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HEALTH AND LABOR MARKET CONSEQUENCES OF ELIMINATING FEDERAL DISABILITY BENEFITS FOR SUBSTANCE ABUSERS

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ABSTRACT

Using annual, repeated cross-sections from national household survey data, we estimate how the January 1997 termination of federal disability benefits for those with Drug Addiction and Alcoholism affected labor market outcomes, health insurance, health care utilization, and arrests among individuals targeted by the legislation. We employ propensity score methods and a difference-in-difference-in-difference approach to mitigate potential omitted variables bias. Declines in SSI receipt accompanied increases in labor force participation and current employment, but had little measurable effect on insurance and utilization. In the long-run, (1999-2002), rates of SSI receipt rebounded somewhat, and short-run gains in labor market outcomes waned.

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I. Introduction

At the heart of the debate about the government's role in providing economic support to individuals with substance disorders are opposing views about the relationship between substance use disorders (SUDs) and economic dependency. Some regard SUDs as any other disabling illness; by this view, afflicted persons may require government support in order to meet their basic needs (Rosenheck, Frisman, & Gallup, 1995; Rossi, 1989; Sosin & Grossman, 1991). An influential analysis documented striking similarities in the heritability and influence of environmental factors, the rate of adherence to recommended treatment, and relapse rates among those with drug dependence compared to patients with type II diabetes mellitus, hypertension, and asthma (McClellan et al. 2000). Others focus on the behavioral aspect of SUDs, and imply that public programs may foster economic dependency and even encourage substance use by providing resources that can be used to purchase substances and by reducing incentives to work (Cohen, 1994; Phillips, Christenfeld, & Ryan, 1999; Shaner et al., 1995). Athough recent studies do not support this latter claim (Rosen, McMahon, Lin, & Rosenheck, 2006; Swartz, Hsieh, & Baumohi, 2003), the idea already has affected public policy. Most dramatically, in March 1996, the US Congress passed legislation barring persons with disabling substance disorders from receiving Supplemental Security Income (SSI) and Disability Insurance (DI) benefits. Prior to this change, which took effect in January 1997, about 209,000 individuals with substance disorders had been receiving SSI, DI, or both (Gresenz, Watkins, & Podus, 1998).

Whether terminating public assistance benefits to individuals with substance disorders encourages economic independence, and whether loss of these benefits causes harm remains a key concern among policymakers. Surprisingly, this fundamental question has not been answered by research, despite recent, major policy shifts in federal disability programs targeting

individuals with substance disorders, and despite the backdrop of growing emphasis on personal responsibility towards all potential public assistance recipients (Davies et al., 2000). The goal of this paper is to estimate the effects of terminating federal disability benefits for individuals likely to have SUDs on labor market, health, and criminal justice outcomes. To estimate these effects, we pooled annual cross-sectional data from 1994-2002 in the National Household Survey on Drug Abuse (NHSDA) and the National Survey on Drug Use and Health (NSDUH). We use a modified difference-in-difference-in-difference (DDD) approach, comparing outcome changes between a group likely to have been affected by the policy change – substance abusers with a high probability of SSI use – and a group of non-substance-abusers with high a probability of SSI use, both before and after the policy change, thus netting out trends in outcomes during this time period that may have affected substance abusers and non-substance abusers with a low probability of SSI use.[1] To make non-substance abusers more comparable to substance abusers in our sample, we used propensity score-based weighting to balance the distribution of observable characteristics across these two groups. Our approach and data source allow us to augment and improve existing evidence on this policy change in several way.: Unlike prior studies of this policy change, we considered both short-run (1997-1998) and longer run (1999-2002) effects of the policy, we incorporate a comparison group for the group targeted by the policy change, and we assess a range of health, labor market, and social outcomes in a nationally representative sample of individuals.

[1] In our empirical estimation described in section 3, we use a triple interaction between the continuous probability of SSI use, presence in the substance abuser group, and an indicator for post-policy change, rather than a strict DDD estimate using a discrete variable for high probability of SSI use,

Using this DDD approach combined with propensity score-based weighting, overall, our results show that individuals with substance problems did increase their economic self-sufficiency after the loss of public support benefits during the mid 1990's, without measurable impact on insurance coverage and health services utilization. The gains were relatively short-lived among individuals reporting symptoms of substance abuse or dependence, among whom SSI receipt, employment, and labor force participation returned to levels closer to the pre-period rates after 1998.

