects of trying to mitigate moral hazard in the DI program by raising the stringency of the screening process for applicants. We do so by studying a dramatic increase in rejection rates for the DI program in the late 1970s, which varied substantially across the states. We find that each 10% rise in denial rates led to a statistically significant 2.8% fall in labor force non-participation among 45-64 year old males. We also find that denial rate increases effectively targeted their incentive effects to more able individuals, according to an anthropometric measure of disability.

Chapter 1

Is Disability Endogenous? The SSI Disability Program and the Health of Children

1.1 Introduction

U.S. policy towards the low-income population over the past twenty years has moved in two distinct directions: assistance programs for the able-bodied are becoming less generous or disappearing as programs targeted for the disabled expand. Many states have eliminated General Assistance for adults without dependent children while the remaining states now provide little more than short-term emergency aid. Meanwhile, driven by the growth in adult disability benefit recipients, Supplemental Security Income (SSI) passed Aid to Families with Dependent Children (AFDC) as the largest government cash welfare program.[1] Assistance for children is following a similar trend; real AFDC benefits have been falling over the last twenty years while disabled child SSI payments are indexed for inflation.

As government policy tilted toward health-based assistance, the number of children suffering from disabling health problems increased dramatically; the percentage with activity limitations caused by chronic health conditions jumped from 2.7% to 6.6% over the period 1970 to 1993 (U.S. Department of Health and Human Services, various years). This growth does not reflect a deterioration in the living environment but a radical reinterpretation of what constitutes child illness by physicians, parents and educators. Known as the "new morbidity" in the public health literature, psychological impairments barely known before this time such as learning disabilities, emotional problems and behavioral disorders supplanted conventional medical illnesses as the major health problems of children. Spurred by mental health advocates and legislation such as the Education for All Handicapped Children Act of 1975, schools took on much of the responsibility for detecting and treating these mental illnesses; by 1993,

[1] In 1993, SSI paid $23.6 billion dollars in cash benefits while AFDC paid $22.3 billion (U.S. House of Representatives [1994]).

46% of all students receiving special education services were diagnosed with specific learning disabilities caused by mental conditions (U.S. Department of Education [1995]).

In this era of changing health norms, the federal government extensively redesigned the SSI child disability program. After a Supreme Court mandated liberalization of the disability standard in 1990 and a revision that same year of the procedures used to evaluate child mental disorder claims, the number of children receiving cash benefits from SSI exploded; caseloads swelled from under 300,000 in 1989 to over 900,000 at the end of 1994, and benefit payments increased from $1.2 billion to $4.5 billion per year. Although recipient growth has slowed somewhat since 1994, if the program remains unchanged the number of children receiving SSI is projected to reach almost two million at the beginning of the next century (General Accounting Office (GAO) [1995a]). This expansion is the largest single liberalization of a public disability transfer program in the last thirty years.[2]

At the same time, the rate of identification of child mental disorders accelerated again after a lull in the 1980s. The new increases were dominated by the emergence of Attention Deficit-Hyperactive Disorder (ADHD), a neurobehavioral illness that inhibits a child's ability to concentrate and remain still, as the most common mental impairment in children.[3] Two million children were treated for ADHD in 1993, twice as many as 1990 (Swanson *et al.* [1995]). Consumption of methylphenidate, a central nervous system stimulant usually prescribed to control ADHD, increased by four times between 1990 and 1995 so that between three to five percent of all schoolchildren are on the medication (*New York Times,* October 21, 1995).[4]

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[2] The major government disability programs are Social Security Disability Insurance (DI), Workers Compensation and SSI. Only SSI extends benefits to children with disabilities. See Burkhauser and Haveman [1982] for an overview of these programs.

[3] ADHD is also commonly called Attention Deficit Disorder (ADD).

[4] Methylphenidate is better known by its brand name Ritalin. This powerful drug is classified by the U.S. Drug Enforcement Agency as a Schedule II controlled substance, the same category as methamphetamine and cocaine. Prescriptions can only be refilled after monthly doctor's visits. See U.S. Department of Justice [1995] for an overview of the growth in methylphenidate use.

This paper examines several questions regarding how changes in the availability and generosity of SSI affect the probability that a family identifies a health problem in their child and moves onto the SSI rolls. First, did the liberalization of the disability standard in 1990 contribute to the growth in the identification of child health impairments? The prospect of benefit receipt could compel families to examine the health of their children more closely and to be more willing to accept the shifting views of the time of what constitutes illness. Second, how sensitive are these health status assessments to the amount of SSI benefits available to the family? Higher potential SSI payments might also induce families to reevaluate their child's health. Finally, if there is evidence that SSI parameters contribute to the decision of low-income families to downgrade their child's health, what are the consequences of classifying these children as disabled? For example, schools are obligated by law to provide much of the treatment of impaired students; did the SSI expansion of 1990 or benefit increases funnel more children into special education or other treatment programs?

Using data from several years of the National Health Interview Survey (NHIS) and the March Current Population Survey (CPS), I track the disability trends of children during the late 1980s and early 1990s to determine if the SSI disability standard changes in 1990 contributed to the parent-reported deterioration of child health over this time period. I provide some suggestive evidence that the SSI liberalization is linked with changes in the disability rates of children from low-income families.

However, the bulk of this paper is devoted to studying the effects of SSI benefit generosity on the behavior of low-income families. Empirical studies of adult disability transfer programs are plagued with difficulties in finding plausibly exogenous variation in benefit generosity to identify how individuals react to these potential payments. I exploit a source of benefit variation that is unique to child SSI: the amount of net SSI benefits a family eligible for assistance from AFDC can receive created by the interaction of the SSI and AFDC benefit schedules. I find that AFDC-eligible families are sensitive to the amount of extra SSI benefits they can obtain; after the SSI liberalization, families with higher potential SSI benefits are more likely to receive SSI and much more likely to begin to report that

they have a child with a mental impairment. Many of these children begin attending special education classes. Higher potential SSI benefits also increase the probability that the head of the family will curtail work effort.

There are important implications of these results for the design of social insurance policy. When choosing a method for targeting welfare assistance to the needy, the government wants to find a tagging device that uses immutable characteristics to decide eligibility for benefit receipt. The evidence from this study suggests that health status is a fluid concept and that family motivation plays a role in labeling a disability; as mental illness becomes the dominant impairment in adults as well as children, my estimates imply that the efficacy of the health-based welfare system will deteriorate even further. The impact of this system is felt by all levels of government; most significantly, the child SSI expansion is contributing to the shift of local school resources away from regular instruction and toward the treatment of the newly disabled.

The paper proceeds as follows. In Section I, I provide background on the SSI program and its recent changes. Section II discusses the data and trends in the reported health of children in the 1990s. In Section III, I present the empirical strategy, and Section IV reviews the results on the sensitivity of SSI receipt, health outcomes, labor force participation measures and special education usage to SSI benefit generosity. Section V concludes.

1.2 SSI and the 1990 Expansions

SSI is a government transfer program for the low-income elderly and disabled, created to federalize a patchwork of state assistance programs and administered by the Social Security Administration (SSA). Recipients must meet an income and asset test and people under 65 years old must also pass a disability screening. Cash payments are relatively generous compared to other public transfer programs. Benefits consist of a monthly maximum federal grant, $468 in 1995, and an optional

state supplement.[5] About twenty states provide supplements to SSI children; however, most are only a few dollars per month. Recipients are also eligible for Food Stamps and Medicaid coverage.[6]

Including disabled children in the program was almost an afterthought when Congress created SSI in 1972. The disability standard, which is the same as for the DI program, is defined with adults in mind in terms of how much a person's impairment must hinder work capacity in order to be eligible for benefits. First, the SSA compares a person's health condition with a list of conditions that automatically qualifies the applicant for benefits; if the impairment is not listed, then the SSA must determine if the ailment is severe enough to prevent the applicant from working. Since children are not expected to be able to work, the SSA must use a different standard. Before 1990, children were granted benefits only if their disability met the listing; however, the Supreme Court, in the 1990 case *Sullivan* v. *Zebley*, ruled that holding children to a stricter disability standard than adults was illegal and ordered the SSA to develop a second stage of evaluation, similar to determining if adults can work, to judge whether an impairment inhibits a child's ability to pursue age-appropriate activities. Known as the Individualized Functional Assessment (IFA), for most children this activity is attending school.[7]

The same year as the *Zebley* decision, the SSA released regulations revising the procedures used to evaluate child mental impairment cases. The new rules more clearly define how the SSA considers claims of new mental illnesses such as ADHD and developmental disorders. The SSA also modified the type of evidence used to judge the damage of mental illness; less emphasis is placed on the testimony of

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[5] In the mid-1990s, the family income limit for SSI eligibility was around $11,000 per year, and the liquid asset limit was $2000. Benefits are indexed yearly for inflation and reduced as a recipient's other income increases. Unlike AFDC, SSI benefits and income cutoffs are not adjusted for family size.

[6] States are allowed to set more restrictive criteria for receiving Medicaid coverage than cash benefits from SSI. Twelve states, known as 209(b) states for the section of the law that allows for the tighter eligibility requirements, set more binding standards for receiving Medicaid either by requiring a lower income limit or a higher disability standard than the cash benefit criteria. It is difficult to exploit these differences in state Medicaid coverage rules as differences in SSI generosity because these state rules do not change much over time.

[7] As part of the *Zebley* settlement, the SSA agreed to allow children who applied for SSI during the previous ten years to reapply under the new standard and receive retroactive compensation if they were found eligible for benefits. About 300,000 children reapplied.

medical professionals and more weight is given to the information parents, teachers and counselors provide about the child's condition. As with the IFA, the new mental impairment regulations focus on how disabilities affect a child's performance in school.

These changes transformed SSI into a major welfare program for children. Figure 1 plots the evolution of child SSI caseloads from the program's inception in 1974 until 1994. For its first fifteen years, the program grew slowly but steadily until about 300,000 children received assistance. After 1990, the number of applicants more than quadrupled and the acceptance rate climbed from one-third to over one-half, allowing over 600,000 new children onto the SSI rolls. This liberalization profoundly altered the case mix of the SSI population. Table 1 presents data on the primary diagnosis of SSI recipients from 1988 to 1994. Although the number of recipients in every diagnostic class rose significantly beginning in 1990, the growth in the number of children receiving SSI for mental conditions other than mental retardation dwarfs the other categories; most of these recipients suffer from ADHD.[8] The percentage of new allowances with physical disabilities fell considerably while the proportion with mental retardation remained about constant. The implementation of the IFA accelerated the movement of the case mix toward mental disorders; over eighty percent of the participants approved for benefit receipt at the IFA stage suffer from mental impairments, and half of all allowances for mental disorders other than retardation were made at the IFA level (GAO [1995b]).

In the popular media, most of the coverage of the growth of child SSI is devoted to investigating the surge in mental condition cases and charges that families are fraudulently claiming their children are impaired to receive benefits. Some opponents of the expansion accuse parents of coaching their children to fake psychological problems in order to receive "crazy checks."[9] Studies searching for indications of

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[8] See National Academy of Social Insurance [1995] for a finer breakdown of the impairment distribution of SSI recipients.

[9] Articles criticizing SSI occasionally highlight accusations even worse than fraud. In Boston, a newspaper prominently ran a story about a family accused by police of intentionally injuring their child in the hopes of receiving SSI (*Boston Globe*, January 30, 1996).

such coaching have found no evidence to support claims that large numbers of children receiving SSI are faking their illnesses (Social Security Administration [1994], U.S. Department of Health and Human Services [1994], GAO [1995b] and GAO [1996]).

There is little doubt that the children accepted onto the SSI rolls for mental disorders suffer from social and scholastic problems; questions are raised about the validity of their claims because it is difficult to demonstrate that the troubles are health related. Even among researchers and advocates of learning disabilities, there is profound disagreement over the prevalence of these impairments in children; some lament the recent over-diagnosis of mental disorders while others assert that an even larger number of children with conditions go undetected.[10] All mental health experts do agree, however, that there is no universally acknowledged definition of learning disabilities and no standard procedures for identifying the problem. When a child is doing poorly in school, there are many possible explanations for the problem, including a learning disability. Often there is no conclusive evidence of the cause; little is known about what distinguishes a learning disability from any other problem in learning.

Because of the ambiguities in diagnosing mental illness, it is difficult to draw health policy conclusions from the sensitivity of the health status of children to the presence and generosity of disability benefits. Critics of the growth of SSI can interpret these results as evidence that the definitions of mental impairments are too vague and easily manipulated by families wanting extra welfare benefits; however, child mental health advocates can argue that this sensitivity demonstrates that the low-income population is still unfamiliar with the extent of mental illness in society and needs assistance in discovering the true health of their children. This debate about the rise of the "new morbidity" is complicated and an analysis of its arguments is well beyond the scope of this paper.

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[10] Compare Adleman [1992] and Lovitt [1992].

But no matter what the health policy implications are of the relationship between disability benefits and child health, the era of the SSI program as a major cash assistance program for children will be short-lived. Spurred by the burgeoning caseload and bad press, the federal government is redesigning the child SSI program as part of its welfare reform initiative. The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 substantially toughens the disability standard for receipt of SSI benefits by children. The new law creates a child disability definition independent of the adult standard, eliminating the need for the child standard to be considered legally comparable to the adult definition; therefore, the IFA will be discontinued. Hundreds of thousands of child SSI recipients could be affected by these changes as their eligibility for benefits is re-determined over the next couple of years. Also, the number of new beneficiaries is expected to decline considerably. Although the new welfare policy slows the trend of targeting assistance to children by health status, ending AFDC as an entitlement program widens the gap in generosity of assistance programs for able-bodied and disabled adults.

1.3 Description of Data and Trends in the Health of Children

To investigate the link between the 1990 SSI reforms and changes in the reported morbidity patterns of low-income children, I use several years of data from the NHIS and the March CPS. With over 100,000 respondents per year, about 25,000 of whom are school-age children, the NHIS is the largest source of information on the health, disability status, and medical care usage of the U.S. population. It is an important tool of public health researchers in tracking disability trends over time and is used to derive many of the government's estimates of the health of the nation.[11] Although it contains a wealth of health information on individuals which I use to examine how SSI parameters affect child disability prevalence, the NHIS asks few questions about sources of income; most importantly, it does not ask respondents if they receive SSI benefits. Therefore, I turn to the March CPS to examine how SSI

[11] See Newacheck, Budetti and Halfon [1986] and Newacheck and Taylor [1992] for other examples of how the NHIS has been used to analyze trends in child health problems.

benefit generosity affects the probability of becoming a SSI participant. Surveying slightly more people than the NHIS, the familiar March CPS asks detailed questions about respondents' labor market activities and income sources, including SSI receipt. However, the CPS provides little information on the respondent's health status.[12]

All health and disability information in the NHIS is reported by the survey participant; for children, the data are provided by a parent or guardian, usually the mother.[13] Such subjective measures capture more than just the underlying health status of the child; they also reflect the parent's motivation to seek out and report information about the child's health and attitudes about the extent illness affects activities. Assuming that changes in SSI benefit generosity and availability are not correlated with shifts in the underlying health of low-income children, then a relationship between SSI parameters and the reported health of children measures the effect of the SSI program on these motivations and attitudes of parents.

The use of subjective health measures to assess the growth of disability programs is similar in spirit to Bound and Waidmann [1992]. As the number of older men receiving DI and SSI grew over the last four decades, the labor force participation rate of this group plummeted. To determine if many of the disability recipients would work in the absence of the programs, Bound and Waidmann ask if the disability roll growth was accompanied by an increase in the percentage of men that claim a health limitation. Also using data from the NHIS, they find that the number of men with work limitations does mirror the growth of DI and SSI.[14] However, without plausibly exogenous cross-section variation in the

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[12] Adults not working are asked if they are unable to work because of a health problem. No information is gathered about the health of children.

[13] 17 and 18 year olds are allowed to answer the survey themselves if they choose. Excluding those ages from the analysis does not change the results presented below.

[14] Using their estimates, half of the drop of labor force participation of men between the ages 45-54 and between one-quarter and one-third of the drop among men between 55-64 from the years 1949 to 1987 is associated with the movement of men into disability programs. They carefully argue that the deterioration of the reported health of older men cannot be attributed to changes in the underlying health of the population.

generosity of disability benefits, it is difficult to isolate the causal role of DI and SSI. Did the availability of disability benefits cause older men to reevaluate their health status and leave work? Or did the prevalence of health conditions and the disability roll move together because of changing attitudes in society and the medical profession about the extent of disability over the last four decades that simultaneously affected disability reporting and labor force participation decisions? With my variation in SSI benefit generosity that I use below, I am able to more reliably gauge the effect of disability program parameters on health attitudes.

My subjective health measure is derived from a question in the NHIS which asks respondents if they have a health condition that limits their ability to perform activities.[15] Those who answer that they do have a health-related limitation are then asked what condition inhibits their actions.[16] I track the percentage of children between the ages 5 and 18 with a health impairment over the period 1987 to 1993 to look for any changes in disability reporting patterns after the 1990 SSI liberalization. Figure 2 presents the fraction of all children reported by their parents to be suffering from a health-related activity limitation for those years. Even with the growth in the last couple of decades in disability prevalence rates, children are still very healthy compared to any other age group; in 1993, 10.5% of 18 to 44 year olds and 23.4% of 45 to 64 year olds reported an activity limitation (U.S. Department of Health and Human Services [1994]). The graph demonstrates the substantial rise in reported child health problems beginning after 1990; from 1990 to 1993 the activity limitation rate rose from 6.5% to 7.8%. With over

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[15] The question reads, "Is _____ limited in ANY WAY in any activities because of an impairment or health problem." The four coded responses are "Unable to perform major activity," "Limited in kind or amount of major activity," "Limited in other activities" and "Not limited." For children older than four years old, their major activity is attending school.

[16] There is a second question in the NHIS in which parents are asked if their school-age child is inhibited in school by health limitations. The series of questions that form this school disability response reads, "Does any impairment or health problem NOW keep _____ from attending school? Does _____ attend a special school or special classes because of any impairment or health problem? Does _____ need to attend a special school or special classes because of any impairment or health problem? Is _____ limited in school attendance because of health?" The coded responses for these questions are "Unable to attend school," "Attends special school or classes," "Needs special school or classes," "Limited in school attendance," "Limited in other activities," and "Not limited." If I construct school disability measures similar to the activity limitation measures I use below, I obtain very similar estimates.

fifty million children between the ages 5 and 18 in the country, these numbers suggest that about 700,000 more children were considered disabled by their parents over that four year span.

Since SSI is a means-tested program, the probability that children from moderate and high-income families would become eligible for benefits is remote; therefore, it is unlikely that these families would be sensitive to changes in SSI availability. I disaggregate Figure 2 into the trends for different family income groups to determine if the growth in child disabilities is concentrated in one income class. Figure 3 shows the disability prevalence rates of three groups. The first is children from families that earn less than $11,000 a year, around the income-eligibility cutoff for SSI, and the second is children from families that report earnings of over $30,000 a year.[17] Because much of my subsequent analysis will focus on families eligible for AFDC, the third group consists of children from female-headed households that earn below the SSI income limit. There are large differences in the perceived health of children from low- and high-income families; by all measures, low-income children suffer many more impairments than high-income children. The growth in disability prevalence after 1990 is also higher for children from low-income families; the rate increased by 2.1 percentage points to 11.5% for children of all low-income families and 3.1 percentage points to 14% for children from low-income female-headed households. About 190,000 more children from low-income families were labeled disabled between 1989 and 1993; over four-fifths of these children live in female-headed households. Reflecting the general movement in child health trends, the percentage of children from high-income households with activity limitations also rises somewhat in this period but without the sharp increase after 1990 as for children from low-income families. Almost one-quarter of the growth of the disability prevalence rate in children in the early 1990s can be accounted by the health deterioration of children from low-income female-headed households.