In the short-run, 1997-98, the policy change accompanied higher participation in the labor force and current employment, and lower use of SSI among individuals likely to use SSI and likely to have substance abuse or dependence. In contrast to prior studies reporting a drop in Medicaid use, we found no significant changes in rates of health insurance coverage from any source or health services utilization. Welfare receipt changed little in the short-run. In the long-run, 1999-2002, rates of SSI receipt rebounded somewhat, but welfare use fell, and effects on employment and labor force participation persisted among a broad group of moderate to heavy substance users. Long-run employment gains were more modest and stemmed mainly from part-time rather than full-time work among individuals reporting symptoms of substance abuse or dependence. Not surprisingly, labor force participation among individuals reporting symptoms of substance abuse or dependence fell in the long-run after increasing in 1997-98. We find no long run differential trend in health insurance status and utilization. . Finally, we find suggestive evidence that arrest rates rose within the targeted groups following the policy change, especially after 1998.

Section II of the paper describes changes in public support programs affecting individuals with substance abuse and the evidence to date on the consequences of the elimination

of SSI/DI for drug abuse and alcoholism. Section III describes our empirical strategy. Section IV describes the data used, Section V presents the results of our analyses, and Section VI discusses the implications and conclusions from our results.

II. Changes in public programs affecting individuals with SUDs

A. Elimination of Federal Disability Benefits for Substance Abusers

The federal SSI and DI programs are public income assistance programs for the disabled. Low income elderly, blind, and disabled persons can qualify for SSI payments, while DI provides federal disability insurance, and more generous cash payments to covered workers. Disabled workers with adequate earnings history are eligible for DI benefits. However, low-income disabled workers with very limited DI benefits can supplement low benefits with SSI to reach SSI benefit levels. Participation in the DI program for at least 24 months entitles beneficiaries to Medicare, and SSI recipients in most states automatically qualify for Medicaid (Gresenz et al., 1998). From the inception of the SSI/DI programs in the early 1970s, substance disorders were considered potentially disabling conditions. Initially, however, the number of beneficiaries in this impairment category was very small (Guydish, Ponath, Bostrom, Campbell, & Barron, 2003). During the late 1980s and early 1990s, the number of SSI recipients with substance disorders began to grow rapidly – between 1989 and 1994, the number of SSI recipients in the substance disorder disability category increased from 16,100 to 101,685 (Barber, 1996). In response, the federal government in 1994 imposed a three year time limit on receipt of disability benefits for those with disabling substance conditions (Guydish et al., 2003). In addition, substance abusers were required to be in treatment, which would be monitored and enforced by referral and monitoring agencies.

Despite these changes in 1994, the number of SSI/DI recipients with substance abuse problems continued to rise during the 1990s, although as a group, beneficiaries qualifying due to drug abuse or alcoholism comprised less than 3 percent of the total SSI/DI adult population (Stapleton, Wittenburg, & Tucker, 1998). The federal government became increasingly concerned that recipients were not engaging in treatment and returning to work, as intended by the program (Gresenz et al., 1998). Moreover, despite the requirement that SSI/DI payments to addicted persons be managed by individuals called representative payees, there was concern that disability payments were being used to purchase drugs. There was at that time and continues to be mixed empirical support for this claim (Catalano & McConnell, 1999; Frisman & Rosenheck, 1997; Rosen et al., 2006; Shaner et al., 1995; Swartz et al., 2003). Nevertheless, these perceptions contributed to a major policy change.

In March 1996, the US Congress passed legislation terminating eligibility for SSI/DI programs on the basis of disabling drug abuse and/or alcoholism (DA&A); new and pending applications for cases in which a SUD was material to the disability determination would no longer be considered (Davies et al., 2000). The legislation also mandated that existing DA&A recipients be terminated from the program as of January 1, 1997 (Davies et al., 2000).