[17] My results are not very sensitive to these exact income groupings.

This deterioration appears to be confined to the growth of disabling conditions; for broader measures of health, the changes are much smaller. For example, the NHIS asks parents to rate their children's health on a five point scale: excellent, very good, good, fair and poor. Figure 4 graphs the percentage of children reported to be in fair or poor health from 1987 to 1993 for children from low- and high-income families. The percentage increase after 1990 in the number of low-income children reported to be in fair or poor health is only one-fourth the size of the growth in the number of children reported to suffer from activity limitations; there is no increase for children from high-income families.

One interpretation of these trends is that a large number of low-income families were reevaluating how health problems affect the lives of their children during the early 1990s; however, another explanation is that over time the cohort leaving the sample of children 5 to 18 years old was healthier than the cohort entering the sample.[18] For example, increases over time in the number of low-birthweight children caused by advances in neonatal care technology or growing drug use among expectant mothers could drive up the child disability incidence in the 1990s. However, such a story would probably imply that the broader health measures would also deteriorate as much. To assess the potential of this cohort theory, I follow the reported health trends of one cohort of children, those who were born between 1977 and 1982; children born in these years are between the ages 5 and 18 throughout my sample period of 1987 to 1993. The percentage of these children from the different income groups that have an activity limitation caused by health is presented in Figure 5. The same patterns emerge in this cohort as for the sample of all children; disability prevalence of children from low-income families grows sharply after 1990, especially for children from low-income female-headed families; in fact, the increases are larger for this sample than for the sample of all children. Therefore, the rise in activity limitations is not caused by the entrance of more unhealthy young children into the sample over time.

[18] Yet another possible reason is that survey procedures changed around 1990; however, there is no evidence that the NHIS altered how it asked the health limitation questions.

These graphs suggest that there is a potential relationship between the SSI expansion in 1990 and the parent-reported deterioration of child health. The early 1990s was a period when a large number of lower-income families decided to reevaluate the health of their children. Since SSI awards benefits for specific activity impairments caused by health problems and not for general poor health, the divergence in the trends in broad health measures and disability indicators hints that potential SSI receipt might have motivated families to search for how health problems affected their child's schooling. However, these plots only provide suggestive evidence that the SSI expansion affected families' health decisions. As described above, many factors that influence families' judgments about the health of their children were changing over this time period. It is not possible to isolate the effects of the 1990 SSI liberalization on health reporting from these other changing factors. Therefore, to find a causal link between disability program parameters and changes in health reporting, I turn to studying the effects of variation in SSI benefit generosity on disability reporting. Then I demonstrate that benefit generosity also affects SSI participation and the labor market decisions of parents.

1.4 Empirical Strategy

1.4.1 Background

The ideal approach for identifying the effect of SSI generosity would be to use across-family variation in potential SSI benefit amounts and ask if families eligible for higher SSI benefits are more likely to downgrade the health of their children and begin SSI receipt. One candidate for this variation is the differences in the amount states supplement the federal SSI grant. Although about twenty states offer these supplements, except for a couple, they are only a few dollars a month and vary little over time. An alternative strategy is to use variation in the amount of SSI benefits a family that receives AFDC can gain. By SSI and AFDC rules, a child cannot receive benefits from both programs; if found eligible for

SSI benefits, a child is removed from the AFDC family unit.[19] The family gains the SSI benefit while their AFDC benefit amount is reduced to the benefit level a family with one fewer child receives. Families with one child end AFDC participation.

Since AFDC benefit schedules are set by states, this net gain of SSI receipt for a family receiving AFDC depends at a point in time on the family's size and state of residence. Table 2 presents an example of how the potential net SSI benefit for a family receiving AFDC varies across states and over time by displaying the maximum joint AFDC/SSI benefit levels families of one adult and two children are eligible for in Arizona and California. The top half of the table shows the benefit calculations for the two states in 1990. When a three person family from Arizona receiving AFDC has a child found eligible for SSI in 1990, the family's AFDC benefit is reduced by $720 from $3516 (column 1) to $2796 (column 2) per year after the child is removed from the AFDC unit. Since the SSI benefit they receive is $4632 (column 4), the net gain for SSI receipt is $4632 - $720 = $3912 (column 5). For a similar family in California, if a child is found eligible for SSI, the family's yearly AFDC benefit is reduced by $1608 from $8328 (column 1) to $6720 (column 2). Therefore, the net gain to SSI receipt is $4632 - $1608 = $3024 (column 5). Since the gap in AFDC benefits from moving from a three-person family to a two-person family is wider in California than in Arizona, the net gain to SSI receipt in California is smaller by $888 in 1990.

The bottom half of Table 2 presents the same benefit calculations for families in Arizona and California in 1993. Since columns (1) and (2) show that the AFDC benefit gap from moving from a three-person to a two-person family narrows in California relative to Arizona between 1990 and 1993, the difference in the net gain to SSI receipt also narrows to $558. Therefore, because of the AFDC benefit schedule changes in Arizona and California between 1990 and 1993, the gap in the difference of

[19] The SSI benefit the child receives is not counted as income when calculating AFDC eligibility and benefits of the rest of the family. Similarly, AFDC benefits are not considered when determining if a family is below the SSI income cutoff.

the SSI benefit gain between Arizona and California narrows by $330 between 1990 and 1993. These changes in SSI benefit generosity caused by revisions to the AFDC benefit schedules are the variation I exploit in identifying the effects of disability benefit generosity on health status.

Such benefit calculations appear to be relevant for a large percentage of families that consider applying for SSI; evidence suggests that many of the new SSI recipients after 1990 were previously AFDC participants. Although the March CPS does not ask specifically about AFDC receipt, 44% of families with children under age 18 in the 1994 CPS that receive income from SSI also report payments from other public assistance programs. AFDC caseload data also indicate that many of the new SSI recipients also receive AFDC. In 1990, 38% of families receiving AFDC assistance had at least one family member that was excluded from the assistance unit; in 1995, 48% of families had an excluded member. There are many reasons for having a family member excluded from the AFDC assistance unit, including having a child qualify for SSI. However, the factors other than changes to SSI caseloads do not appear to have changed much over time; 38% of families receiving AFDC in 1977, the first year data are available, had an excluded family member, the same as 1990. Therefore, assuming that the changes between 1990 and 1995 in the percentage of AFDC families with excluded family members was caused by the movement of children onto SSI, then the numbers imply that almost 450,000 families that receive AFDC had a child become eligible for SSI benefits since 1990.

1.4.2 Previous Literature

This unique interaction of the AFDC and SSI benefit schedules allows me to use arguably exogenous variation in SSI benefits available to AFDC-eligible families to identify the effect of disability benefit generosity on a family's health status and labor market decisions. The empirical literature examining the incentive effects of adult disability programs is hindered by a lack of exogenous variation in the amount of disability benefits available to individuals. There are many studies that attempt to use cross-sectional variation in DI benefits to measure the effect of DI on the labor force participation

decisions of older men. Parsons [1980] finds a large effect of potential DI benefit levels on work decisions, implying that increases in DI generosity in the 1960s and 1970s can explain all of the substantial drop in the labor force participation rate of older men during that time. However, potential DI benefits are a non-linear function of a worker's past earnings history; low-income workers receive a higher replacement rate from DI. Workers with high potential DI replacement rates might be more likely to leave the labor force to receive DI benefits; however, there are many reasons beyond DI generosity why more older low-income workers might be leaving the labor force over this time compared to high-income workers.[20]

Without a method of separating the effect of cash disability benefit generosity from other factors that influence labor force participation decisions, more recent studies turn to examining other variation in the administration of the DI program. These papers ask how changes in the probability of acceptance onto the DI rolls changes the likelihood that a person applies for benefits and leaves the labor force. Halpern and Hausman [1986] build and estimate a structural model of the decision to apply for DI and find strong effects of acceptance rates for DI on application probabilities. Parsons [1991] and Gruber and Kubik [forthcoming] study the effects on application probabilities and labor force participation decisions of a large increase in the screening stringency of DI in the late 1970s. Yelowitz [1996] examines another incentive effect of disability programs and measures the effect of Medicaid receipt on the SSI participation decisions of disabled adults. This paper returns to examining the effects of disability benefit generosity; the variation in child SSI benefits created by the interaction of the AFDC and SSI benefit schedule is much cleaner than cross-sectional variation in DI benefits. Therefore, this work provides credible evidence on how disability benefit generosity influences decision making.

Empirical Approach

[20] See Bound [1989] for a detailed critique of using cross-sectional variation in potential DI benefit generosity.

I estimate regressions to determine if families with a higher potential SSI benefit are more likely to downgrade the health of their children and begin SSI receipt. The individual-level equations have the form:

$$(1)\quad Disability_{ijkt} = \alpha + \beta_1 SSIGain_{jkt} \cdot Female_i + \beta_2 AFDC_{jkt} \cdot Female_i + \beta_3 Female_i + \beta_4 SSIGain_{jkt} + \beta_5 AFDC_{jkt} + \beta_6 X_i + \beta_7 State + \beta_8 Year + \beta_9 FamilySize + \varepsilon_{ijkt}$$

where subscript i corresponds to individuals, j denotes state of residence, k represents family size and t indexes years. $Disability_{ijkt}$ is an indicator that the child suffers from a disabling health condition. $SSIGain_{jkt}$ is the maximum amount of benefits a family receiving AFDC can gain by obtaining SSI given their family size, state and year. $Female_i$ is a dummy variable for the child being in a female-headed household. $AFDC_{jkt}$ is the maximum amount of AFDC benefits the family of the child is eligible given the family's state of residence, size and year. X_i denotes a vector of demographic characteristics including a sex dummy, race dummies and age dummies; *State* is a set of state dummies. *Year* is a full set of year effects. *FamilySize* is a set of family size dummies, and ε_{ijkt} is the error term.

The main coefficient of interest is β_1, measuring the relationship between child health and net SSI benefit generosity for children from female-headed households. Since my specification includes year, state and family size dummies, this is a modified version of the "differences-in-differences" strategy of identifying the effect of net SSI benefits on health indicators by within-state changes in the AFDC benefit schedule over time. It is a modified version because I also include an in-state control group in the sample, children from non-female-headed families for whom the SSI benefit differences caused by AFDC benefit schedule changes should have little significance, both to increase the precision of my estimates and to control for AFDC benefit changes that might be correlated with general state health trends.

I also include a measure of the maximum AFDC benefit level the child's family is eligible given their state of residence and family size. This variable controls for the effects of changes in AFDC benefit

generosity that occur when a state revises its AFDC benefit schedule in a way that alter the gaps between benefit levels for different family sizes that affect the net SSI benefit amounts. The sensitivity of health measures to SSI parameters should be larger after the SSI liberalization than before; therefore, I estimate the regressions for a *before* period of 1987-1989 and an *after* period of 1990-1993.

The means of the NHIS data for the different samples used in the regressions are reported in Table 3; these data were created by joining together observations of children between the ages 5 and 18 from 1987 to 1993. Children from one child families are omitted from the sample. Since a family with one child that begins receiving SSI loses AFDC assistance, the parent loses Medicaid coverage; therefore, the SSI benefit gain variable does not accurately reflect the marginal benefit of SSI receipt for those families. The table of means shows that children from female-headed families are more likely to be older, nonwhite and live in larger families than the rest of the population. By all the health measures listed, they also tend to have significantly more disabling conditions.

Table 4 presents similar means for the CPS data, which are used to determine the sensitivity of SSI receipt and labor force participation measures to SSI benefit generosity.[21] Because the dependent and independent variables of interest in the CPS only vary at the family level, the household is the unit of observation in these data and not the individual child as in the NHIS. The two data sets appear comparable; families that are female-headed are more likely to have larger families, be nonwhite and receive SSI and other public assistance benefits. The heads of female-headed families are also less likely to be in the labor force or work full-time.

1.5 Results

1.5.1 Basic Results

[21] Since the CPS questions I am interested in are asked about the previous year. I join together the 1988 through 1994 March CPSs to study the years 1987-1993.

The estimates for equation (1) are reported in Table 5; the dependent variable is an indicator for the parent reporting that the child has a health condition that limits activities. For the sample of children after the SSI liberalization in column (1), higher potential SSI benefits increase the probability that children report an activity limitation.[22] The coefficient for the net SSI benefit gain variable for children from female-headed households is positive and statistically significant from zero. The AFDC benefit coefficient for children from female-headed families is also positive; however, the point estimate is small. The SSI and AFDC benefit parameters have little effect on children from other types of families. Male children are much more likely and nonwhites are slightly less likely to have a reported disability.

In column (2), I show the results of the same regression for the sample of children before the SSI liberalization. The coefficient on the net SSI benefit for female-headed families is small and not statistically different from zero. The AFDC benefit coefficient is again positive and statistically significant from zero. The other coefficients are very similar to the results from the *after* sample. The pattern of estimates for the different time periods is consistent with the story that welfare-eligible families respond to SSI benefit differences created by the interaction of the SSI and AFDC benefit schedules. Female-headed families react to these benefit differences, and this sensitivity only begins after the SSI expansions in 1990. The coefficient on the SSI net benefit variable for children of female-headed families from column (1) is quite large; a 10% increase in the benefit raises the rate of activity limiting disability by 0.9 percentage points, translating to about 60,000 more children labeled with an impairment.[23]

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[22] Since there is more than one child from the same family in my samples, I correct the standard errors of all regressions by allowing for intra-family correlation in the error term and the heteroscedasticity associated with linear probability models.

[23] Since the sample period being studied is a time when many new families are considering application for SSI because of the new rules, these estimates might overstate the effect of benefit generosity on the probability that a family ever downgrades their child's health or receives SSI. Some of the estimate might be capturing the effect of higher benefits on the speed in which families reevaluate their child's health or apply for benefits.

Since parents of children identified with activity limitations are asked to specify the type of health condition that impairs their child, I examine which illnesses are sensitive to changes in potential SSI benefits. Table 6 splits the activity limitations into mental and physical conditions. Column (1) reports the estimates of the basic regression with the dependent variable an indicator for a child being limited by a mental illness; in column (2), I present the results of the same regression with the dependent variable an indicator for a physical disability. Almost all of the sensitivity of health reporting is confined to mental illnesses; the net SSI benefit variable barely affects physical impairment reporting. This is not because the SSI liberalization only affected mental disability claims or that children have few physical disabilities. Table 1 demonstrates that the number of children receiving SSI for physical conditions grew significantly after 1990, although not as much as mental illnesses. Table 2 shows that a large percentage of the reported activity limitations are for physical impairments. However, physical impairment identification is not spurred by SSI benefit payments.

Columns (3) and (4) examine the effect of net SSI benefit changes on mental illness reporting separately for boys and girls. As described above, the growth in the 1990s in the diagnosis of mental impairments such as ADHD has been dominated by the increase in the number of boys with these illnesses. The coefficients on the net SSI benefit variables in columns (3) and (4) show that disability benefit changes predominately influence the beliefs about the mental health of boys.

Next I turn to determining if net SSI benefit differences affect SSI receipt using data from the CPS. Families are not asked in the CPS if children under age 16 receive SSI income; however, the family does report a cumulative SSI income measure. Using a dependent variable of an indicator whether anyone in the family has SSI income, I estimate the same equation as reported above for families with children in the CPS. Since the observation of the CPS data is at the family-level instead of the child, some of the control variables are altered; there is no dummy for a male child, and the age dummies are for the age of the oldest child. Column (1) of Table 7 presents the results of the estimation on the sample of children after the SSI liberalization. The coefficient on the net SSI benefit variable for female-

headed families is positive and statistically significant from zero; a 10% increase in the SSI benefit available to female-headed families increase SSI receipt by 0.77 percentage points, 21,000 families. The results of the regression with the sample of children before the SSI liberalization are reported in column (2). As with the disability status regressions, there is no effect of net SSI benefits on SSI receipt for this *before* sample.

Therefore, the basic regressions of net SSI benefits on health indicators and SSI receipt demonstrate that the SSI expansion contributed to the identification of disabilities in children and their movement onto the SSI rolls. Following the trend of this time in the growth in mental impairment identification in children, especially for boys, higher SSI benefits spurred female-headed families to reevaluate the extent mental problems affected their child's schooling. The questions to answer now are what effect does this re-labeling have on the child and the family? For example, does the family of the newly identified child alter its labor supply decisions? Does the child begin special education classes?

1.5.2 Implications

Since the amount of SSI benefits available does sway benefit receipt decisions, the next step is to examine if it also affects a family's labor force decisions. One of the justifications for extending disability insurance protection to children is to allow families with sick children to leave work to care for them. Therefore, it would not be a surprise to see that work decisions of the parent of female-headed families are also sensitive to benefit generosity. Column (1) presents the results of the usual regression with the dependent variable an indicator if the parent worked at any time during the year. Net SSI benefit increases have a negative and marginally significant effect on the decision of the parent to work; AFDC benefit increases have a stronger and very statistically significant negative impact on work. The coefficient suggests that a 10% increase in the net SSI benefit causes about 27,000 heads of female-headed households to leave the labor force.

Beyond moving out of the labor force, increasing SSI benefits also affects the number of hours worked by female household heads. Column (2) shows the results for a regression with the dependent variable hours worked per week for a sample of families with heads that work non-zero hours. Net SSI benefits and AFDC benefits have a negative and statistically significant effect on hours worked for female heads; a 10% increase in SSI benefits implies a 0.67 hour reduction in work effort. In column (3), I add the rest of the sample and estimate a Tobit hours model. Not surprisingly, the effect of SSI and AFDC benefits on hours worked is strengthened using the Tobit model compared to column (2).

One of the largest potential impacts of the SSI expansion is that it moved children into special education programs. If parents are worried that their child is inhibited in school because of a health problem, schools are obligated to test the child for learning disabilities; a positive finding binds the school to provide the treatment the child needs to progress in school. The appeal of SSI benefits might persuade parents to have their child tested; even if the child never receives SSI, the program might have large consequences for schools if that testing reveals learning problems. The number of children receiving services from special education programs has grown considerably since 1990; from the 1989 to the 1993 school year, 500,000 more children were being treated in special education classes, an increase of over 10%. Sixty percent of the growth was accounted for by children diagnosed with learning disorders (U.S. Department of Education [1995]).

To determine if the SSI expansion contributed to the growth in the identification of children needing special education programs, I measure the effect of the net SSI benefit variable on the reported special education usage of the NHIS child sample. One of the responses of the school limitations question of the NHIS is whether the child attends special education classes.[24] Column (1) of Table 9 presents the results of that regression for the sample of children in the NHIS from 1990 to 1993. Net SSI benefits have a positive and marginally significant effect on the use of special education classes by

[24] The wording of the question only precludes parents that answer that their child is unable to attend school from answering that their child attends special education classes.

children from female-headed families. AFDC benefits have little effect on special education usage for female-headed households, and both measures have little impact on other families. As with the other health measures, boys are much more likely to attend special education classes, and nonwhite children are slightly less likely to be treated.

The estimated coefficient on the net SSI benefit variable for female-headed families implies that a 10% increase in the SSI payment increases the number of children receiving special education services by 0.42 percentage points. This translates to a growth of about 30,000 children. As with the other health indicators, the effect of the SSI benefits is much smaller for the *before* sample. Column (2) demonstrates that the effects of SSI benefits on special education usage can only be found for the sample of children after the SSI liberalization.