Individuals with SUDs and a co-occurring disabling mental or physical condition were given the opportunity to be re-assessed for SSI/DI eligibility based on their co-occurring disability. If re-assessed successfully, these recipients regained their cash and health insurance benefits through the SSI/DI programs, but without required treatment activity or a representative payee (Watkins & Podus, 2000). Of the 209,000 beneficiaries targeted by the January 1997 policy change, 80% were SSI beneficiaries and only 11% were DI beneficiaries who had never

received SSI.[2] One year after the policy change, about 71,000 of the 209,000 targeted beneficiaries had re-gained SSI/DI eligibility (Stapleton et al., 1998). In other words, receipt of disability insurance among individuals disabled by SUDs fell by two thirds. As individuals with little work history who do not qualify for the more generous DI benefits, and given that the vast majority of DA&A recipients received SSI payments, we expect that potential SSI recipients were those most vulnerable to any adverse consequences of the program change. Thus, for this reason and due to data limitations regarding DI receipt, we focus on SSI recipients in this analysis. Figure 1 shows administrative estimates of the overall SSI caseload, those collecting benefits for DA&A, and for SSI recipients qualifying due to a mental health disorder (which would include the DA&A population) over the 1990s and after 2000. The drop in SSI receipt from 1996 to 1997 is sudden, dramatic, and largely offset by later gains in the mental health caseload.

B. Evidence on the Effects of Losing Disability Benefits on Individual Outcomes

Individuals that received SSI/DI payments for DA&A are an extremely disadvantaged group. DA&A clients were mostly male (about 73 percent) and middle-aged (about 40 percent were between 40 and 49 years old) with high levels of psychiatric impairment, medical co-morbidities, limited work experience, and low levels of education (Davies, Iams, & Rupp, 2000; McKay, McLellan, Durell, Ruetsch, & Alterman, 1998; Stapleton et al., 1998). Disability recipients targeted by the program changes had extensive criminal histories; a study of one large Referral and Monitoring Agency suggests that about 84% of this population had been charged

[2] DI recipients whose benefits are below SSI levels can qualify for SSI to increase benefit levels. Thus, many DA&A beneficiaries, about 120,000 qualified for both programs simultaneously (Campbell, Baumhol, and Hunt 2003).

with any criminal offense, and almost a third of males had a history of charges for a violent crime (Stapleton et al., 1998). As of 1995, about 53% of substance abusers receiving SSI benefits were classified as alcohol dependent, 18% were classified as drug dependent, and the remaining 29% had both alcohol and drug dependence (Barber, 1996). The average monthly SSI payment to disabled substance abusers was $425 in 1995, and 69% of substance abusers receiving SSI in 1995 had no other source of income (Barber, 1996).

Given these characteristics, there has been concern that former DA&A recipients may have faced significant barriers in entering the labor market and finding jobs after the policy change took place. Moreover, losing SSI/DI cash benefits, as well as losing the oversight of treatment and access to Medicare or Medicaid, may have adversely affected these individuals' mental and physical wellbeing, utilization of health services, and criminal involvement. The few studies available on this topic, however, offer mixed support for this idea.

Watkins, Podus, Lombardi, & Burnam (2001), for example, follow 253 SSI beneficiaries in Los Angeles, interviewing them for the first time around the time the 1997 policy change went into effect and then again at 12 month, 18 month and 24 month follow-up interviews. Surprisingly, they find no evidence that the mental health status of respondents declined during this period, even though only 106 of the 253 respondents were still receiving SSI benefits at the 24 month follow-up interview. There also was no increase in emergency department visits and hospitalizations among respondents who lost SSI benefits (Watkins et al., 2001). The authors posit that the lack of adverse consequences can be traced to other county-based programs such as General Assistance which may have replaced SSI benefits.

Guydish et al. (2003) report similar null findings in a multi-site study of 1,670 individuals who at baseline were receiving SSI benefits for a disabling substance use condition. Most

baseline interviews were conducted between November 1996 and March 1997. Respondents were then interviewed and administered the Addiction Severity Index (ASI) every six months over a two year follow-up period. Over time, they find either no change or improvement in ASI score, and no apparent association between ASI score and SSI receipt status. In the same multi-site study, and the only evidence of labor market outcomes to date, Campbell et al. (2003) assessed employment and earnings among 661 study subjects who were not collecting SSI, welfare, or general assistance in the 24 months following the policy change. They found two-fold to three-fold increases in employment rates six months after baseline, but earnings were very low. After 24 months, less than 25 percent of respondents earned more than $500 per month (equivalent to the level of Substantial Gainful Activity) in six out of nine cities studied.

Finally, a recent study based on data from Chicago indicates that termination of SSI benefits may have led to reduced access to Medicaid. Hanrahan et al. (2004) used longitudinal data on 11,740 individuals who had been receiving SSI benefits for a substance disorder in Chicago in 1995, and showed that by 1998, almost half of these individuals had lost their Medicaid coverage (Hanrahan et al., 2004). The limited geography of this study makes it difficult to infer results in other parts of the country.