Because special education instruction can be very expensive, these extra recipients of treatment can add a large burden to school budgets. However, it is difficult to estimate the marginal cost of these additional special education recipients. When estimating the impact of the increase in the number of low-birthweight children on special education costs, Chaikind and Corman [1991] employ the average of special education services; using their number of $4350 per child per year in 1990 dollars, the additional education cost of these 30,000 children moving in special education is about $130 million, about the cost of SSI benefits if these children go onto the SSI rolls. However, it is unlikely that the average cost measure is appropriate for this calculation; treatment of mental disabilities, especially the less severe ones such as ADHD, are not as expensive as many physical impairments. These children do not need the special facilities and constant supervision that many children with physical infirmities need.

But even if the marginal cost of these additional students in special education programs is a fraction of the $4350 average cost, the amount is still significant for local schools. As obligations for services for students with disabilities grow, it is difficult for schools to meet these responsibilities and still provide the same amount of services to the non-disabled student body. Cullen [1996] finds that there is a one-for-one tradeoff of special education obligations and spending on normal classroom activities.

Therefore, increases in the availability and generosity of SSI benefits are accelerating the shift of local resources away from traditional school instruction and toward treatment of the disabled.

1.6 Discussion

Disability insurance programs have grown rapidly over the past thirty years; almost half of all cash welfare assistance from the federal government is directed toward people who can pass a medical screening. Health status has replaced family structure as the mechanism of choice for targeting aid to the needy. This paper examined the effect of the extension of the health-based welfare system to assistance for children through the liberalization of the SSI child disability program. I found that, after the SSI expansion, the reported health status and probability of SSI receipt of children from disadvantaged families was very sensitive to the amount of SSI benefits available; an increase in the amount of SSI benefits a family can receive raised the probability that a child in that family will have a health impairment and begin SSI participation. Often the child begins receiving special education services. More generous SSI benefits also allow the head of these families to leave the labor force or cut back on work hours. My results suggest that many health and labor market decisions of are affected by using disability status as a targeting mechanism for giving assistance to children.

My results indicate that most of the sensitivity of health status to SSI benefit generosity is confined to the identification of mental impairments. With mental health science in its infancy and the definitions of mental illness vague, motivation plays a large role in determining whether a family decides that their child suffers from a mental condition. As mental impairments move toward becoming the dominant illnesses of adults also, my estimates imply that the incentive effects of disability benefit generosity should grow. Although only 26% of DI awards in 1992 were for mental conditions, that is a significant increase from 1982 when only 11% of DI claims granted benefits were for mental impairments. The adult SSI program resembles more the child program than DI; 56% of cases in 1993

were for mental disabilities, and 64% of the growth in adult SSI disability caseloads was accounted for by the growth in the number of mental illness allowances.

Increases in SSI benefit generosity induce more children to be placed in special education programs. Above, I outlined the fiscal consequences of these movements; however, it is difficult to know how being labeled disabled affects the child. Do these newly identified children receive the treatment they need to overcome their problems and excel in school or are they shunted aside and considered a hopeless case? There is little data on how identifying a disability affects a child's schooling achievement. Targeting welfare benefits for children by disability status affects how the health of many children is perceived; however, it is not possible to determine if these changes in attitude have any long-term effect on the child.

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Figure 1.1: Number of Children Receiving SSI Benefits: 1974-1994

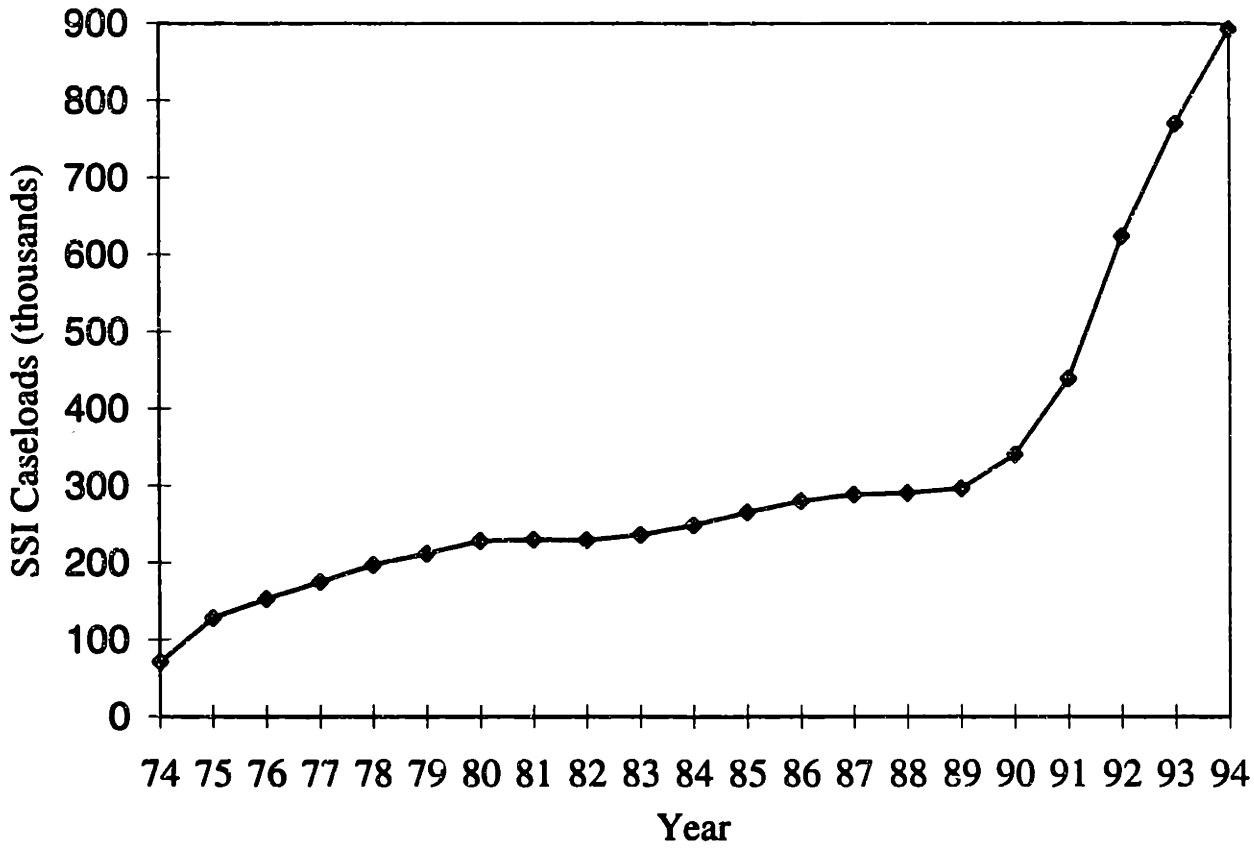

Figure 1.2: Percentage of Children with an Activity Limitation: 1987-1993

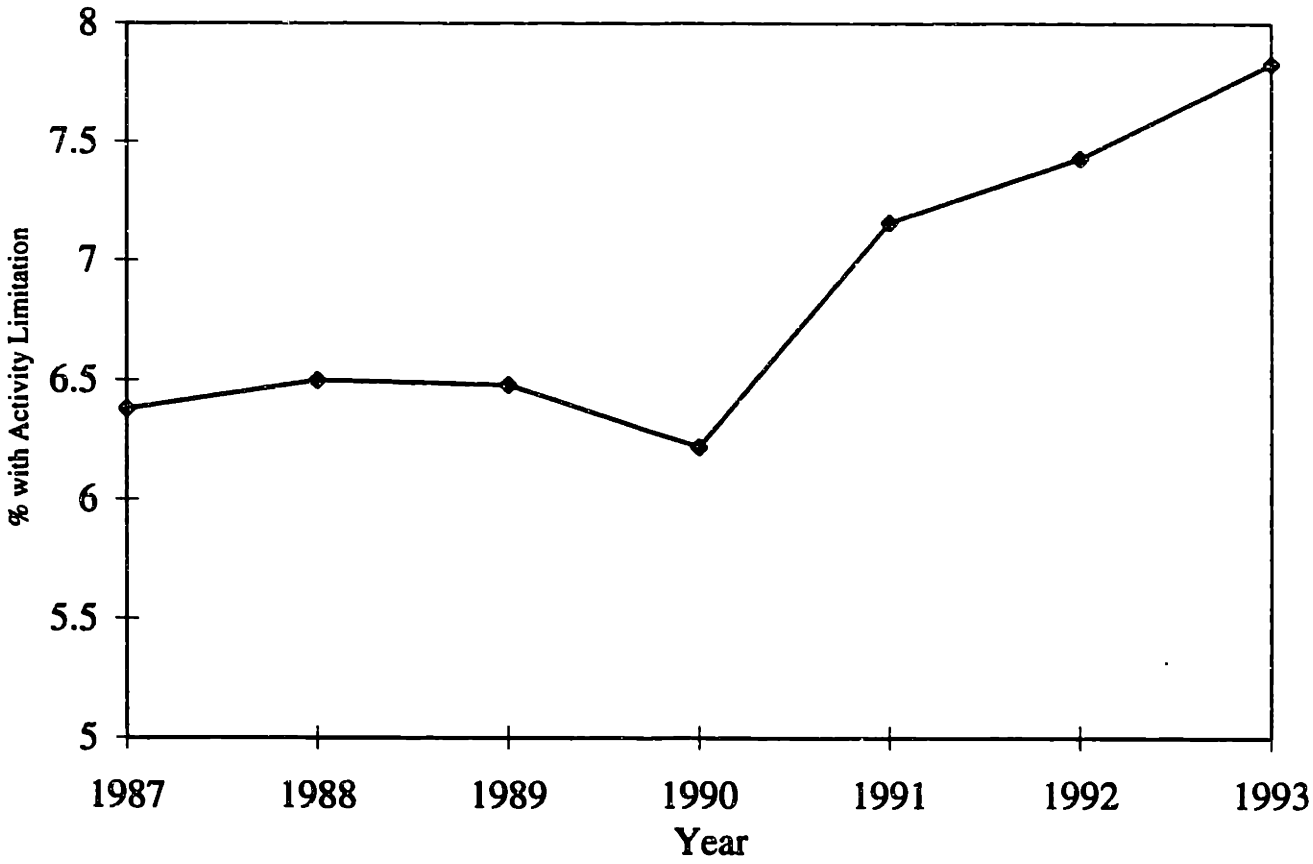

Figure 1.3: Percentage of Children with an Activity Limitation by Income Group: 1987-1993

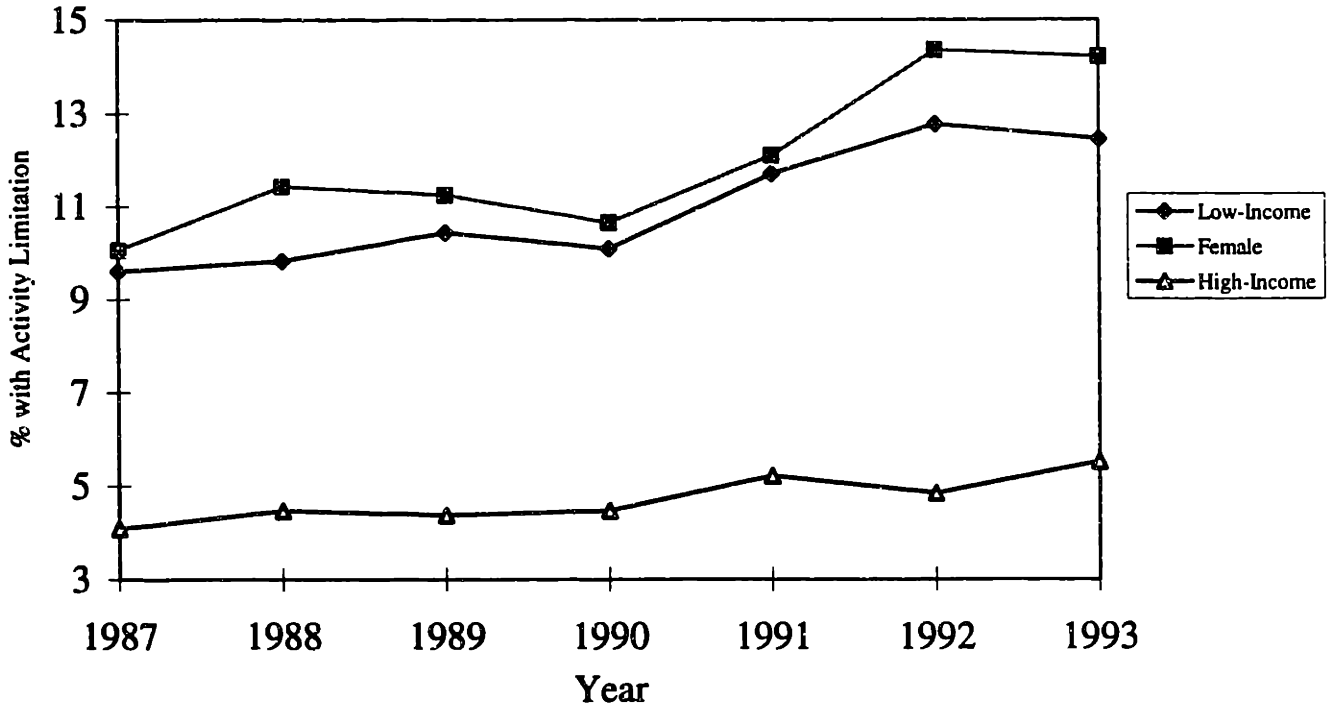

Figure 1.4: Percentage of Children Reported to be in Fair or Poor Health by Income Group: 1987-1993

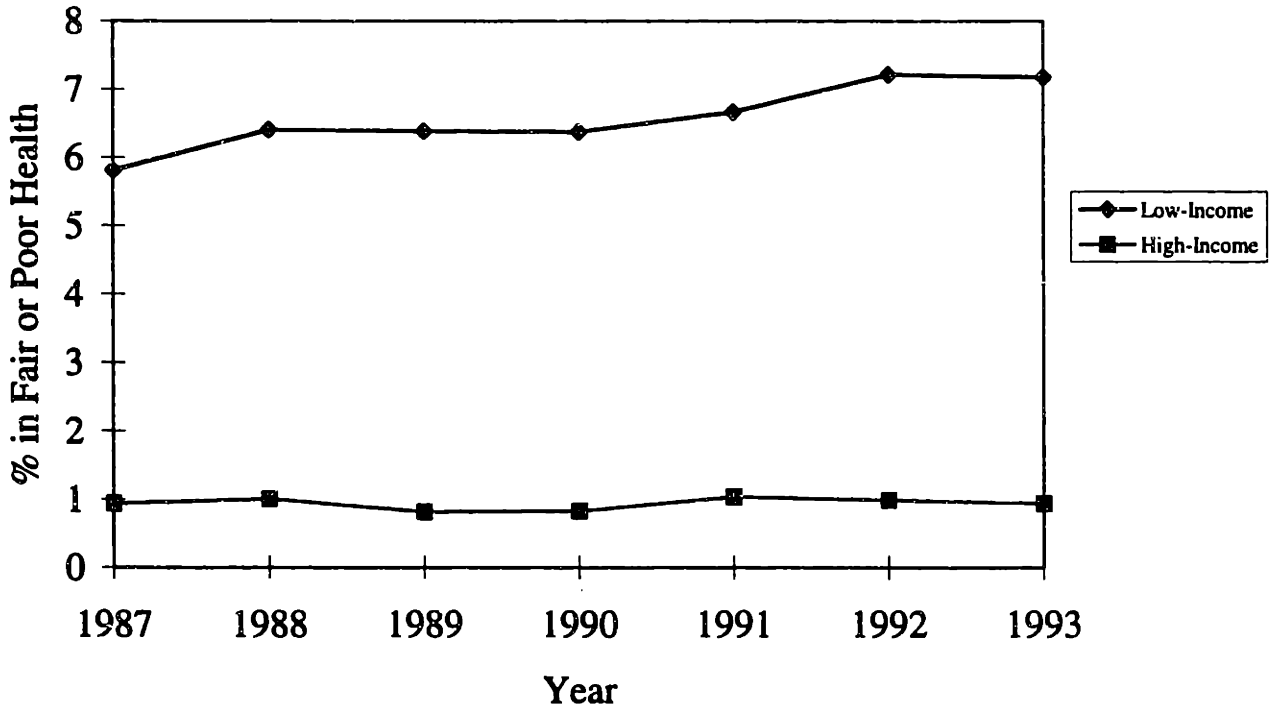

Figure 1.5: Percentage of Children with an Activity Limitation, Cohort of Children Born 1977 to 1982

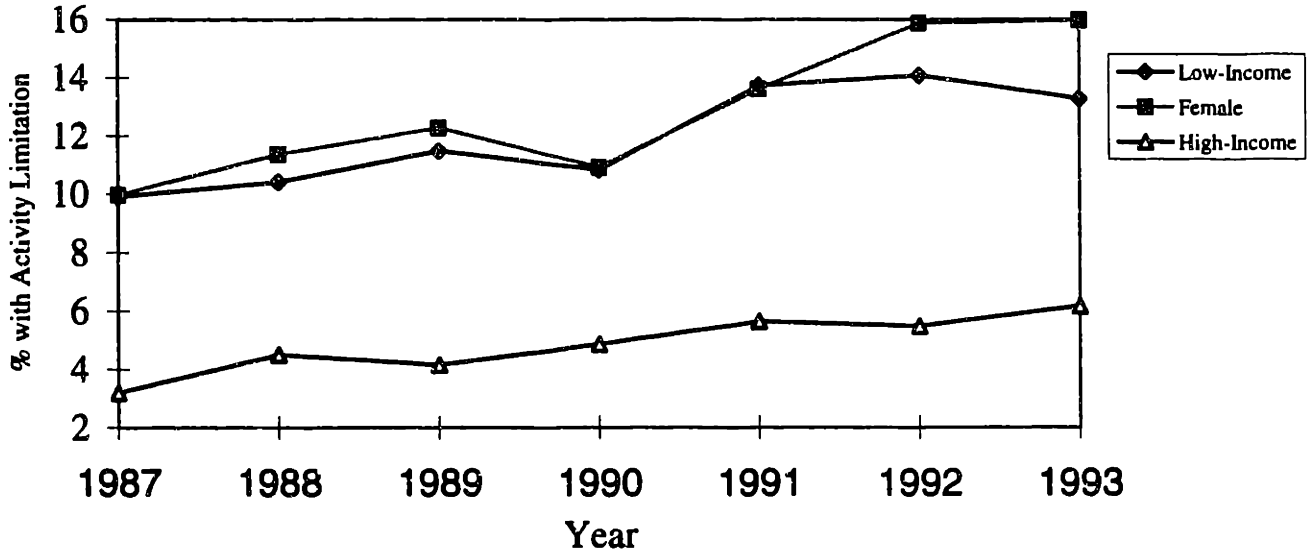

Table 1: Children Receiving SSI by Primary Diagnosis, 1988-1994

Diagnosis	1988	1989	1990	1991	1992	1993	1994
Total with Diagnosis	260,300 (100%)	262,300 (100%)	296,700 (100%)	376,300 (100%)	559,700 (100%)	704,500 (100%)	812,400 (100%)
Physical Disorders	134,900 (51.8%)	135,500 (51.7%)	151,300 (51.0%)	178,700 (47.4%)	238,900 (42.7%)	286,900 (40.7%)	315,100 (38.8%)
Mental Retardation	108,600 (41.7%)	110,200 (42.0%)	121,800 (41.1%)	152,700 (40.6%)	231,300 (41.4%)	281,400 (39.9%)	317,400 (39.1%)
Other Mental Disorders	16,800 (6.5%)	16,600 (6.3%)	23,600 (8.0%)	44,900 (11.9%)	88,900 (15.9%)	135,200 (19.3%)	179,900 (22.1%)

Source: *Annual Statistical Supplement to the Social Security Bulletin*, various years. Not all cases have a coded diagnosis; percentages are of caseloads with available diagnosis.

Table 1.2: AFDC and SSI Benefits for Arizona and California, 1990 and 1993

	AFDC Benefit for Three Person Family	AFDC Benefit for Two Person Family	Difference (1) - (2)	SSI Benefit	Net SSI Benefit (4) - (3)
			1990		
	(1)	(2)	(3)	(4)	(5)
Arizona	$3516	$2796	$720	$4632	$3912
California	$8328	$6720	$1608	$4632	$3024
Difference					$888
			1993		
	(1)	(2)	(3)	(4)	(5)
Arizona	$4164	$3300	$846	$5208	$4362
California	$7284	$5880	$1404	$5208	$3804
Difference					$558
Change in the Difference					-$330

Table 1.3: Means of the NHIS data, 1987-1993

	1990-1993		1987-1989	
	Full Sample (1)	Female-Headed (2)	Full Sample (3)	Female-Headed (4)
% Male	51.19	50.58	50.69	50.41
% Nonwhite	25.04	52.65	24.61	48.26
Age	10.84 [3.85]	10.98 [3.78]	10.96 [3.92]	11.10 [3.84]
Number of Children in Family	2.85 [1.06]	2.93 [1.12]	2.84 [1.06]	2.90 [1.11]
% Activity Limitation	7.05	10.47	6.36	8.94
% School Limitation	5.02	7.69	4.40	6.60
% Attends Special Education Class	3.44	4.81	3.10	4.35
% Physical Activity Limitation	4.67	6.28	4.34	5.91
% Mental Activity Limitation	2.38	3.59	2.03	3.03
Maximum Net SSI Benefit	4023 [436]	4039 [450]	3381 [369]	3400 [376]
Maximum AFDC Benefit	5695 [2524]	5599 [2586]	5283 [2352]	5192 [2327]
Observations	68965	11610	52914	9034

Notes: Tabulations from the NHIS, 1987-1993. Standard deviations are in brackets.

Table 1.4: Means of the CPS data, 1987-1993

	1990-1993		1987-1989	
	Full Sample (1)	Female-Headed (2)	Full Sample (3)	Female-Headed (4)
Number of Children in Family	2.52 [.829]	2.56 [.882]	2.56 [.854]	2.60 [.895]
Age of Oldest Child	10.77 [4.61]	10.96 [4.56]	11.79 [3.94]	11.91 [3.88]
% SSI Receipt	2.66	4.96	1.87	3.90
% Public Assistance Receipt	12.4	39.8	10.7	38.3
% Nonwhite	17.9	35.1	17.3	36.0
Hours Work per Week	37.59 [17.42]	23.57 [19.50]	38.16 [17.49]	24.31 [19.72]
% Work	86.3	60.8	88.25	64.08
% Work Full Time	79.6	46.9	81.51	51.15
Maximum Net SSI Benefits	4015 [439]	4021 [442]	3395 [355]	3392 [357]
Maximum AFDC Benefits	5471 [2310]	5464 [2359]	5083 [2124]	5056 [2133]
Observations	46964	10957	34030	6659

Notes: Tabulations from March CPS, 1988-1994. Standard deviations are in brackets.

Table 1.5: The Effect of Net SSI Benefit Changes on Health Indicators
Dependent Variable is a Dummy for Reporting an Activity Limitation

	After Liberalization (1)	Before Liberalization (2)
Net SSI Benefits for Female-Headed Families / 1000	.0233	.0018
	(.0114)	(.0153)
AFDC Benefits for Female-Headed Families / 1000	.0024	.0056
	(.0020)	(.0026)
Net SSI Benefits for Other Families / 1000	-.0112	-.0011
	(.0107)	(.0109)
AFDC Benefits for Other Families / 1000	-.0039	-.0024
	(.0026)	(.0029)
Female-Headed Family	-.0685	-.0581
	(.0541)	(.0629)
Male	.0270	.0217
	(.0020)	(.0021)
Nonwhite	-.0033	-.0044
	(.0032)	(.0029)
Observations	68965	52914

Notes: All regressions also include age dummies, state dummies, year dummies, and family size dummies. Standard errors are adjusted to correct for intra-family correlation.

Table 1.6: The Effect of Net SSI Benefits on Health Indicators
Mental and Physical Impairments
Mental Impairments in Males and Females

	Dependent Variable		Dependent Variable: Mental Disability	
	Mental Disability (1)	Physical Disability (2)	Males (3)	Females (4)
Net SSI Benefits for Female-Headed Families / 1000	.0184 (.0063)	.0049 (.0093)	.0256 (.0098)	.0114 (0072)
AFDC Benefits for Female-Headed Families / 1000	.0021 (.0011)	.0004 (.0015)	.0031 (.0018)	.0011 (.0012)
Net SSI Benefits for Other Families / 1000	-.0095 (.0057)	-.0022 (.0089)	-.0072 (.0087)	-.0120 (.0061)
AFDC Benefits for Other Families / 1000	-.0034 (.0015)	-.0010 (.0020)	-.0034 (.0023)	-.0031 (.0016)
Female-Headed Family	-.0721 (.0301)	.0036 (.0440)	-.1045 (.0465)	-.0407 (.0336)
Male	.0179 (.0012)	.0091 (.0016)		
Nonwhite	-.0037 (.0018)	.0003 (.0026)	-.0049 (.0029)	-.0029 (.0020)
Observations	68965	68965	35458	33507

Notes: All regressions also include age dummies, state dummies, year dummies, and family size dummies. Standard errors are adjusted to correct for intra-family correlation.

Table 1.7: The Effect of Net SSI Benefit Changes on SSI Participation
Dependent Variable is a Dummy for SSI Income Receipt

	After Liberalization (1)	Before Liberalization (2)
Net SSI Benefits for Female-Headed Families / 1000	.0192 (.0073)	-.0016 (.0105)
AFDC Benefits for Female-Headed Families / 1000	-.0009 (.0013)	-.0028 (.0018)
Net SSI Benefits for Other Families / 1000	-.0077 (.0046)	-.0030 (.0082)
AFDC Benefits for Other Families / 1000	-.0014 (.0019)	-.0017 (.0031)
Female-Headed Family	-.0457 (.0339)	.0431 (.0423)
Observations	46964	34030

Notes: All regressions also include age dummies, state dummies, year dummies, and family size dummies.

Table 1.8: The Effect of Net SSI Benefits on Various Labor Force Participation Variables

	Work in Year (1)	Hours per Week (2)	Tobit Hours per Week (3)
Net SSI Benefits for Female-Headed Families / 1000	-.0245	-1.691	-2.879
	(.0144)	(.4215)	(.6290)
AFDC Benefits for Female-Headed Families / 1000	-.0305	-.3164	-1.767
	(.0027)	(.0896)	(.1189)
Net SSI Benefits for Other Families / 1000	-.0057	.4148	.1229
	(.0095)	(.3542)	(.5711)
AFDC Benefits for Other Families / 1000	-.0023	.0496	-.1286
	(.0038)	(.1359)	(.2037)
Female-Headed Family	-.0300	1.168	1.061
	(.0686)	(2.046)	(3.010)
Observations	46964	40240	46964

Notes: All regressions also include age dummies, state dummies, year dummies and family size dummies.

Table 1.9: The Effect of Net SSI Benefit Changes on Health Indicators
Dependent Variable is a Dummy for Attending Special Education Classes

	After Liberalization (1)	Before Liberalization (2)
Net SSI Benefits for Female-Headed Families / 1000	.0104	.0019
	(.0071)	(.0109)
AFDC Benefits for Female-Headed Families / 1000	.0015	.0036
	(.0013)	(.0018)
Net SSI Benefits for Other Families / 1000	-.0028	-.0006
	(.0065)	(.0078)
AFDC Benefits for Other Families / 1000	-.0032	.0003
	(.0018)	(.0021)
Female-Headed Family	-.0344	-.0663
	(.0343)	(.0436)
Male	.0204	.0177
	(.0014)	(.0015)
Nonwhite	-.0043	-.0059
	(.0021)	(.0020)
Observations	68965	52914

Notes: All regressions also include age dummies, state dummies, year dummies, and family size dummies. Standard errors are adjusted to correct for intra-family correlation.

Chapter 2

Fiscal Federalism and Welfare Policy: The Role of States in the Growth of Child SSI

2.1 Introduction

All of the major government programs for assisting the needy are jointly administered by the federal government and the states. Typically, the federal government sets broad outlines on how the welfare program is to be managed and provides a portion of the money for its operation; the states then decide the exact structure of the benefits, deal with the recipients, and contribute the rest of the money.[1] Researchers often exploit this cross-state variation in the configuration of the programs to measure how people respond to the incentives created by the presence of the welfare system. However, little work has been done examining how states react in structuring their benefits and shepherding recipients through the welfare system to the incentives the federal government creates through the ground rules they prescribe for each program.

The two substantial cash assistance programs for the poor in the early 1990s were Aid to Families with Dependent Children (AFDC) and Supplemental Security Income (SSI): AFDC provided benefits to low-income families with minor children while SSI gives assistance to the poor with medically-certifiable disabilities.[2] Before 1990, the vast majority of children receiving welfare obtained their assistance from AFDC; however, due to a liberalization in the child disability standard for SSI mandated by the Supreme Court in the case *Sullivan* v. *Zebley*, SSI receipt became an option for many

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[1] This is a rough sketch of the framework of the Aid to Families with Dependent Children (AFDC) program. Some states required county governments to also provide financial and administrative support for AFDC.

[2] In 1993, SSI paid $23.6 billion dollars in cash benefits while AFDC paid $22.3 billion (U.S. House of Representatives [1994]). The figure for SSI includes the benefits paid to low-income people older than age 65 that do not have a disability.

more children.[3] SSI child caseloads swelled from under 300,000 in 1989 to over 900,000 at the end of 1994, and benefit payments increased from $1.2 billion to $4.5 billion per year.

The creation of a second sizable cash welfare program for children in 1990 altered the financial obligations states had to support poor children. AFDC benefits were funded by both the federal government and the states; whereas, only the federal government pays for SSI benefits.[4] The SSI liberalization allowed many children receiving AFDC benefits to obtain SSI instead, saving the states money. Therefore, states had a financial incentive to assist children receiving AFDC in applying for SSI benefits.

This paper carefully examines the incentives states face in shifting welfare recipients between programs and searches for empirical evidence that states play a substantial role in determining the caseload mix of various welfare programs. My empirical strategy involves identifying states that are in fiscal distress and seeing if those states tend to transfer children from the AFDC roles to the SSI program after the *Zebley* decision. I assume that states in poor financial condition are more likely to value the monetary gain of shifting a child and are therefore willing to bear the expense of identifying children on the AFDC rolls likely to be eligible for SSI and assisting them in applying for benefits.

Learning about the role that states play in the welfare determination process is important not only for understanding the growth in child SSI after the *Zebley* decision, but also for predicting the consequences of the recent radical reform of the welfare system. The replacement of AFDC by a series of block grants from the federal government to the states again greatly alters the financial incentives that states face. Now, instead of the federal government paying a percentage of the benefits for every child placed on the AFDC rolls, the states now bear the entire cost of placing an extra child on the program. This increases the monetary reward for a state finding a child receiving AFDC that is eligible for SSI.

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[3] See Kubik [1997] for a full explanation of the changes to child SSI in 1990.

[4] States are allowed to supplement the federal SSI benefit grant. More detailed information on the funding process for SSI and AFDC is provided below.

Therefore, if states do play a large role in guiding AFDC recipients to SSI, then welfare reform might lead to another spurt of applications for SSI from AFDC recipients.

Using state-level data on AFDC and SSI caseloads during the late-1980s and early-1990s, I find that states facing adverse revenue and deficit shocks have a higher growth of their SSI rolls relative to the size of their AFDC population after the *Zebley* verdict in 1990. The growth in SSI is concentrated in children from families who have no other children, families for which the state's monetary gain from moving a child from AFDC to SSI is the greatest. I also provide some evidence that states that must pay a larger percentage of their AFDC benefits in the early 1990s are more likely to assist the movement of children from AFDC to the SSI rolls. My results confirm that states do play a large role in identifying children for SSI receipt, and it is important to consider the incentive states face in assigning people to welfare programs.

The paper proceeds as follows. In Section I, I discuss the incentives that states and individuals face to move AFDC recipients from AFDC to SSI. Section II explains the empirical framework I use to measure the part states play in determining the caseload mix of welfare programs. Section III provides the results of this work, and Section IV concludes.

2.2 The Incentives for States and Recipients to Move Children from AFDC to SSI

2.2.1 Incentives for States

A state gains financially by having a child receiving AFDC become eligible for SSI because federal law does not allow a person to receive both AFDC and SSI. Under AFDC rules, a child newly accepted on SSI is removed from the assistance unit when calculating AFDC benefits. Therefore, a family of size k from state i has a benefit loss of:

$$(1) \qquad AFDC\ Benefit\ Loss_{i,k} = AFDC\ Benefit_{i,k} - AFDC\ Benefit_{i,k-1}$$

For example, a family of a parent and two children receives the AFDC benefit of a two-person family if one of the children is found eligible for SSI. One-child families lose all AFDC benefits.

Since states only pay a part of the AFDC benefits, the federal government also pays a fraction, they do not save all of the AFDC benefit that the family loses. The percentage of the benefits that a state pays, known as the state matching rate, is a nonlinear function of a state's per capita income relative to the average per capita income of the U.S. The formula for the match rate is:

$$(2) \qquad Match\ Rate\ State_i = 0.45 \times \frac{\left(Per\ Capita\ Income\ State_i\right)^2}{\left(Per\ Capita\ Income\ U.S.\right)^2}$$

capped at a level of 0.50 and with a minimum match of 0.17. Table 1 presents the matching rates for the states for 1994. The poorest state, Mississippi, only pays about 20% of AFDC benefits disbursed in its state whereas several of the richest states pay the maximum 50%. Although there are large differences across states in their match rates, a state's per capita income rarely changes much relative to the rest of the country over time so there are not large within-state movements of the match rates.

The gain to the state of having a child receiving AFDC become eligible for SSI is therefore:

$$(3) \qquad State\ Gain_{i,k} = State\ Match\ Rate_i \times AFDC\ Benefit\ Loss_{i,k}$$

However, since some states supplement the benefit given to a child SSI recipient, the net gain of having an AFDC recipient go on the SSI rolls is:

$$(4) \qquad Net\ State\ Gain_{i,k} = (State\ Match\ Rate_i \times AFDC\ Benefit\ Loss_{i,k}) - State\ SSI\ Supplement_i$$

Table 2 presents the average, minimum and maximum state gain in 1994 for a child from a one-, two- and three-child family becoming eligible for SSI.[5] There is a large range in the amount that states save, from

[5] All calculations are made assuming that the family has no other sources of income except from public assistance programs.

less than $100 per year to several thousand dollars per year. The table also shows that states save much more money, on average about four times more, when a child from a one-child family becomes eligible for SSI relative to a family with more than one child.[6] The average savings falls slightly as the number of children in the family increases above two. Because of the nonlinear shape of state AFDC benefit schedules with respect to family size, the largest loss of benefits for a family is if the family has one child and loses all AFDC benefits after the child becomes eligible for SSI.

Such calculations might exaggerate the gain to a state of having a child found eligible for SSI. Encouraging families to find disabilities in their children might mean that more children demand expensive special education services that are charged to the state. Kubik [1997] finds some evidence that higher SSI benefits push families to place more children in special education classes. As one state's example demonstrates below, one way to avoid this problem is for states to target only children that are already receiving some type of special education treatment in school.

2.2.2 Methods of Identifying Children

There are at least two ways that states can attempt to funnel AFDC recipients onto the SSI rolls. The first is through the state's control of the first stage of the adjudication process for SSI applicants. The initial disability determinations are done by state agencies working on a contract from the Social Security Administration. Although the disability standard for receipt of benefits is set by the federal government, there is wide variation across states in the methods used to evaluate disability claims by these state agencies, and there is believed to be wide differences in the rigor of these agencies in applying the federal standard.[7] States can be more lenient in deciding cases involving AFDC recipients in an

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[6] The savings to states for families with more than two children is similar to the savings presented for a two-child family.

[7] See Gruber and Kubik [forthcoming] for an example of work using the differences in the standards used by these agencies in evaluating Social Security Disability Insurance claims, which are also adjudicated by these agencies.

effort to help the state coffers, although the Social Security Administration does have procedures to try to check on the accuracy of the state agencies. There is little information on how these state agencies are run and no state would admit to not being completely impartial in deciding disability claims. Therefore, it is difficult to gauge to what extent states can affect who receives benefits at this stage.

States are much more open about the second potential method for states to attempt to shift welfare recipients. Many states have programs for combing through their AFDC rolls and identifying the recipients most likely to be eligible for SSI benefits. After a child is identified as being potentially eligible for benefits, the state assists the family in preparing an application for SSI. Several states have programs structured similar to a program in Texas.[8] The state instructs all their caseworkers to ask AFDC families if any members suffer from disabilities. Also, they match names on the AFDC rolls to special education records to determine which children are most likely to be eligible for benefits. Once these children are identified, Texas caseworkers help the family through the application process, especially encouraging the family to appeal a negative decision. The state estimates that spending $500,000 per year on these efforts will save the state $5,000,000 per year by the year 2000.

Anecdotal evidence also suggests that governments below the state level also try to identify children eligible for SSI to save money on AFDC. In New York City, the city government pays part of the state's share of the AFDC benefit. A representative of the New York City Board of Education described the city's effort to move children onto SSI by saying:

> "It is a way of saving money in these lean times and making the federal government responsible for assuming some of the costs on its own. We've been extremely aggressive. We're doing a *mitzvah*, a good deed. The city saves money on the public assistance grant, and the client benefits from a more generous SSI program...For too long, New York City has played the role of sucker *vis-à-vis* Washington," (*New York Times*, April 6, 1996).

[8] See Comptroller for the State of Texas [1995] for a full description of the Texas program for identifying AFDC clients eligible for SSI.

All of the descriptions of state and local government efforts to identify potential SSI recipients cite not only the fiscal benefit to the government of the program, but they also highlight how they are providing more cash assistance to the recipient. Therefore, states argues that everyone benefits from these programs and no one loses.

2.2.3 Incentives for Families

For AFDC families of all sizes living in all states, the gain of SSI benefits from having a child become eligible for SSI is greater than the loss of AFDC benefits.[9] Although all families gain financially, some families can lose health insurance coverage when a child begins to receive SSI. When a family receives AFDC assistance, the entire family is covered by Medicaid; however, only the child is covered by Medicaid when the child receives SSI.[10] This is not a problem for families with more than one child; they receive both AFDC and SSI, and the disabled child receives Medicaid from SSI while the rest of the family is covered by Medicaid from AFDC. However, for one-child families that no longer receive AFDC, the parent is no longer covered by Medicaid after the child begins to receive SSI. Because of the uncertainty of placing a cash value on the Medicaid benefits the parent receives from AFDC, it is difficult to say whether the gain in cash benefits from SSI receipt for one-child families offsets this loss in health insurance coverage.[11]

Even without knowing how the family values Medicaid coverage for the parent, it is possible to conclude that the AFDC families states most want to encourage to apply for SSI are also the families that

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[9] The federal benefit for SSI in 1995 was $5720 per year. That number does not include the supplements that some states provide. The largest loss of AFDC benefits is $4250 for a one-child family from Alaska in 1995.

[10] States are allowed to set more restrictive criteria for receiving Medicaid coverage than cash benefits from SSI. Twelve states, known as 209(b) states for the section of the law that allows for the tighter eligibility requirements, set more binding standards for receiving Medicaid either by requiring a lower income limit or a higher disability standard than the cash benefit criteria.