Because of the tremendous economic expansion of the late 1990s, our understanding of the effects of the policy change would be enhanced by comparing individuals targeted by the change to a control group of individuals unlikely to be affected by the termination of disability benefits. Furthermore, while results such as those found in Chicago regarding Medicaid declines are provocative, further information from a more geographically diverse sample would augment the evidence available regarding the elimination of cash benefits to individuals with disabling SUDs. Finally, with the passage of time, it is now possible to assess longer term effects of

terminating disability benefits for SUDs, something that wasn't possible in prior studies.

C. Contribution of Our Study

The key question of interest to policymakers is the following: has the elimination of federal disability payments, as well as the health insurance and monitoring of treatment and finances that the disability programs previously had provided, had long-term effects on labor market, health, and criminal behavior outcomes in the national population of individuals with disabling substance problems? Prior studies in this area were based on data from geographically limited, narrowly defined populations and followed respondents for only about one to two years after the policy change took effect in 1997. Moreover, an important methodological limitation of prior studies is the lack of comparison groups; in these studies, outcomes are compared pre and post policy change in a group that was affected by the policy. None of the existing studies described above can provide evidence on total effects of policies terminating benefits to those with substance use disorders. These policies can affect not just those receiving benefits at the time of the legislation, but potential new applicants who were shut out of benefits, as well as those individuals diverted from the welfare caseload, who might otherwise have qualified for SSI benefits. Even among those who maintained benefits because of co-occurring disorders, the new policy eliminated the requirement that individuals obtain treatment for their substance disorder, which might reduce the amount of treatment sought and obtained by those on SSI. SSI leaver studies cannot capture any of these effects.

The present study, on the other hand, is based on repeated, nationally representative cross-sectional samples of individuals from the 1994-2002 NHSDA/NSDUH. The use of the NHSDA/NSDUH allows us to examine for the first time the long-term, national effects of this important policy change. Also, a major advantage of our paper is the utilization of a comparison

group as well as several other methods to help control for or mitigate other, potentially confounding factors and trends that may be related to the outcomes of interest. Because most of the individuals affected by the policy change collected SSI benefits, and because we know of no data offering similar detail on substance use and symptoms of dependence combined with DI receipt, and finally because of the more vulnerable situations among SSI recipients, we focus on program effects among individuals likely to use SSI.

3. Empirical Approach

A. Identification Strategy

In a non-experimental setting, the central challenge in estimating the effects of any public policy change on individual-level outcomes is determining whether or not an observed association represents a causal relationship. The current analysis shares this methodological hurdle. When analyzing the effects of the termination of disability benefits on economic and health care outcomes, results may arise from a causal relationship or from unmeasured factors affecting the outcome variable of interest at the same time as a major policy change. In this case, the association between the policy change and the outcome would not necessarily be causal.

In this paper, we combine several methods to attempt to circumvent this problem. Our general strategy is to compare changes in mean outcomes before and after the policy change for individuals most likely to have been targeted by the change and for similar individuals who were unlikely to have been affected. Our potentially targeted group consists of individuals who have both elevated propensity to use SSI based on their observed characteristics as well as recent heavy substance use and/or symptoms of substance dependence or receipt of substance abuse treatment (substance measures described in detail in the next section). We compare changes pre-

and post- policy change, among individuals who are and are not likely to have SUDs, interacting these characteristics (time period and treatment group) with an individual's propensity to use SSI. This difference-in-difference-in-difference (DDD) approach mitigates the problem of unobserved heterogeneity, or the possibility that high-propensity SSI substance abusers have fixed, unmeasured characteristics that differ from other high-propensity SSI individuals (e.g., greater likelihood of co-morbid mental disorders) and affect the outcomes of interest. We opt for a DDD approach to account for potentially confounding trends in outcomes between substance abusers and non-abusers that are not causally related to the policy change. Thus, by differencing out changes in outcomes between substance abusers and non-abusers, we address the possibility that an economy-wide trend that affected all substance abusers relative to non-abusers during this time period confounds the effects of the policy change (e.g. that substance abusers respond last to economic expansions).