[11] One piece of evidence that suggests that the value of Medicaid for the parent might be small comes from Yelowitz [1995]. He finds that families are more likely to leave AFDC when Medicaid is extended to cover children not receiving AFDC even though the parents will lose Medicaid coverage.

have the smallest personal incentive to pursue a claim. One-child families lose the most AFDC benefits, hurting the family but helping the state. Kubik [1997] finds evidence that higher net gains of SSI to the family encourage AFDC recipients to apply for SSI; therefore, one-child families would be least likely to apply for benefits on their own. Families can easily apply and receive SSI without any assistance from the state; however, states cannot assist a child's application without the family's cooperation. Parents must take the child to doctors to build a medical report for the application, and parents of children applying for SSI benefits because of a mental impairment must provide testimony on how the health condition the child's normal activities.[12]

Some states try to make the application process as inexpensive as possible for AFDC families in an attempt to encourage even those with only a small monetary incentive to apply to cooperate in the application process. For example, Texas suggests that caseworkers schedule the meetings and doctors appointments necessary for the application for the family and monitor the application process to make sure there are no problems.[13] If the carrot does not work, the state could rely on the stick. For instance, states could threaten to require the head of an AFDC family to join a job program unless the family applies for SSI, which has less harsh work rules than AFDC. Whatever method, it appears that the state must offer some method of encouragement to make SSI receipt attractive to the families they most want to leave the AFDC rolls.

2.3 Empirical Strategy

My approach for searching for empirical evidence that states play a large role in encouraging children from AFDC families to apply for SSI after the *Zebley* decision involves identifying states suffering fiscal pressures and asking whether those states are more likely to shift children between

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[12] See United States General Accounting Office (GAO) [1994] for a description of the role of parents in the application for SSI because of mental disabilities.

[13] See Comptroller of the State of Texas [1995].

welfare programs. I assume that states in fiscal trouble more highly value the savings of moving a child off the AFDC rolls. There are at least a couple of mechanisms by which a state in fiscal distress could decide to make a concerted effort to identify potential SSI recipients. First, a state that suffers an adverse tax revenue shock or an unexpected expenditure increase could quickly develop or expand a program within the department that administers welfare programs to encourage AFDC recipients with disabilities to apply. However, there need not be a formal effort by the state to spur this encouragement. State social workers fearing for their job during the fiscal crisis might resolve to save the state money, and at the same time helping the family financially, by assisting families in applying for SSI benefits. Therefore, I look for increases in a state's SSI caseload relative to its AFDC caseload in states in fiscal trouble compared to states doing relatively well financially.

Few other papers examine the incentives of states to game the welfare system by assisting public assistance recipients to shift between programs. Most other work are case studies of the efforts of individual states in identifying adults with disabilities and assisting them in applying for the adult part of the SSI program. Bound *et al* [1995] examine the state of Michigan's program for moving recipients of their General Assistance (GA) program to the adult SSI rolls after the state decided to phase out their GA program. Reputed to be one of the most aggressive and efficient states in assisting welfare clients apply for SSI, Michigan had a large out-reach program for helping their former GA recipients, providing them with doctors to consult and other counselors to help with the SSI application process. The state had by far the largest growth rate of adult SSI in the late 1980s and early 1990s, and it also had the largest percentage growth of child SSI cases after the *Zebley* decision, suggesting that it might have used similar resources in identifying potential child SSI applicants. Lewin [1995] surveys the activities of six states, including Michigan, in teaching their residents more about the federal disability programs they are potentially eligible. They conclude that a substantial proportion of the growth of the adult SSI program in the 1980s and 1990s to state efforts to advertise the program and reduce their GA rolls.

This paper is the first to systematically measure the relation between state incentives and the large growth of the child SSI roles after the *Zebley* decision. I create an annual panel of the size of the AFDC and SSI caseloads of all states and the District of Columbia for the years 1987 through 1994 from Social Security publications. Using an approach similar to Poterba [1994], I construct measures of the fiscal health of the states over this time from data produced by the National Association of State Budget Officers (NASBO). These data provide both the expected and actual tax revenues and expenditures of all the states during each fiscal year. From these measures, I form measures of unexpected revenue and expenditure shocks to each state *i*:

(5) $Unexpected\ Revenue\ Shock_i = Actual\ State\ Revenue_i - Expected\ State\ Revenue_i$

(6) $Unexpected\ Expenditure\ Shock_i = Actual\ State\ Expenditures_i - Expected\ State\ Expenditures_i$

all measured per capita. I make my gauge of the fiscal health of a state from these variables:

(7) $Unexpected\ Deficit\ Shock_i = Unexpected\ Expenditure\ Shock_i - Unexpected\ Revenue\ Shock_i$

My model suggests that states that experience a deteriorating fiscal condition are more likely to attempt to shift public assistance recipients to the SSI program. Therefore, I estimate regressions of the form:

(8) $Measure\ of\ Shifting\ to\ SSI_{it} = \alpha + \beta_1 Unexpected\ Deficit\ Shock_{it} + State_i + Year_t + \varepsilon_{it}$

where $Measure\ of\ Shifting\ to\ SSI_{it}$ is a gauge of welfare shifting in state *i* at time *t*, $State_i$ is a full set of state dummies, $Year_t$ is a set of year effects, and ε_{it} is an error term. β_1 is the coefficient of interest and should be an economically significant value if states do look to save money during harsh fiscal times by having the federal government take over some of their public welfare expenditures.

I use a couple of measures of state welfare recipient shifting. The first is the child SSI caseload divided by the child AFDC population of the state; when a child moves between the two programs, this ratio rises. Figure 1 plots this proportion for the entire U.S. over the years 1987 through 1994. It rises significantly after the 1990 *Zebley* decision, almost tripling in a four year period. This increase varies significantly across the states. Figure 2 shows the SSI roll growth over the late 1980s and early 1990s in the states of Michigan and Oregon. While Michigan has a very large growth in caseloads, almost quadrupling in size after the *Zebley* decision, Oregon's caseloads only double over the same period.

Using this ratio as a measure of welfare shifting might be problematic. The fiscal health of a state is obviously affected by the general economic climate of the state; a downturn in the economy lowers state tax revenues and increases expenditures. The proportion of children on SSI relative to AFDC might also be influenced significantly by the state economy; children of families whose head is newly thrown out of work might be more likely to apply for one of the programs rather than the other. That produces a correlation between the unexpected deficit variable I use and the SSI to AFDC proportion that is not related to the actions of the states. Therefore, I also use a second measure of welfare shifting that I argue is not influenced by changes in the state economy: the proportion of children receiving SSI that are only children. As shown in Table 2, states on average save much more money when a child with no siblings leaves the AFDC rolls for SSI. If the proportion of SSI recipients that are only children grows when a state has a fiscal crisis, that suggests that the state is actively searching for methods to save money from AFDC through SSI. It is difficult to argue that this proportion should be related to the economic health of the state.[14]

Table 3 presents the means and ranges of the variables used in the subsequent analysis for the years after the *Zebley* decision, 1991-1994. As discussed before, there are wide ranges across states in

[14] One argument is that the heads of families with only one child tend to be younger than other parents. If economic downturns hurt younger workers relatively more than others, then there could be some correlation with state's economic health. I assume that such effects are small.

the growth of the SSI program after 1990. Also, the recession of this period affected states much differently; some states had large unexpected deficits from the downturn while others weathered the recession relatively unscathed.

2.4 Results

2.4.1 Fiscal Stress Measures

Before I move to the main results of my work, I begin by examining how any unexpected deficit shock affects the measures of welfare shifting. In Table 4, I show the results of OLS regressions similar to the model presented in equation (8); however, I replace the unexpected deficit shock measure with an indicator variable that the state is suffering a deficit shock of any size. Columns (1) and (2) employ the dependent variable of the proportion of child SSI recipients to child AFDC recipients; columns (3) and (4) use the percentage of SSI recipients that have no siblings measure. The sample period is 1991 to 1994 in column (1); the coefficient on the deficit indicator suggests that the proportion of SSI recipients to AFDC recipients rises by about one-half of a percentage point when a state is in fiscal distress. This translates to about a 6% gain on average over this period. In column (2), I add the 1987 through 1990 sample into the regression and split up the effect of fiscal crises before and after the *Zebley* verdict. An unexpected deficit has little effect before 1991 but has a similar, but slightly smaller, measured effect on the proportion of SSI to AFDC caseloads after 1990 as measured in column (1). Column (3) shows that a state deficit increases the percentage of child SSI recipients that have no siblings for the post-1990 period; however, the coefficient is so imprecisely estimated that it cannot be said to be different than zero. Adding the pre-*Zebley* period into the sample in column (4) does not help the precision of the estimates enough to conclude that deficits have an effect.

The table suggests that unexpected deficits have an effect on the proportion of children on SSI relative to AFDC; however, I have not yet found evidence for the cleaner measure of welfare shifting. Now I move to using the value of a state's unexpected fiscal deficit as a measure of the fiscal situation of

the state, as described in equation (8). First, in Table 5, I examine the effects on the proportion of SSI to AFDC recipients variable. Column (1) shows the effect of a deficit on the post-*Zebley* sample; the coefficient is the predicted sign and statistically significant from zero. The estimate suggests that a one standard deviation increase in a state's deficit increases the proportion of SSI to AFDC recipients by about three-tenths of a percentage point, a three percent increase on average. Including the pre-1991 sample and splitting the effect of the deficit shock in column (2) increases the coefficient; a standard deviation increase raises the proportion by three and a half tenths of a percentage point, or a 4% increase.

It can be argued that there is a problem with the unexpected deficit measure in these regressions. Since spending for AFDC benefits and SSI supplements are included in state expenditures, unexpected expenditure shocks and therefore unexpected deficit shocks are related to SSI and AFDC caseloads, the dependent variable. This relationship could bias the estimates of the regressions. One solution to this problem is to only use unexpected revenue shocks to the state instead of deficit shocks. Columns (3) and (4) of Table 5 present the results of regressions removing unexpected expenditures. In column (3), a positive revenue shock has a negative and marginally statistically significant effect on the proportion of child SSI to AFDC recipients, as expected. An increase of one standard deviation of the revenue variable decreases the proportion by almost three-tenths of a percentage point; a 3.5% fall. Adding the pre-1991 sample to the regression in column (4) strengthens the coefficient slightly.

Tables 4 and 5 provide substantial evidence that states in fiscal stress have a higher proportion of SSI recipients to AFDC recipients relative to other states; now I move to more carefully examine how fiscal crises affect the cleaner measure of welfare shifting. Column (1) of Table 6 shows the results of the deficit shock regression for the post-*Zebley* sample with the dependent variable the percentage of SSI children without siblings. The coefficient is positive and marginally statistically significant from zero; the usual one standard deviation increase in the deficit shock increases the proportion of children on SSI who are only-children by about three percentage points, or about an 8% increase. Column (2) reveals that adding the pre-1991 sample slightly increases the measured effect of unexpected deficits on the post-

Zebley period. As in Table 5, columns (3) and (4) exclude unexpected expenditures and only include unexpected revenue shocks. The coefficients on revenues demonstrate a similar, but opposite sign pattern, as the unexpected deficit results. Unexpected gains in revenues significantly lower the proportion of children on SSI that have no siblings.

The results presented above demonstrate that movements of children onto SSI and off AFDC are sensitive to the fiscal health of the state after the 1990 changes to the SSI program created a second significant welfare program for children. States doing poorly are more likely to have higher SSI caseload growth relative to their AFDC caseload size relative to other states. More convincing, a higher number of children with no siblings, which are big catches from the perspective of the state, move onto SSI when a state is doing poorly fiscally. Now that I have established a link between a state's fiscal health and their efforts to move children off AFDC and onto SSI, I move toward examining how the proportion of AFDC benefits a state has to pay affects its decisions.

2.4.2 State Matching Rates

A fundamental problem with attempting to analyze the effect of matching rates on state decision-making is the mechanical relationship between a state's match rate and its economic health (see equation (2)). It is very difficult to separate the effect of changing match rates from changing economic indicators. I rely on the nonlinear relationship between state per capita income and the match rate to isolate the influence of changes in the match rate on the propensity of states to shift children between AFDC and SSI.[15]

Table 7 presents results of regressions similar to the model presented in equation (8); however, the fiscal variables are replaced with the state match rate. The dependent variable is the measure of the state's child SSI caseload relative to the size of its AFDC rolls. Column (1) contains the standard

[15] Ideally, I would be able to use changes in the formula to calculate the match rate that affected some states more than others. Unfortunately, there are no such changes over the time in question.

regression for the sample of states after the *Zebley* decision. An increase in a state's match rate is estimated to have a positive and statistically significant effect on the SSI to AFDC proportion. The coefficient is very large; a one standard deviation increase in the match rate implies a 2.8 percentage point increase in the SSI to AFDC ratio, a 32% increase. However, a one standard deviation movement of the match rate is well out of sample for an individual state. No state match moves more than a couple of percentage points over the 1990s. Therefore, predicting the effect of such large movements might be beyond the power of these regressions. Appending the pre-*Zebley* sample to the regression in column (2) produces a similar large estimate.

To attempt to disentangle the effect of match rates from changes in a state's economic condition, I exploit the fact that ten of the states are so well off that they always have the highest possible match rate of 50%.[16] Barring economic disaster, they will always have the same match rate; therefore, for those states, the measured effects of economic variables on the measures of welfare shifting are due to the economic conditions themselves and not a consequence of changing match rates. My plan for isolating the effect of match rates is to measure the direct effect of economic variables on welfare shifting by first looking at the ten full match states. Then, under the assumption that the direct effect of the state's economy on welfare shifting is the same for those ten full match states as the rest of the country, I can cleanse the welfare shifting variables of the direct economic state effect using the estimates from the full match states and then measure the direct match rate effect on the rest of the states.

I run the same OLS estimates for the sample of states that do not always have to full match in columns (3) and (4) as I ran for the full sample in columns (1) and (2). The coefficients on the match rate are slightly smaller than the estimates for the full sample and, not surprisingly, less precise. In columns (5) and (6), I attempt to filter out the direct effect of the state economy using the full match states. First I run a regression of the state proportion of SSI to AFDC recipients on a large number of powers of state

[16] The ten states are Alaska, California, Connecticut, Washington DC, Illinois, Maryland, Massachusetts, New Hampshire, New Jersey, and New York.

per capita and unemployment measures, both current and lagged, on the sample of full match states. Using those coefficients on the income and unemployment variables, I construct the residuals of the ratio of SSI to AFDC recipients for the sample of states that are not full matching. Finally, I regress those residuals on the match rate in a model like equation (8) for the non-full match sample. Column (5) presents the results for the post-1990 sample; the coefficient on the match rate is almost two times larger than the OLS estimate in column (3). However, the pre- and post-*Zebley* regressions produce a smaller estimate for the post-*Zebley* period.

Table 8 duplicates the exercise above but with the dependent variable of the percentage of children receiving SSI from one-child families. The match rate has a positive but imprecisely estimated effect on the percentage of SSI recipients with no siblings in column (1); however, when the pre-*Zebley* sample is added, the estimated effect of post-*Zebley* changes in the match rate is positive and statistically significant from zero. A one standard deviation increase in the match rate increases the percentage of children receiving SSI that are from one-child families by almost 5 percentage points, a 11% increase. The attempt to use full match states to isolate the direct economic effects does not appear to be as successful for this dependent variable. Column (3) and (4) reproduce the OLS estimates for the sample of non-full match states; the results are very similar to columns (1) and (2). The two-step procedure reported in columns (5) and (6) produce estimates that are wrong-signed and imprecisely estimated.

It is difficult to interpret the results from these regressions of welfare shifting measures on match rates. On one hand, increases in share of AFDC benefits the state must pay does appear to be associated with a higher degree of welfare shifting as predicted; however, interpreting the extremely large coefficients is problematic. Using these results as a predictor, the increase in the SSI caseload relative to the AFDC caseload will be enormous as states face a 100% marginal match rate under the new block grant scheme implemented under the welfare reform legislation. The dilemmas of not having much within-state variation in the match rate over time and all of the within-state variation that does exist being

related to changing state economic conditions suggest that this method of identifying the effect of match rates on welfare shifting might never be satisfying.

2.5 Discussion

The growth in child SSI caseloads after the 1990 *Zebley* decision was many times greater than anyone predicted at the time. People have attributed the surge in SSI applications to the diligent work of child disability advocacy groups in advertising the liberalization of the program and to the significant financial gain that low-income families can enjoy if their child's application for benefits is approved. This paper demonstrates that state governments are also an important participant in encouraging low-income families to apply for SSI benefits.

The funding rules of AFDC and SSI reward states for successfully transferring a child receiving AFDC benefits to the SSI program. I show that states suffering from fiscal problems take advantage of these gains by assisting families on the public assistance rolls apply for SSI after the *Zebley* verdict. States in fiscal distress have a higher SSI caseload relative to AFDC recipients and have a higher ratio of child SSI recipients that have no siblings, a group for which states receive a relatively high financial reward. Also, I provide some evidence that states that must pay a higher percentage of AFDC benefits are more likely to attempt to shift welfare recipients to SSI.

The recent welfare reform legislation extensively modifies the financial arrangements for welfare programs between the federal government and the states. AFDC has been eliminated and replaced by a series of block grants, known as TANF; the marginal cost of a new welfare recipient placed on the rolls is now borne completely by the state. The evidence in this paper suggests that states will increase their efforts to identify children eligible to be removed from the able-bodied welfare roles and placed on SSI. However, Congress also mandated a tightening of the child disability standard in the same legislation. The Clinton Administration recently released a new set of rules governing the adjudication of child

disabilities, but it is unclear at this time how far they cut back the program.[17] If the federal government has not completely cut back the SSI program, then states will continue to look for ways to save money and move people onto the program.

[17] See *New York Times* (February 6, 1997) for a description of the new SSI child guidelines. The best guess at this time is that the program will still be more generous than the pre-*Zebley* days.

2.6 References

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Table 2.1: State Matching Rates for AFDC Benefits, 1994

State	Match Rate	State	Match Rate
Alabama	28.78	Montana	28.95
Alaska	50.00	Nebraska	38.02
Arkansas	25.54	Nevada	49.69
Arizona	34.10	New Hampshire	50.00
California	50.00	New Jersey	50.00
Colorado	45.70	New Mexico	25.83
Connecticut	50.00	New York	50.00
Delaware	50.00	North Carolina	34.86
Florida	45.22	North Dakota	28.87
Georgia	37.53	Ohio	39.17
Hawaii	50.00	Oklahoma	29.61
Idaho	29.08	Oregon	37.88
Illinois	50.00	Pennsylvania	45.39
Indiana	36.51	Rhode Island	46.13
Iowa	36.67	South Carolina	28.92
Kansas	40.48	South Dakota	30.50
Kentucky	29.09	Tennessee	32.85
Louisiana	26.51	Texas	35.82
Maine	38.04	Utah	25.65
Maryland	50.00	Vermont	40.45
Massachusetts	50.00	Virginia	50.00
Michigan	43.63	Washington	45.76
Minnesota	45.35	West Virginia	24.28
Mississippi	21.15	Wisconsin	39.53
Missouri	39.36	Wyoming	34.37

Table 2.2: Monetary Gain to States of Moving a Child from AFDC to SSI, 1994

	Average State Savings	Maximum	Minimum
Family of One Parent and One Child	$1139	$3084	$152
Family of One Parent and Two Children	$465	$1842	$90
Family of One Parent and Three Children	$361	$882	$61

Table 2.3: Means of Data, 1991-1994

	Mean	Minimum	Maximum
Child SSI Recipients / Child AFDC Recipients	.0865 [.0513]	.01784	.3614
% of SSI Recipients from One-Child Families	43.08 [3.61]	39.21	48.09
State Match Rate (%)	38.85 [9.23]	20.00	50.00
Per Capita Unexpected Revenue Shock	24.57 [124.3]	-460.95	1255.78
Per Capita Unexpected Deficit Shock	-15.18 [59.54]	-437.29	125.43
State Unemployment Rate (%)	6.29 [1.53]	2.6	11.3
State Per Capita Income	19781 [3197]	13214	30555
Maximum AFDC Benefit for Family of One Parent and Two Children	4532 [1855]	1152	11076

Notes: 204 State-level Observations. Standard deviations in brackets.

Table 2.4: The Effect of an Unexpected State Fiscal Deficit on the Composition of the Welfare Population

Dependent Variable:	SSI Recipients / AFDC Recipients		% of SSI Recipients from One-Child Families	
	(1)	(2)	(3)	(4)
Indicator that State has an Unexpected Deficit	.0056 (.0031)	.0013 (.0021)	.0153 (.0283)	-.0033 (.0171)
Indicator for After *Zebley* Decision		.0810 (.0110)		.0374 (.0198)
Deficit Dummy * *Zebley* Dummy		.0039 (.0020)		.0134 (.0186)
Year Effects?	Yes	Yes	Yes	Yes
State Effects?	Yes	Yes	Yes	Yes
Observations	204	408	204	408

Notes: Standard errors in parentheses.

Table 2.5: The Effect of Unexpected Deficit and Revenue Shocks on the Proportion of Children on SSI Relative to AFDC

	(1)	(2)	(3)	(4)
Per Capita Unexpected Deficit Shock / 1000	.0451 (.0210)	.0002 (.0182)		
Per Capita Unexpected Revenue Shock / 1000			-0239 (.0127)	.0008 (.0167)
Indicator for After *Zebley* Decision		.0112 (.0041)		.0113 (.0042)
Deficit Shock * *Zebley* Dummy		.0573 (.0186)		
Revenue Shock * *Zebley* Dummy				-.0323 (.0148)
Year Effects?	Yes	Yes	Yes	Yes
State Effects?	Yes	Yes	Yes	Yes
Observations	204	408	204	408

Notes: Standard errors in parentheses.

Table 2.6: The Effect of Unexpected Deficit and Revenue Shocks on the Proportion of SSI Recipients from One-Child Families

	(1)	(2)	(3)	(4)
Per Capita Unexpected Deficit Shock / 1000	.5871 (.3369)	.0996 (.1449)		
Per Capita Unexpected Revenue Shock / 1000			-.3318 (.1606)	.0501 (.1286)
Indicator for After *Zebley* Decision		.0095 (.0211)		.0082 (.0210)
Deficit Shock * *Zebley* Dummy		.7897 (.3602)		
Revenue Shock * *Zebley* Dummy				-.4240 (.2044)
Year Effects?	Yes	Yes	Yes	Yes
State Effects?	Yes	Yes	Yes	Yes
Observations	204	408	204	408

Notes: Standard errors in parentheses.

Table 2.7: The Effect of the Match Rate on the Proportion of Children Receiving SSI Relative to AFDC

	Full Sample		Sample of States less than Maximum Match			
			OLS		Residuals	
	(1)	(2)	(3)	(4)	(5)	(6)
State Match Rate	.3003	-.0652	.2513	-.0522	.4417	.1995
	(.1661)	(.1257)	(.1788)	(.1314)	(.2368)	(.1222)
Indicator for After *Zebley* Decision		-.1535		-.1240		.0504
		(.0857)		(.0916)		(.0160)
Match Rate * *Zebley* Dummy		.3671		.3092		.1766
		(.1801)		(.1925)		(.0406)
Year Effects?	Yes	Yes	Yes	Yes	Yes	Yes
State Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	204	408	164	328	164	328

Notes: Standard errors in parentheses.

Table 2.8: The Effect of the Match Rate on the Proportion of Children Receiving SSI Relative to AFDC

	Full Sample		Sample of States less than Maximum Match			
			OLS		Residuals	
	(1)	(2)	(3)	(4)	(5)	(6)
State Match Rate	1.827	2.170	2.285	2.350	-.9118	1.253
	(2.026)	(1.073)	(2.135)	(1.063)	(3.402)	(2.574)
Indicator for After *Zebley* Decision		-.2000		-.2049		-.1574
		(.1218)		(.1374)		(.1758)
Match Rate * *Zebley* Dummy		.5309		.4839		-1.554
		(.2875)		(.3489)		(2.843)
Year Effects?	Yes	Yes	Yes	Yes	Yes	Yes
State Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	204	408	164	328	164	328

Notes: Standard errors in parentheses.

Figure 2.1: The Number of Children Receiving SSI Relative to the Number Receiving AFDC, 1987-1994

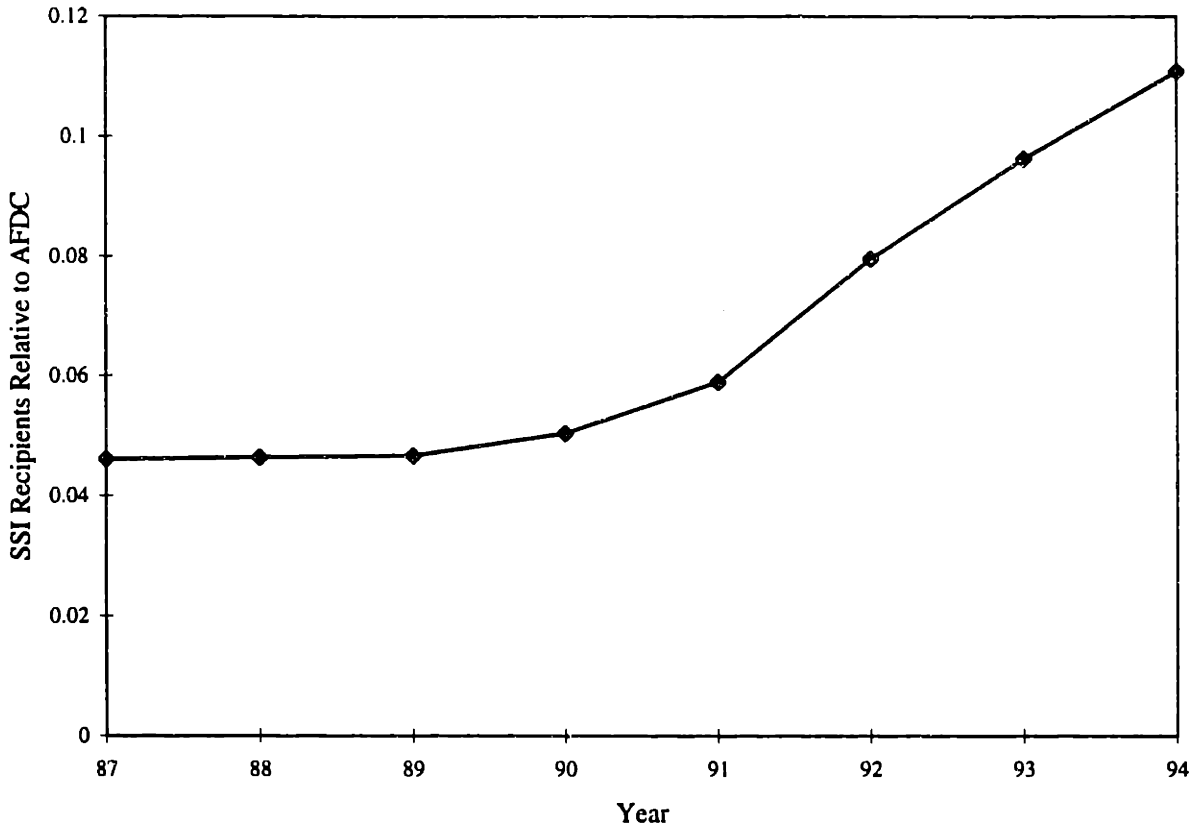


Figure 2.2: % of Children on SSI, Oregon and Michigan 1987-1994

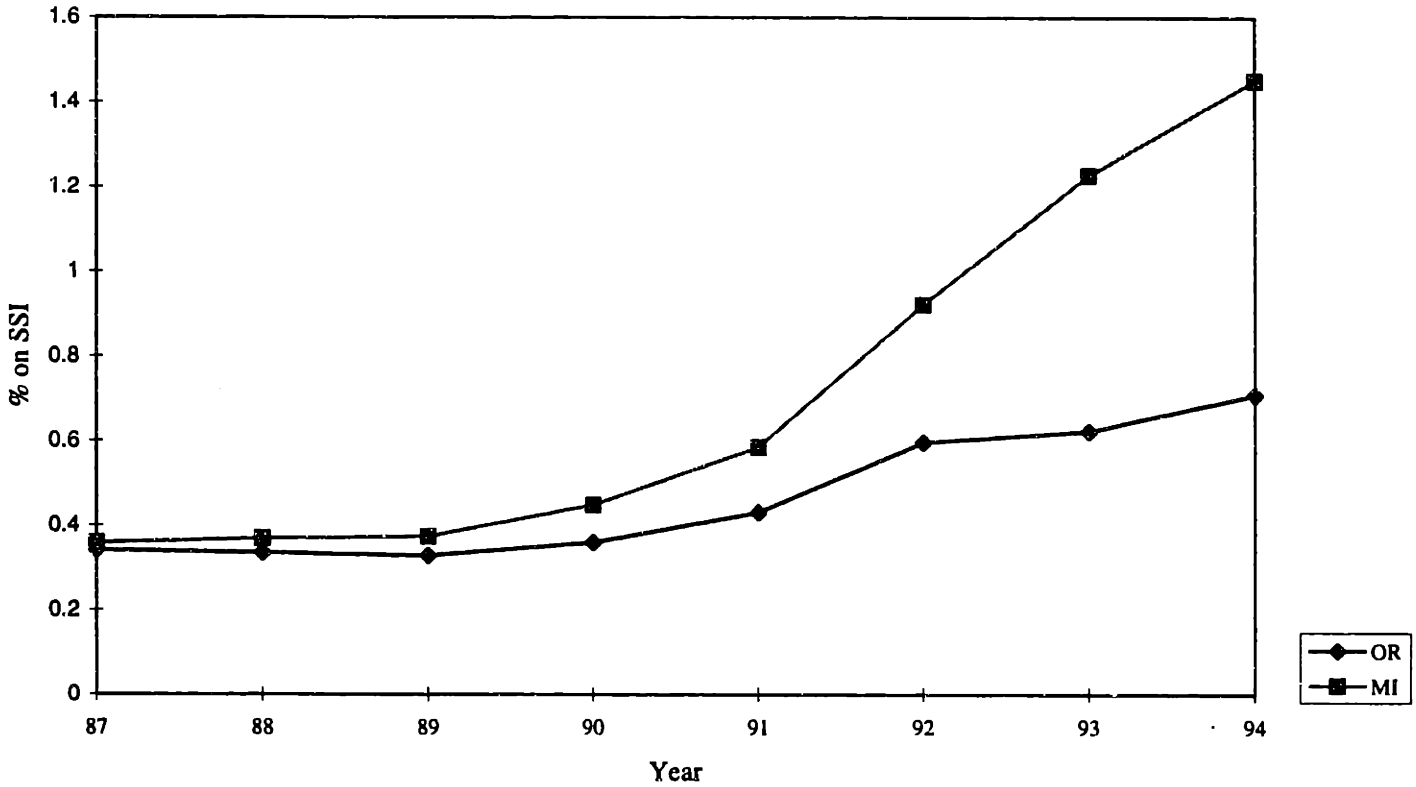
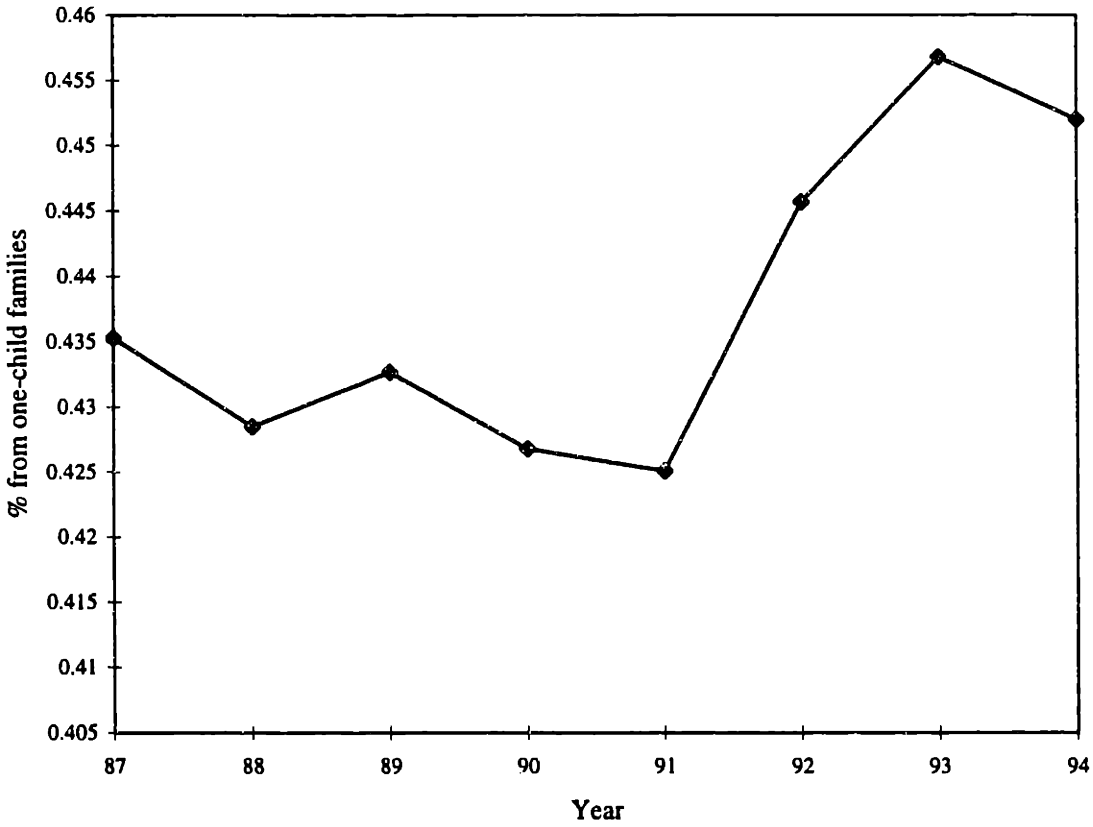


Figure 2.3: Percentage of SSI Recipients from One-Child Familes



Chapter 3

Disability Insurance Rejection Rates and the Labor Supply of Older Workers

3.1 Introduction

Disability Insurance (DI) is one of the largest social insurance programs in the U.S., with over 5 million beneficiaries and benefit payments of over $36 billion (*New York Times*, April 8, 1994). In theory, this program provides income support to workers who are unable to continue employment due to physical disability. The difficulty of defining disability, however, as well as the generosity of potential benefits under the DI program has led many analysts to question whether DI is distorting labor supply decisions among those able to work. As Parsons (1980) and others have noted, the rapid growth in the DI program during the 1960s and 1970s closely parallels the rapid rise in labor force non-participation among older male workers.

This potential moral hazard problem would ideally be resolved by improving the precision of the screening process used by the DI program. Barring this, there are a number of second-best approaches to addressing the problem and slowing program growth. The approach which has received the most attention in the empirical economics literature is a reduction in the benefits available to the disabled. A number of articles have found that cutting benefits will slow the rate at which older workers leave the labor force and enter the DI program, but the estimated effect on these flows varies widely. There are, however, a number of other policy tools available for addressing the moral hazard problem induced by imperfect screening which have received much less attention.

The policy alternative that we consider in this paper is an increase in the stringency of the screening process for DI applicants. As noted by Marvel (1982) and Parsons (1991a), initial DI decisions are made by state agencies, leading to substantial variation across the states in the fraction of DI applications which are granted. In a single cross-section, variation in state denial rates reflects a number of factors, including the

granted. In a single cross-section, variation in state denial rates reflects a number of factors, including the average health mix of the underlying applicant pool, so that this variation is not useful for identifying the effect of DI policy on labor force behavior. But there was a dramatic rise in state denial rates in the late 1970s, due to a funding crisis for DI at the federal level, that potentially provides a more fruitful source of identification. Average denial rates rose by 15.5 percentage points (30%) over a 3 year period, and this increase occurred at a differential rate across the states. We match information on these differential denial rate increases to the National Health Interview Survey (NHIS), a large nationally representative micro data survey, to address two questions about the effects of DI screening on the labor force non-participation of 45-64 year old males.

First, do increases in state denial rates increase the labor supply of older workers? We find that there is a significant negative relationship between denial rates and labor force non-participation among 45-64 year olds. Our estimates imply that the 30% increase in denial rates from 1977 to 1980 led to a fall in labor force non-participation among this group of 1.4 percentage points (8.3%). Some part of this effect is due to the return to work of denied applicants; but correcting for this reemployment only explains (at most) 1/2 of our estimate.

Second, how well targeted were the incentive effects of increased denial rates? We use an anthropometric measure of underlying health, the Body Mass Index, to examine the differential effect of changes in denial rates on those in better vs. worse health. We find that, according to this measure, increases in denial rates effectively targeted the labor force incentives to individuals in better health. Under our strictest measure of disability, we find that there was no effect of this policy change on the disabled, but that the labor force non-participation of the able fell by 11.1%.

The paper proceeds as follows. In Part I, we provide some background on the DI program, and review previous evidence on the effect of DI on labor force participation. In Part II, we discuss the policy

change under consideration. In Part III, we describe the data and modelling strategy. Part IV presents the results on the overall effects on labor force participation, and Part V presents the results on targeting efficiency. Part VI concludes.

3.2 Background on DI[1]

3.2.1 Institutional Details

DI provides insurance for those persons who have an "inability to engage in substantial gainful activity by reason of physical or mental impairment;" this impairment must be medically determinable and last at least 12 months. Eligibility is contingent on previous labor force attachment; most applicants must have worked 20 of the 40 quarters prior to application, although the rules differ somewhat for younger applicants. However, application for DI cannot be made unless the individual has been out of work for at least five months, providing the first of several serious application barriers.

While DI is a federal program with nationally uniform standards, initial awards decisions are made by state DI boards, based on a medical examination. Denied applicants are then entitled to a series of appeals, first to the state agency, then to an Administrative Law Judge who is independent of the state boards, then to an Appeals Council, and finally to the Federal court system. The award decision is well known to be a highly imperfect targeting device. In two studies reviewed by Parsons (1991b), reconsideration of initial determinations by either the same review panel one year later or a separate team of medical experts revealed substantial Type I and Type II errors.

The DI benefits for which individuals are potentially eligible are fairly generous. Benefits determination follows the same methodology as Social Security benefit calculations, with benefits computed

[1]Except where otherwise noted, details on the DI program are for 1992 and are from U.S. Congress, House Committee on Ways and Means (1993).

as a (redistributive) function of past earnings history. After-tax replacement rates averaged approximately 60% by the mid-1970s (Bound, 1989).

Perhaps as a result of these generous benefits and imperfect targeting, there was a tremendous growth in the DI rolls during the 1960s and 1970s. The number of DI recipients grew from 455,000 in 1960 to 2.8 million by 1977, and total payments grew twentyfold (U.S. Department of Health and Human Services, 1990). At the same time, there was a dramatic decline in the labor force participation rate of older males, and the two time series moved in almost exact parallel, as documented in Parsons (1980). But drawing causal inferences from this time series data is problematic, as there were a number of other changes in the labor market and non-labor market opportunities of older males during this era.[2]

3.2.2 DI Program Parameters and the Behavior of Older Workers

A sizeable literature has attempted to use cross-sectional variation to identify the role that DI plays in the labor force participation decisions of older men. These studies generally proceed by modelling labor force participation or DI recipiency as a function of potential DI benefit levels. Parsons (1980) estimates very large disincentive effects of DI generosity, which suggest that DI program growth can explain almost all of the change in the labor force participation of older males during the era of DI expansion. Other estimates have supported the contention that DI has a significant disincentive effect, although the estimated magnitudes have generally been smaller than that of Parsons; see Leonard (1986) and Bound (1989) for reviews of this evidence.