The basic econometric specification, ignoring covariates for the moment, is as follows.

$$\begin{aligned} E[Y_{it}] = {} & \alpha + \beta_1 SA_{it} + \beta_2 \Pr(SSI)_{i\,\Pr e-} + \beta_3 SA_{it} * \Pr(SSI)_{i\,\Pr e-} + \beta_4 After1996_t + \\ & \beta_5 After1998_t + \beta_6 SA_{it} * After1996_t + \beta_7 SA_{it} * After1998_t + \\ & \beta_8 * After1996_t * \Pr(SSI)_{i\,\Pr e-} + \beta_9 * After1998_t * \Pr(SSI)_{i\,\Pr e-} + \\ & \beta_{10} * SA_{it} * After1998_t * \Pr(SSI)_{t\,\Pr e-} + \beta_{11} * SA_{it} * After1996_t * \Pr(SSI)_{i\,\Pr e-} \end{aligned} \quad (1)$$

Where SA_{it} = Substance Abuser (treatment group); $Pr(SSI)_{iPre-}$ = propensity to use SSI during 1994-96 period; $After1996_t$ = indicator for observations in 1997-2002; and $After1998_t$ = indicator for observations in 1999-2002.

In this set up, Y_{it} is an outcome variable for individual i in year t. The key independent variables are the two triple interaction terms between substance abuser, the probability of SSI use based on pre-period program rules, and each of the two binary variables representing time

periods after the federal government terminated DA&A benefits (After 1996 and After 1998). These coefficients represent the changes in outcomes pre- and post- policy change for substance abusers versus non-abusers allowing the effect to vary by individuals' probability of SSI use, based on pre-period relationships between observed characteristics and SSI receipt.

The After 1996 and After 1998 variables are determined only by time. The variables both take on a value of zero before the policy change (1993–1995 calendar years which correspond to the 1994-1996 survey years). After the policy change, the After 1996 variable takes on a value of one for the rest of the study period (1996-2001 calendar years which correspond to the 1997-2002 survey years). Throughout the paper we will refer to survey years, rather than calendar years. The After 1998 variable takes on a value of one for the 1999 to 2002 survey years. We view the policy change as occurring in calendar year 1996, since it was in March of this year that the legislation was enacted and individuals could no longer apply for SSI benefits if they had a substance-related disability. Note that this set-up allows the "After 1998*SA" indicator and the interaction terms that include this indicator to capture the marginal effects of the policy change in the long-run (1999-2002), relative to effects during 1997-98.

In this model, the DDD coefficients (β_{10} and β_{11}) are unbiased estimates of the policy effects in the short-run and the long-run effects relative to short-run effects. This interpretation will be correct as long as our identifying assumption, that no unmeasured event or policy change occurred at the same time as the SSI policy change that affected trends in outcomes over time differentially for substance abusers with high propensity to use SSI, holds. For example, a crucial policy change occurring during our study period was welfare reform, which was implemented in the mid- to late 1990s depending on the state. After passage of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), welfare benefits

were subject to 60-month lifetime limits with shorter limits (12-36 months) for continuous benefit receipt in many states, and welfare recipients were subject to work requirements in exchange for cash payments. Welfare recipients who did not comply with requirements under PRWORA could have benefits sanctioned (either reduced or terminated). As 90 percent of welfare recipients are female, this would affect females disproportionately. Because our method differences out pre- and post- trends for individuals likely to use SSI benefits (presumably a group that also has elevated propensity to use welfare benefits), our approach may be contaminated by welfare reform only to the extent that its provisions differentially affected likely welfare users who were substance abusers.

Some provisions of PRWORA could plausibly affect substance abusers more than others. Convicted felons can be denied welfare benefits under PRWORA, and the legislation allowed drug testing of welfare recipients so that states could deny benefits to individuals testing positive for illicit drug use. In practice no state has used suspicionless drug testing, but this could create an environment in which drug using women are less likely to apply for welfare benefits. Evidence on how PRWORA affects drug using women offers mixed results. Women reporting symptoms of substance dependence were more likely to be on welfare in the mid- to late 1990s (Danziger et al. 2000; Meara and Frank 2007; Montoya et al. 2003), but also more likely to be sanctioned following welfare reform (Meara and Frank 2007; Morgenstern 2003). Over a longer period, women with a history of illicit drug use were less likely to be on welfare (Pollack and Reuter 2006), but this disproportionate exit from welfare occurred after 2000, so it is likely to affect our long-run estimates only.