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[2]For example, there was rapid growth in retirement incomes in this era, both due to increased Social Security benefit levels, and increased coverage of the labor force by pensions (Lumsdaine and Wise, 1990). Bound and Waidmann (1992) attempt to surmount this problem by looking at changes in self-reported disability among job leavers before and after the broad expansion of DI in the 1960s. They estimate that DI growth can explain as much as 80% of the rise in non-participation among 45-54 year olds in the 1970s, but that for 55-64 year olds the effects are only one-half as large.

Bound (1989) argues, however, that this type of strategy is likely to yield misleading inferences for the effect of DI generosity on labor force participation. Since DI benefits are a redistributive function of past earnings, there is a fundamental identification problem in modelling the effect of potential DI benefits on work decisions: a finding that workers with higher potential DI replacement rates are more likely to leave their jobs may simply reflect differential tastes for work among high and low wage workers.[3] Bound suggests an alternative empirical strategy: examining the behavior of workers who apply for DI benefits, but are rejected. In theory, these workers should be at least as healthy as those who are on the program, so that their labor force participation rates provide an upper bound on the potential labor force participation of accepted workers. Bound finds, however, that less than 50% of rejected workers had returned to work by 18 months (or more) after their rejection, which suggests that DI program growth can explain no more than 40% of the rise in non-participation among older males.[4]

As Parsons (1991a) notes, however, there are a number of tools available to the DI policy-maker who is trying to mitigate moral hazard. His theoretical model predicts that DI applications rise as benefits rise, and that they fall with increases in either the denial rate or the delay in processing applications.[5] But we are aware of only three other studies which have estimated the impact of alternative program parameters on the behavior of older workers. Halpern and Hausman (1986) build a detailed structural model of the decision to apply for DI benefits as a function of DI acceptance probabilities and potential benefits, both of

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[3]Studies such as Haveman and Wolfe (1984) attempt to correct for this omitted variables bias in a number of ways, but Bound (1989) argues that the problem has not been convincingly resolved.

[4]The validity of denied applicants as a control group, however, rests on two key assumptions. First, these applicants must be unobservably no less likely to work than accepted applicants; Bound uses pre-application differences in characteristics to demonstrate that this is true. Second, the process of applying must have no lasting effects on labor market performance. This assumption is more difficult to evaluate; see Bound (1991a) and Parsons (1991b) for differing opinions on its validity.

[5]In the context of Parsons' (1991a) model, changes in applications behavior are automatically linked to changes in labor supply, so that these policy tools affect labor force participation equally. But, as we note below, this implication is not necessarily borne out in reality, since takeup of DI among those out of the labor force is less than full.

which are in turn modelled as a function of individual characteristics. Marvel (1982) and Parsons (1991a) follow a reduced form approach, modelling the number of DI applications in a state/year as a function of state-level denial rates. While Marvel's results are mixed, both Halpern and Hausman and Parsons find strong effects of denial rates on applications probabilities; both estimate that the elasticity of applications with respect to changes in the denial rate is approximately 0.45.[6]

3.2.3 Targeting Efficiency

One important issue which has been unexamined in previous empirical work is the extent to which increased denial rates efficiently *target* their incentives: if labor supply increases and applications fall, will the response come mostly from the truly able or the truly disabled? This question goes to the feasibility of denial rate increases as a self-screening device, to use Parsons' (1991a) terminology. He finds that, for plausible parameter values, increases in denial rates should disproportionately reduce applications among the able. It is possible, however, that denial rate increases could have a perverse effect, reducing applications more among the disabled than among the able. Such a perverse effect could arise due to higher discount rates or capital market constraints for that group, so that they are less able to bear the increased uncertainty that arises from uniform increases in screening stringency. Parsons does provide some evidence that the quality of screening under DI, as measured by the ultimate mortality of accepted vs. rejected applications, is highest at the initial determination stage.[7] But this evidence cannot resolve the important question of the differential effect of denial rate increases on more and less able applicants.

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[6]Parsons (1991a) notes that Marvel's (1982) results were rendered inconclusive because of a data error.

[7]This evidence is difficult to interpret, as Parsons notes, due to sample selection at the different stages of the application process. If the "easy cases" are dealt with at lower stages in the applications process, then it is natural for the targeting efficiency to appear to be worse at higher levels.

Lacking state-by-state information on the health of DI applicants in this era, we are unable to directly investigate targeting efficiency in the application decision. But it is possible to use information on the health of workers and non-workers to assess the targeting efficiency of the labor supply incentives of denial rate changes. That is, to the extent that denial rate rises cause less labor force non-participation, we can assess whether this response is occurring mostly within the able population (which would indicate efficient targeting) or the disabled population (which would indicate perverse self-screening).

3.3 Changes in State Denial Rates

The cross-sectional variation necessary to evaluate the effect of changes in DI allowance policy arises from increased denial rates across the U.S. states in the late 1970s, as employed by Marvel (1982) and Parsons (1991a). As noted in Part I, the initial determination for DI is through medical examinations by state disability boards. In the mid-1970s, DI initial denial rates averaged approximately 54%. By 1977, however, DI tax collections had not kept pace with the rapidly expanding recipient base, and the DI trust fund faced a deficit of 20% of expenditures. The response to this funding crisis was twofold: the payroll tax rate was increased, and the stringency of initial screening for DI was tightened. As a result, denial rates rose from 53.8 percent in 1977 to 69.3 percent in 1980.[8]

This initial stage of applications, however, is not fully under the control of the federal government; while it sets national standards for defining disability, it is up to the state boards to interpret these standards. The increased stringency worked on average, as witnessed by the rise in initial denial rates nationwide; furthermore, denial rates became more uniform across the states, as the coefficient of variation fell from 0.101 to 0.090. But the success of the federal standards change was differential across the states, as is

[8]This is a weighted average, where the weights are the population of 45-64 year olds in the state, as measured in the March 1978 Current Population Survey. This weighted average is taken over the 47 states (and the District of Columbia) for which we have denial rate data; we do not have data on denial rates for Arkansas, Alaska, and Hawaii. We are grateful to Donald Parsons for providing the denial rate data.

illustrated in Table 1, which reports denial rates by state in 1977 and 1980, and the change from 1977 to 1980. Some states, such as Connecticut and New York, saw very large increases in their denial rates, while in others the denial rates rose only slightly. It is this differential response which identifies the effect of DI in the empirical work below. That is, we will assess whether states which increased their denial rate the most in this era saw the largest rise in the labor force participation of older workers. The plausible exogeneity of these state level policy changes raises another advantage of studying this alternative DI policy tool, relative to focusing on potential DI benefit differences (given the inherent identification problems arising from benefits which are a function of lifetime earnings).

There are three important questions to be asked about these denial rate changes. First, why is the variation from this policy change more useful than the point in time variation in denial rates across the states? The key difference is that the point in time variation represents both administrative parameters and the nature of the application pool. In states with a higher propensity to apply for DI at a given health level (perhaps due to poor alternative labor market opportunities), the average applicant will be of better health. As a result, there will be higher denial rates for a given level of screening difficulty. Since individuals have to drop out of the labor force to apply, this will lead to a natural positive correlation between labor force non-participation and denial rates; denial rates will be highest where the most older persons are out of work. Using the change over this era provides a sharp source of variation which can separate administrative influences on the denial rate from the effect of the composition of the pool.

Second, given the availability of an appeals option, how influential are initial denial rates for labor force participation decisions? There are two reasons why initial denials are key. First, unless the appeals process becomes more lenient at the same time that the initial screening is becoming more stringent, then increases in initial denials will lead to increases in ultimate denials. In fact, total awards fell lockstep with initial denials in the period from 1977 onwards (Lando et al., 1982). Second, even if the appeal is ultimately

successful, the appeals process is time consuming and potentially costly in terms of foregone earnings. As Parsons (1991a) notes, an application which was successful at the final level would take at least 14 months to get resolved, a figure which does not include the applicant's delay in processing each portion of the application.

Finally, can state denial rate changes be taken as independent of other state correlates of labor force behavior? For example, it may be that even in changes, the denial rate is a function of the structure of the applicant pool, so that the bias noted above remains. However, it seems unlikely that such a dramatic change in denial rates over a relatively short period could be a function of the composition of the applicant pool. Furthermore, the scenario described above would tend to bias against a finding of a negative correlation between the denial rate and labor force non-participation.

Alternatively, it could be that state decisions on how to respond to federal pressure were a function of the state of the local labor market. This type of endogenous administrative response could bias the estimates towards finding a negative correlation between denial rates and non-participation, if states in which there were good labor market opportunities were the ones that increased denial rates the most (because there was less cause for generous DI as a social safety net). While we are unable to rule out this possibility, we attempt to control for it by including indicators for the state of the local labor market, and by assessing the robustness of our results to the experience of outlying regions of the country.

3.4 Empirical Strategy

The data source for the analysis is the National Health Interview Survey. This is a nationally representative survey of households which collects information on the labor force participation of each household member, a number of subjective health measures, and two objective anthropometric measures:

height and weight. We use data from the NHIS for 1976 through 1978 (before the policy change) and 1980 through 1982 (afterwards).[9]

We begin by examining the effect of changes in denial rates from 1977 to 1980 on the labor force non-participation decision of 45-64 year old male workers.[10] We use two NHIS questions about labor force participation: participation in the week of the interview, and participation in the previous year. We will primarily rely on the question about activity at the time of the survey in the empirical work for two reasons. First, the question about labor force participation last year changed substantially after 1981, so this limits our sample size. Second, since the waiting period for DI application is less than one year, lower non-participation due to higher denial rates may be overlooked by a measure with a one year window.[11]

We use these data to run regressions of the form:

$$(1) \qquad NP_{ijt} = f(\alpha + \beta_1 X_{ijt} + \beta_2 DEN_{jt} + \beta_3 LM_{jt} + \beta_4 \delta_j + \beta_5 \tau_t + \epsilon_{ijt})$$

NP is our measure of non-participation for individual i in state j in year t. X is a set of controls for other individual attributes that may affect the participation decision: age, marital status, race, and education. DEN is the log of the denial rate in 1977 or 1980; that is, we don't use the actual denial rate in each year, but rather use a "before" and "after" denial rate. In this way, we avoid short run changes in the denial rate

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[9]We include 1978 in our before period in order to increase the sample size and to follow Parsons (1991a); the results are similar, although somewhat less precisely estimated, if 1978 is excluded.

[10]We focus on males and this age group for comparability to the previous literature, and because the average male in our sample is much more likely to be eligible for (and apply for) the DI program than are females. We ignore the availability of reduced Social Security (SS) benefits for 62-64 year old early retirees. This availability reduces both the benefits of applying for DI (since the net increment to income is only the difference between full and reduced SS benefits) and the costs of applying (since if the worker cannot return to their job they can rely on SS benefits), so that the net increment to the incentive effects of DI are unclear. It is also difficult to infer the effects of SS early retirement because we are using a stock of workers for several years after the policy change, rather than a flow of workers at specific ages at the point of the change, so that the effects on workers of a given age amalgamate effects over the past few years.

[11]That is, when denial rates were low, an able individual may have taken 5 months off to apply, gotten rejected, and returned to work within the year. In that case, a rise in denial rates which caused that individual to not leave their job in the first place will not have any effect on whether there was measured labor force non-participation during the year.

which may reflect changes in the composition of the applicant pool, and focus on the discrete administrative change between 1977 and 1980. In that same vein, we include state dummies (δ_j) to capture systematic fixed differences across states in the nature of the applicant pool and year dummies (τ_t) to measure other time series trends in labor force participation decisions. Finally, LM_{jt} represents two indicators for the condition of state j's labor market in year t: the unemployment rate and the log of state per capital personal income (both from Blanchard and Katz, 1992). These will capture state-specific shocks which may be correlated with both administrative denial rate changes and labor force decisions of older workers in the state. All regressions are run as probits.[12]

There are several difficulties in precisely estimating the effects of denial rates in this framework. First, some of the men in our sample may not be eligible for the DI program.[13] Second, we are examining stocks of working/non-working older persons, rather than the flows in and out of work, so that it will be more difficult to find effects on average. Finally, we are interpreting β_2 as the long run response of non-participation levels to changes in the denial rate. This may not be true if individuals have not adjusted their behavior to reflect the policy change by our "after" period (1979-82). In this case, we may understate the long run change in the stock of workers from denial rate increases. Our hypothesis, which is borne out below, is that the dramatic change in denial rates over this period will be sufficient to overcome these difficulties facing the estimation of incentive effects.

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[12]The results for both the basic labor force non-participation model (1) and the targeting efficiency model (2) are quite similar if linear probability models or logits are estimated instead. Note that the fact that the variable of interest only varies at the state/year level might lead us to understate our standard errors, if there is significant within state/year correlation in labor force non-participation. In the linear probability model context, it is straightforward to correct the standard errors to allow for general intra-state/year correlation (Huber, 1967). Alternatively, one can do two-stage Generalized Least Squares estimation whereby labor force participation is regressed on the control variables and a full set of state/year dummies in the first stage, and the coefficients on those dummies are then regressed on the denial rate in the second stage (Card and Krueger, 1992). Both of these approaches yield inferences that are similar to what we report below; in the former case, the estimates are somewhat larger.

[13]In practice, this is not a very important consideration, however; using data from the Panel Study of Income Dynamics (PSID), we tabulate that 93% of 45-64 year old men in the 1978-1980 period had worked 20 out of the previous 40 quarters.

Second, we use these data to examine the targeting efficiency of changes in state denial rates. As noted above, we focus on one aspect of targeting efficiency: the differential effects of denial rate changes on the labor supply of more and less able workers. We do so using an augmented version of equation (1):

$$(2) \qquad NP_{ijt} = f(\alpha + \beta_1 X_{ijt} + \beta_2 DEN_{jt} + \beta_3 DIS_{ijt} + \beta_4 DIS_{ijt}*DEN_{jt} + \beta_3 \delta_j + \beta_4 \tau_t + \epsilon_{ijt})$$

where DIS is an indicator variable for being disabled (as defined below). In this specification, we can separately identify the effect of changes in denial rates on the able and the disabled. If changes in denial rates perfectly target their labor force incentives to those who are not truly disabled, then the coefficient β_2 will be negative, and the coefficient β_4 will be of the opposite sign and same magnitude (so that there is no net effect on the disabled). On the other hand, if the incentive effect is equal across all categories of health, then the interaction term will be zero.

There is no precise objective measure of disability in the NHIS data. Previous research on health and labor supply has measured health status using self-assessed health, but a number of articles have noted that such self-assessed indicators are problematic in the context of labor supply equations.[14] Thus, we use instead an objective anthropometric measure of disability: the Body Mass Index (BMI), which is (body mass in kilograms)/(height in meters)^2. A number of studies have documented a strong U-shaped correlation between the BMI and both health status and mortality, controlling for other demographic factors (such as age and sex) (Waaler, 1984; Kushner, 1984). Being underweight (for one's height) is associated with increased risk of lung cancer and other respiratory diseases; much of this effect may be due to the correlation between smoking and other adverse health behavior (such as drinking) and being underweight. Being overweight for one's height is associated with increased risk of cardiovascular disease, diabetes, and colon cancer. These types of diseases are among the most common conditions which qualify individuals for disability insurance (Social Security Administration, various years).

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[14]In particular, many of the self-assessed measures are defined in terms of ability to work, rendering them clearly endogenous in a labor supply model. The strengths and weaknesses of self-assessed health status are discussed in Parsons (1982) Anderson and Burkhauser (1985), Bazzoli (1985), Butler et al. (1987), and Bound (1989b).

The relationship between BMI, self-reported (poor or fair) health status, and labor force non-participation is documented for our sample of older men in Figure 1. There is a strong U-shaped relationship with being in poor or fair health, and a somewhat shallower but still pronounced relationship with labor force participation.[15] Based on the correlation of BMI and self-reported health status, and on the previous literature, we use three categories of BMI to measure disability, in order of severity: BMI less than 22 or greater than 28; BMI less than 21 or greater than 32; and BMI less than 20 or greater than 34.[16]

The means of the NHIS data are reported in Table 2, for our full sample of 45-64 year old males, and separately for those in and out of the labor force last week. Being more highly educated, married, and younger is correlated with being in the labor force last week. For our disability measures, we divide the proportion reporting disability into those who are underweight (for their height) and those who are overweight. Being either under and overweight is associated with higher labor force non-participation, although the correlation seems stronger in the former case. 44% of older males are "disabled" according to our most broad definition of disability; only 19% are in our most narrow BMI group.

3.5 Denial Rates and Labor Force Non-Participation

3.5.1 Basic Results

Our estimates for equation (1) are reported in Table 3. The first column reports the effect on non-participation in the last week, and in the second column non-participation in the last year is the dependent variable. Since the probit coefficients are not directly interpretable, the figures in square brackets show the effect of a 10% increase in the denial rate on non-participation rates. In both cases, the results indicate that

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[15]This parallels the findings of Costa (1993) for the 1985-1991 period.

[16]We use larger increments on the overweight end of the scale because, as Figure 1 and previous research documents, the gradient of the health relationship is less steep than on the underweight end of the scale. An alternative to these BMI groupings, which would capture the U-shaped correlation between BMI and health, would be to use a quadratic in BMI. The disadvantage of this approach is that it is much less efficient because it doesn't impose a priori information about the most dangerous BMI ranges.

higher denial rates are associated with a lower rate of non-participation. For non-participation in the last week, the estimate is significant, and implies that a 10% increase in the denial rate leads to a 0.48 percentage point decrease in non-participation. At the mean level of non-participation in our sample (17.3%), this represents a response elasticity of -0.28.

A 10% increase in the denial rate lowers non-participation in the last year by 0.37 percentage points, but the estimate is only significant at the 15% level. However, non-participation is lower by this measure, so that the estimated elasticity of non-participation, -0.28, is the same as that using non-participation last week. For the reasons noted above, participation in the previous week is the preferred measure of labor force attachment for this exercise, and we will rely on that measure for the remainder of the empirical work; the results are all similar if participation last year is used.

The covariates in the model are all highly significant. Being married or more highly educated lowers the probability of being out of the labor force both in the previous week and the previous year. Being non-white raises the probability of non-participation in the last week, but lowers it in the last year, perhaps reflecting more transitory labor force attachment for this group. The probability of non-participation rises uniformly with age. Finally, non-participation rises with both the state/year unemployment rate and average income. The former most likely captures short-run labor market disequilibria, while the latter reflects the fact that retirement is a normal good.[17]

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[17]One potential problem with our approach, noted above, is that there may be an endogenous legislative response to local labor market conditions in setting DI denial rates. Indeed, examining Table 1, a clear regional pattern in denial rate changes is apparent, with the largest increases in Connecticut, New Jersey, New York, and Pennsylvania. In order to control for the possibility that there were region-specific shocks correlated with state decisions to raise denial rates, we have reestimated our basic model including a set of 4 region dummies (Northeast, Midwest, South, West), interacted with a dummy for being after the denial rate policy change. In this model, the effect of the policy is identified by within-region changes in denial rates and in labor force non-participation. As we show in Gruber and Kubik (1994), including these region interactions somewhat the estimated denial rate effect, which remains significant at the 7% level; there are similar effects on the results shown in Table 4 as well. Another potential omitted variable is changes in the structure of the Supplemental Security Income (SSI) program, another federal program for the (low income) disabled that is administered at the state level. In order to control for any correlated changes at the state level in the generosity of this program, we have included in our regressions two measures of SSI program generosity: state/year program enrollment and state/year supplementation of federal SSI payments. In neither case is there much change in our regressor of interest.