B. Generating the Probability of SSI Use Based on 1994-96 Policies

To create the probability of SSI use, we first estimate logit models to predict the

probability of SSI use during the period before the legislative change (survey years 1994 through 1996) as a function of demographic characteristics.

$$\Pr(\mathrm{SSI}_{it} = 1) = f(X'_{it}\beta) \qquad (2)$$

In this set-up, $\mathbf{X_{it}}$ is a vector of characteristics for individual i, described below, and f(.) is the logistic function, or $f(.) = e^x/(1+e^x)$. Using the coefficients from these models, we estimate the propensity to use SSI for the sample in all years of data. We then generate a continuous variable, $\Pr(SSI)_{iPre-}$, indicating the respondent's predicted propensity to use SSI under the pre-period program rules. This variable and its interactions with other variables are then included in Equation 1, as described above.[3]

C. Using Propensity Score Models to Balance Treatment and Control Groups

As described above, the main methodological challenge to this study is forming an appropriate treatment and control group to address the potential omitted variable bias that could affect our interpretation of estimates as the effect of terminating disability benefits for substance abusers. We use propensity score-based weights to balance observed characteristics between substance abusers and others in our control group.

[3] In practice, 99% of pre-period respondents had a predicted probability of SSI use below .25, and the propensity distribution most closely resembles actual SSI use for these respondents, while propensities were higher than actual SSI use among respondents in the top percentile of the SSI propensity distribution. To reduce the noise introduced by this error in predicting SSI propensity at the top of the distribution, we ranked respondents on predicted SSI use, divided the sample into 25 bins averaging 6,250 respondents each, and replaced the predicted Pr(SSI) with the average actual SSI use within that bin during the pre-period (survey years 1994-1996). Thus, our SSI propensities range from near zero to just under .25.

Using survey years 1994-1996, we estimate a logit model that predicts whether an individual is a substance abuser as a function of observed characteristics.

$$\Pr(SA_{it} = 1) = f(X'_{it}\beta) \quad (3)$$

This model has the same covariates as in (2) and again, $f(.) = e^x/(1+e^x)$. We use the estimated coefficients from (3) to generate the predicted probability of being a substance abuser, *p*, and we weight each observation in our sample by the probability of being in the opposite group. That is, individuals in the substance abuser group receive a weight of $(1 - p)$, and individuals not in the substance abuser group receive a weight equal to *p*. This gives more weight to observations with characteristics similar to the opposite group, and automatically prevents single observations from receiving extremely high relative weight. By construction, this propensity score weighting technique, used previously in the clinical and health services literature, forces the distribution of observed characteristics in the propensity model to be identical across the two groups (McWilliams et al. 2003). In the paper, all estimates presented weight each observation with the propensity-score-based weight multiplied by the sample weight provided in the NHSDA/NSDUH surveys, to make the data representative of the target population for the surveys. This modified weight balances the distribution of observed characteristics during the pre-period among substance abusers versus the comparison group.

In equations (2) and (3), we control for the following individual characteristics (**X**): gender, race/ethnicity (Black non-Latino, Latino, and Other non-Latino race versus white non-Latino), interview conducted in Spanish, age categories (22-23, 24-25, 26-29, 30-34, 35-49, 50-65 versus 18-21), marital status (widowed, divorced, never married versus married), education (5 or fewer years, 6, 7, 8, 9, 10, 11, 13, and 14-15, versus 12 years), veteran status, number of household inhabitants (dummies for 2 through 6+ versus living alone), population density

category (MSA > 1 million, MSA < 1 million versus not in an MSA), self-reported health status (poor, fair, good, very good versus excellent), ever used illicit drugs, used alcohol before age 15, used marijuana before age 17, and whether the respondent ever smoked daily.[4]

D. Estimation

Once we have obtained propensity-score based weights for each observation, we estimate models of our main labor market, health insurance, health care and arrest related outcomes. To aid interpretation of our coefficients of interest, we estimate these as linear probability models as in equation 4.

$$\begin{aligned} Y_{it} = {} & \alpha + \beta_1 SA_{it} + \beta_2 \Pr(SSI)_{i\,\Pr e-} + \beta_3 SA_{it} * \Pr(SSI)_{i\,\Pr e-} + \beta_4 After1996_t + \beta_5 After1998_t + \\ & \beta_6 SA_{it} * After1996_t + \beta_7 SA_{it} * After1998_t + \beta_8 * After1996_t * \Pr(SSI)_{i\,\Pr e-} + \\ & \beta_9 * After1998_t * \Pr(SSI)_{i\,\Pr e-} + \beta_{10} * SA_{it} * After1996_t * \Pr(SSI)_{i\,\Pr e-} + \\ & \beta_{11} * SA_{it} * After1998_t * \Pr(SSI)_{i\,\Pr e-} + T_{tk}' \Gamma + \varepsilon_{it} \end{aligned} \quad (4)$$

In addition to the substance abuse indicator, the indicator of SSI propensity, the After 1996 and After 1998 indicators, and interactions between these variables, we also include as controls a set of measures, **T**, that vary across time t, and in some cases across group, k. These measures include the race/ethnicity-gender-specific national unemployment rate, the national rate of arrests per 100,000, and the national rate of incarceration per 100,000. By controlling for these secular changes, we rule out the possibility that measured effects resulted from rising incarceration rates or group-specific employment cycles that were unrelated to the policy change.

[4] We tested whether our results were sensitive to the inclusion or exclusion of self-reported health status, given that it could plausibly be affected by the policy of interest, discontinuing disability benefits, but our results were unchanged, and precision was improved by the inclusion of health status in propensity models.

Because of the binary nature of our outcome variables, we also estimate logit models of the above equation, and present these results in the appendix for comparison. In the logit specification, one cannot interpret coefficients on the triple interaction terms as the DDD in the probability of a binary outcome, as discussed in detail by (Ai and Norton 2003). Instead, we present results on the magnitude of effects based on predicted probabilities obtained from the logit coefficients in the models in three ways. In each case, we compare results for a 10 percentage point increase in $pr(SSI)_{Pre}$ from .05 to .15, which allows us to make estimates within our range of data. First we present the DDD of effects in the early post-period, 1997-1998 versus 1994-1996 for substance abusers versus non- and for SSI propensities of .05 versus .15. Second we present the similar estimates of effects, but comparing the later post-period, 1999-2002 versus 1994-1996. Finally, we present the DDD estimate of effects in 1999-2002 versus 1997-98. For each of these, we bootstrap the standard errors with 100 replications. The OLS and logit model coefficients estimated here reflect Huber/White-adjusted standard errors to address arbitrary heteroskedasticity and the correlation of observations within the primary sampling units, or counties, in the surveys used (White 1980).

IV. Data from National Household Surveys on Drug Use and Health

We use pooled, annual, cross-sectional 1994–2002 data from the National Survey on Drug Use and Health (NSDUH), known as the National Household Survey on Drug Abuse (NHSDA) through 2001. The NHSDA/NSDUH is apt for this study because it is designed to produce substance use incidence and prevalence estimates for the general U.S. civilian, non-institutionalized population aged 12 and older, including residents of non-institutional group quarters such as group homes, shelters, and rooming houses. The survey includes questions from

the Diagnostic and Statistical Manual (DSM) of Mental Disorders that allow diagnostic criteria to be applied to identify symptoms of dependence or abuse of alcohol and various illegal and prescription drugs. Respondents are also asked about substance abuse treatment history, personal and family income sources and amounts, employment, health care access and coverage, and criminal record. Completion rates are consistently close to 80 percent.

The sample design changed somewhat during the analysis period. The 1994–1998 surveys used a multistage area probability sample design involving five selection stages: 115 primary sampling units (e.g. metropolitan areas, counties, groups of counties, and independent cities) selected to represent the total U.S. population, blocks or block groups, housing units or group quarters, age-group-smoking classes within sampled listing units, and eligible individuals within sampled age-group-smoking classes. Blacks, Hispanics and cigarette smokers were over-sampled, the latter to increase the precision of drug use estimates. Information was collected using personal interviews and self-enumerated answer sheets (for drug use questions) and samples averaged about 24,000 respondents per year.

Starting in 1999, eight large states contribute about 3,600 respondents each while remaining states yields about 900 respondents each. The sample is then stratified by field interviewer regions and areas consisting of adjacent census blocks, which form the primary sampling units within which dwelling units are selected by systematic sampling. Roughly equal numbers of people age 12-17, 18-25, and 26 and older are sampled, and as before smokers, blacks, and Hispanics were over-sampled. Computer-assisted personal interviews and audio computer-assisted self-interviews are used to collect information. Starting in 2002, each respondent who completed an interview was given $30.