3.5.2 Interpreting the Basic Finding

The basic regression results are supportive of the contention that DI denial rates matter for labor force decisions. Even in the absence of a behavioral response among workers, however, an increase in the denial rate would lead to an decrease in the number of labor force non-participants, so long as some of those denied workers return to work. Thus, our estimate combines two effects: the effect on those denied applicants returning to work, and the effect of those persons who never leave work to apply for DI (the participation response).

Separating these two effects is difficult, since we do not have precise information on the fraction of denied applicants returning to work. Furthermore, we are modelling the stock decision to be in or out of the labor force, and denials are flows of workers. But we can do a rough calculation by cumulating the implied increase in the stock of denied applicants from 1978 to 1981 (the middle of our "after" period) from the denial rate increase after 1977. In 1977, there were 1.24 million applications to DI, and approximately 53% of those came from 45-64 year old men. Over the 1978-1981 period, if applications behavior had remained unchanged, there would have been 2.6 million applications from this group. Furthermore, if denial rates had remained unchanged, 53.8% of these applicants would have been rejected, for a total of 1.41 million rejections. When denial rates rose, the number of applications fell; using Parson's estimated elasticity of 0.45, each 10% (5 percentage point) rise in denial rates lowered the number of applications from 45-64 year old men to 2.5 million. At a 10% higher denial rate, the number of rejections would be 1.48 million, for an increase of 71,700 in the number of rejected applicants.

Given the size of the 45-64 year old male population in this era (20.7 million persons), we estimate that a 10% denial rate rise led to an increase in the number of workers of 99,360. Thus, even if all of the denied applicants had returned to work, our estimates would still imply some participation response.

Estimates of the likelihood of returning to work in this era are presented in Bound (1989) and Parsons (1991b). Bound's estimates that, 18 months or more after denial, 45% of denied applicants had returned to work (at least part-time). Parsons finds that 75% of denied applicants who are alive and not receiving government assistance are working 5 years after application. Using these two figures to bound the true return to work effect, our estimates imply that between 45,600 and 67,100 older male workers remained on the job rather than leave to apply for the DI program. The implied elasticities of non-participation, accounting for the participation response only, are therefore between -0.13 and -0.19.

As a point of comparison, it is useful to compare our estimate to the findings of the literature on potential DI benefits and labor force non-participation. The estimated effects in that literature range from an upper bound of 0.63 to 1.80 in Parsons (1980) to a lower bound of 0.06 in Haveman and Wolfe (1984). Most of the studies subsequent to Parsons (Leonard, 1979; Halpern and Hausman, 1986) have estimated non-participation elasticities in the range of 0.1 to 0.2. It is difficult to compare the effects of a benefit cut and an denial rate rise from the perspective of a given individual, but they can be compared from the perspective of the program budget constraint. In steady state, a 10% cut in benefits has the same revenue implications as a 10% fall in the DI acceptance rate.[18] A 10% fall in the DI acceptance rate, in turn, corresponds to a 9.1% rise in the denial rate in this era. Our estimates imply that a 9.1% rise in the denial rate would lead to a 1.2 to 1.7% fall in non-participation (focusing only on the participation response). Thus, according to this metric, our findings are in the same range as the post-Parsons benefits literature.

Another important point of comparison is with Parsons' (1991a) and Halpern and Hausman's (1986) applications elasticity of 0.45. The decision to leave the labor force and the decision to apply for DI are

[18]In transition, the benefits cut saves more money, because it will be both prospective to future applicants and retrospective to current recipients. So this statement is only true out of steady state if the benefits cut is a prospective one.

distinct, since there is not necessarily full takeup of DI benefits by potentially eligible older persons.[19] If there is less than full takeup, then the effect of denial rate changes on applications may exceed the effect on labor force participation. This is because high denial rates may make individuals who are already not participating in the labor force reluctant to apply for the program. For the individual who is on the margin between applying and not, a smaller likelihood of acceptance could lower the expected benefits of application below the tangible and psychic costs of application.[20]

In fact, it appears that there was a response among eligible non-participants to this change in program structure. Following the calculations presented above, and using the Parsons/Halpern-Hausman applications elasticity, we estimate that each 10% denial rate rise led to a fall in the number of applications of 117,900 over the 1977-1980 period. This is substantially higher than our estimated participation response of 45,600 to 67,100 persons. Thus, our findings imply that this policy change affected not only the propensity to leave the labor force, but the propensity to apply for the program conditional on labor force leaving.

3.6 The Targeting Efficiency of Raising Denial Rates

As noted above, it is ambiguous ex ante whether increasing denial rates can effectively target the labor force incentives of DI policy. In fact, it is theoretically plausible that denial rate rises can have a perverse effect, with a larger response among the truly disabled, if this group has (for example) higher

[19]Low takeup for various social insurance programs has been documented in numerous other studies (Blank and Card, 1991; Blank and Ruggles, 1993; Currie and Gruber, 1995). The reasons for low takeup are not well understood, but it is generally assumed that stigma over the receipt of government assistance plays a key role (Moffitt, 1983). It is difficult to calculate takeup for DI, since eligibility is a function of both work history and underlying disability. A rough calculation in Gruber and Kubik (1994) suggests a takeup rate for DI of approximately 62%.

[20]Evidence that such a relationship exists for the unemployment insurance program is provided by Blank and Card (1991), who find that takeup of unemployment insurance among eligibles fell as state disqualification rates rose.

discount rates or faces more binding capital market constraints. We investigate this question within the regression framework (2), using the anthropometric measure of disability BMI.

Table 4 reports the regression results. We report only the coefficients of interest from regressions that include all of the controls shown in Table 3. The first column replicates the basic regression from Table 3. The second column includes our first BMI category as a control variable. This variable is highly significant, and indicates that being in this BMI category raises the probability of non-participation by 15%. There is little effect on the denial rate coefficient from including this control.

In the third column, we interact the denial rate with the BMI indicator variable. The denial rate coefficient rises somewhat, while the interaction is positive, although not significant. The results therefore imply that the labor supply effect of the denial rate increase was smaller on less able older males: for "able" persons, non-participation falls by 0.58 percentage points for every 10% denial rate rise, while for the "disabled", the fall is only 0.37 percentage points. This suggests that there is some targeting efficiency to this policy.

In the remaining columns of Table 4 we tighten our definition of disability (ie. the last measure excludes more of the middle of the BMI distribution than the first measure). In each case, the coefficient on the denial rate itself is unchanged when BMI is added to the regression (the fourth and sixth columns). But the evidence of targeting efficiency increases as these more stringent definitions are used. For the second definition, the interaction term is significant at the 10% level, and indicates that the denial rate rise had an effect on disabled workers which was less than half that of able workers.

For the most stringent definition, which labels individuals as disabled only if they have BMI below 20 or above 34, increasing denial rates is estimated to have *no effect* on the participation of the disabled. For able workers, the estimated effect is now larger, with a 10% denial [illegible] leading to a 0.62 percentage

point non-participation rise. This measure also has the largest mean effect on labor force participation (column (6)); being disabled by this metric raises the probability of non-participation by 29%.

This striking finding suggests that denial rate increases are an effective means of targeting the labor market incentive effects of DI policy towards able workers. For workers who are labeled as able by our most stringent BMI measure, the 30% denial rate rise from 1977-1980 led to a 11.1% rise in non-participation. But for those labeled disabled, there was no effect.[21]

3.7 Conclusions

Disability insurance has grown rapidly in the U.S. over the past thirty years. While program growth slowed in the late 1970s and early 1980s, it has risen again recently, so that the DI program faces an imminent financing crisis. This rapid growth has been criticized on the grounds that imperfect targeting and high benefits cause able individuals to use this program to subsidize early retirement. This criticism, in turn, has given rise to a large literature on the effects of potential DI benefits on labor force participation. But there has been little emphasis on the other policy tools at the disposal of DI policy makers.

We have examined one such policy tool, the denial rate for initial DI applications. We did so by exploiting the large rise in denial rates across the states in the late 1970s. We found that increased denial rates did lead to decreases in labor force non-participation, suggesting that there is some moral hazard involved with the imperfect targeting of DI. Our estimated incentive effects lie within the range of recent estimates of the effect of potential DI benefits on labor force non-participation. Furthermore, taken in

[21]If the denied applicants are disproportionately able, we would expect a larger effect on the labor force participation of that group due to the return to work effect. But even if all denied applicants were in the 80% of the population labelled most able by our third BMI measure, and even if all denied applicants return to work, it could not explain the difference between the estimated effects on that group in Table 4 and those on the total population in Table 3 (see Gruber and Kubik, 1994, for an illustrative calculation).

conjunction with earlier evidence on the applications response to denial rate changes, our findings suggest that there is a sizeable response of applications to denial rate changes among those out of the labor force.

We also found that the increase in work incentives appears to have been efficiently targeted to the more able portion of the older male population. This suggests that, conditional on the same mean labor force incentive effect as other policies, denial rate increases may be particularly desirable means of reducing the moral hazard problems induced by DI.

Of course, our findings only solve half of the puzzle for the government policy-maker who is evaluating the optimal level of the DI denial rate. An efficient increase in screening would lower both the Type I and Type II errors of the process. But uniform increases in denials may lower Type I error while raising Type II error, even among those truly disabled persons who continued to apply for the program. That is, even if applications don't fall among the truly disabled, there may still be a fall in the rate at which they are accepted onto the DI program.

This lowered probability of acceptance for truly disabled individuals imposes a welfare cost which must be weighed against the welfare gains from reduced distortions to labor supply for more able workers. Data on the health of accepted and rejected DI applicants before and after the denial rate increases could be potentially used to directly measure the ratio of increased Type II to reduced Type I error, in an effort to fully assess the welfare implications of this policy change.[22]

[22]For detailed theoretical analyses of optimal DI policy when there is Type I and Type II error, see Diamond and Sheshinski (forthcoming).

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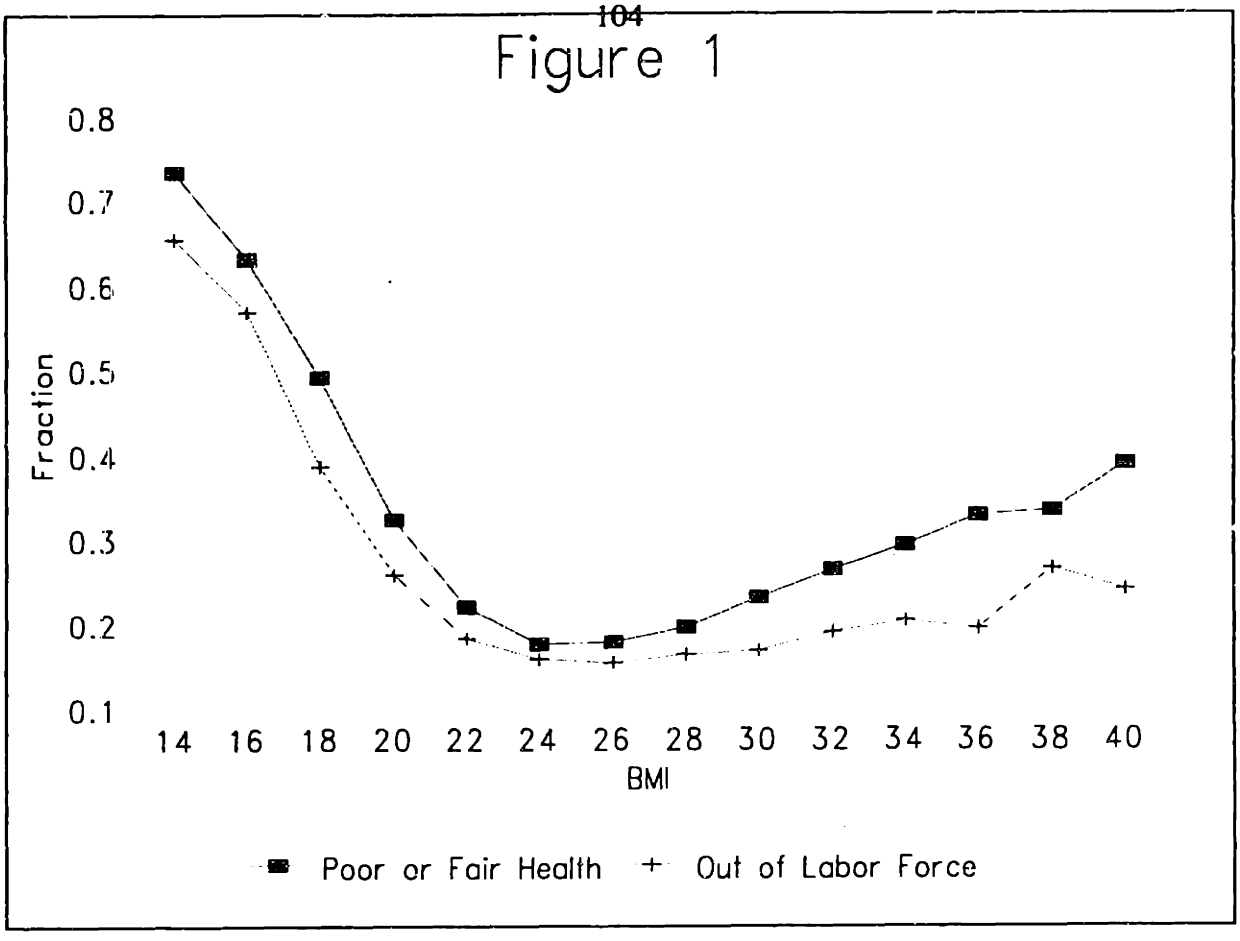
Figure 1


0.8
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14 16 18 20 22 24 26 28 30 32 34 36 38 40
BMI
Poor or Fair Health
Out of Labor Force

Table 3.1: Denial Rates Across States and Over Time

State	Denial Rate 1977	Denial Rate 1980	Percentage Change
AL	64.5	69.3	7.4
AZ	51.9	64.8	24.9
CA	55.6	69.3	24.6
CO	55.5	64.9	16.9
CT	47.3	76.2	61.1
DE	52.1	54.7	5.0
FL	58.1	71.4	22.9
GA	56.8	76.5	34.7
ID	56.5	72.5	28.3
IL	59.1	67.2	13.7
IN	50.7	63.5	25.2
IA	48.8	65.6	34.4
KS	50.3	61.6	22.5
KY	60.5	69.8	15.4
LA	58.1	74.5	28.2
ME	53.5	66.3	23.9
MD	60.3	69.0	14.4
MA	54.8	67.8	23.7
MI	54.4	72.8	33.8
MN	47.7	63.7	34.4
MS	59.8	71.2	19.1
MO	54.0	64.8	20.0
MT	55.4	65.5	18.2
NE	48.3	57.5	19.0
NV	56.4	67.3	19.3
NH	56.9	76.6	34.6
NJ	40.2	59.3	47.5

Table 3.1: Denial Rates Across States and Over Time, Continued

State	Denial Rate 1977	Denial Rate 1980	Percentage Change
NM	66.7	73.1	9.6
NY	47.8	79.7	66.7
NC	54.1	65.3	20.7
ND	55.3	67.9	22.8
OH	49.5	65.0	31.3
OK	59.6	72.9	22.3
OR	60.9	66.5	9.2
PA	47.4	73.4	54.9
RI	46.9	53.9	14.9
SC	57.2	70.2	22.7
SD	52.3	59.3	13.4
TN	62.4	72.3	15.9
TX	60.6	69.8	15.2
UT	50.7	62.5	23.3
VT	49.6	65.9	32.9
VA	61.3	68.0	10.9
WA	53.0	70.1	32.3
WV	60.4	76.2	26.2
WI	47.5	57.4	20.8
WY	59.3	71.2	20.1
Weighted Average	53.8	69.3	30.1

Notes: Figures are average denial rates for the state/year cell.

Table 3.2: Means of the Dataset

	Full Sample	In Labor Force Last Week	Out of Labor Force Last Week
Out of Labor Force last week	.1726 [.3779]		
Out of Labor Force last year	.1313 [.3377]	.0136 [.1157]	.7065 [.4554]
Years of Education	11.51 [3.569]	11.83 [3.435]	9.976 [3.790]
Married (%)	85.25	87.36	75.15
Nonwhite (%)	9.021	8.20	12.96
Age 45-49 (%)	26.20	29.43	11.21
Age 50-54 (%)	26.54	28.90	15.27
Age 55-59 (%)	25.21	25.22	25.20
Age 60-64 (%)	21.95	16.45	48.32
BMI1 (BMI<22, BMI>28)	43.87	42.65	49.61
BMI<22 (%)	10.15	9.321	14.03
BMI>28 (%)	33.72	33.33	35.58
BMI2 (BMI<21, BMI>32)	24.25	22.93	30.43
BMI<21 (%)	5.539	4.735	9.291
BMI>32 (%)	18.71	18.19	21.14
BMI3 (BMI<20, BMI>34)	18.86	17.81	23.72
BMI<20 (%)	2.680	2.089	5.441
BMI>34 (%)	16.18	15.72	18.28
Number of Obs	60825	50324	10501

Notes: Figures from authors' tabulations of the 1976-1978 and 1980-1982 NHIS data (except for out of labor force last year, which is only consistently reported through 1981).

Table 3.3: The Effect of Denial Rate Increases on Labor Force Participation
Dependent Variable is a Dummy for Labor Force Non-Participation

	(1) NP Last Week	(2) NP Last Year
Denial Rate	-.2221 (.1132) [-.0048]	-.2072 (.1392) [-.0037]
Married	-.4664 (.0167)	-.2102 (.0206)
Nonwhite	.1136 (.0218)	-.0733 (.0279)
Education	-.0671 (.0018)	-.0506 (.0022)
Age 45-49	-1.130 (.0192)	-1.418 (.0249)
Age 50-54	-.9730 (.0180)	-1.150 (.0216)
Age 55-59	-.6370 (.0167)	-.7250 (.0190)
State Unemployment	2.543 (.8291)	2.878 (1.032)
State Income	.0532 (.0280)	.0719 (.0387)
Log Likelihood	-23809.4	-16486.8
Number of obs	60825	51101

Notes: Standard errors in parentheses; probability increase from 10% denial rate rise in square brackets. All regressions run as probits. All regressions include a full set of state and year dummies. Column (3) includes interaction of 4 region dummies with a dummy for being after the policy change.

Table 3.4: Targeting Efficiency: Dependent Variable is a Dummy for Labor Force Non-Participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Denial Rate	-.2221 (.1132) [-.0048]	-.2269 (.1132) [-.0049]	-.2713 (.1175) [-.0058]	-.2229 (.1133) [-.0049]	-.2664 (.1158) [-.0057]	-.2243 (.1133) [-.0048]	-.2903 (.1154) [-.0062]
BMI<22, BMI>28 (BMI1)		.1212 (.0139) [.0264]	.1709 (.0379) [.0373]				
Denial Rate * BMI1			.0972 (.0690) [.0021]				
BMI<21, BMI>32 (BMI2)				.2152 (.0176) [.0486]	.2867 (.0482) [.0657]		
Denial Rate * BMI2					.1340 (.0843) [.0029]		
BMI<20, BMI>34 (BMI3)						.2229 (.0213) [.0509]	.3897 (.0593) [.0926]
Denial Rate * BMI3							.2994 (.0995) [.0065]
Log Likelihood	-23809.4	-23771.5	-23770.5	-23735.5	-23735.2	-23754.9	-23750.4
Number of obs	60825	60825	60825	60825	60825	60825	60825

Notes: Standard errors in parentheses; probability increase from 10% denial rate rise in square brackets. All regressions run as probits. All regressions include the covariates shown in Table 3, as well as a full set of state and year dummies.