Analysis Samples: As described above, implementation of the DDD method requires that we

identify individuals who show evidence of having a recent, disabling substance problem. We consider two alternate ways of defining such a group. Our primary group of substance abusers – which we call the "broad" substance abuser group - includes individuals with at least one of the following: (1) heavy current use of alcohol and/or illicit drugs; or (2) 3 or more symptoms of an SUD for at least one single substance; or (3) receipt of any substance abuse treatment, including participation in a self-help group (e.g., Alcoholics Anonymous), in the past year. Heavy current use is defined as 3 or more alcohol binges (5+ drinks in one occasion) in the past 30 days; and/or 6 or more occasions of marijuana use in the past 12 months; and/or 3 or more occasions of other illicit drug use in the past 12 months. The substance disorder symptoms included in the NHSDA/NSDUH surveys are based on DSM criteria and are applied to alcohol, illegal drugs (including marijuana, hallucinogens, inhalants, cocaine/crack and heroin) and non-prescribed use of prescription drugs (including pain killers, tranquilizers, stimulants and sedatives).[5]

[5] Starting in 1999, the survey includes the following six criteria, pertaining to the previous 12 month period: 1) having a period of at least a month during which a great deal of time was spent getting the drug, using the drug, or getting over its effects; 2) building up a tolerance for the drug so that the same amount of the drug had less effect than before; 3) using the drug much more often or in larger amounts than intended; 4) use of the drug often preventing going to work or school, taking care of children, or engaging in recreational activities; 5) the drug causing emotional or psychological problems (such as feeling uninterested, depressed, suspicious or paranoid, or having strange ideas) or health problems (such as liver or stomach disease, pancreatitis, feet tingling, numbness, memory problems, an accidental overdose, a persistent cough, a seizure or fit, hepatitis, or abscesses); and 6) wanting or trying to stop or cut down use of the drug but being unable to do so. The survey items used to capture symptoms of substance

The advantage of using the broad substance abuser group is that it is inclusive enough to capture many different forms and stages of substance problems (e.g., alcohol abuse), but it is still stringent enough to be limited to individuals who are likely to have a recent disabling problem. Notably, this measure includes individuals with recent heavy use of substances, as well as those experiencing symptoms of disorder. Estimates generated from 2002 NSDUH data set indicate that the correlation coefficient between this heavy substance use measure and substance dependence is about 0.38 for women and about 0.41 for men, and these correlations are significant at the .001 level.

A possible disadvantage of the broad group, however, is that it may include some respondents who use substances heavily, but are not experiencing disabling problems associated with use. For this reason, we also consider an alternate, more stringently defined substance abuser group. Our "narrow" group is limited to individuals reporting either (1) 3 or more symptoms of substance disorder for at least one single substance; (2) receipt of any substance abuse treatment, including self-help groups, in the past year, or both (1) and (2). The narrow group includes only respondents who appear to meet DSM criteria for substance dependence. In our sample, as in reports on the pre-legislation DA&A recipients, the majority qualify due to abuse of alcohol, as opposed to illicit drugs. When we categorize individuals into those suffering from at least 3 symptoms of an SUD or recipients of substance abuse treatment in the last year,

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disorder changed somewhat and generally became more extensive between 1994 and 2002. For example, between 1994 and 1995, some items capturing health problems and interference with daily activities were added and changed and in 1999, questions related to withdrawal symptoms were added for substances in which withdrawal symptoms can occur. We dealt with this issue by using the six symptoms listed above which are fairly consistent across all years.

62% qualify due to alcohol only, versus 21% who qualify due to symptoms relating to an illicit drug only, and 18% relating to abuse or dependence of more than one substance. We note that our rate of symptoms relating to alcohol-only is higher than the rate of DA&A recipients for alcohol only, but this could relate to the fact that individuals were more likely to report alcohol use and symptoms v. illicit drug abuse and symptoms in the NSDA/NSDUH.

To maximize power, our primary approach is to estimate models using a combined gender sample. To reflect the disadvantaged nature of the SSI eligible population, we limit the entire analysis sample to respondents with less than 16 years of education. Together, including all surveys from the 1994B NHSDA through the 2002 NSDUH, 325,710 individuals were sampled, including 232,490 individuals aged 18-64. Of these respondents, 156,042 had less than 16 years education and available information on all dependent variables and covariates.

Outcome variables: We consider four types of outcomes: (1) receipt of public assistance; (2) labor market outcomes; (3) health insurance and health services utilization; and (4) arrests.

NHSDA/NSDUH respondents were asked whether they had received any SSI payments or assistance from welfare in the last calendar year. To ensure that respondents understood that they were being questioned about payments from SSI versus another public support program, the interviewer specified that "…federal SSI checks are either automatically deposited in the bank or mailed to arrive on the first of every month. If mailed, they are sent in a blue envelope." Similarly, the question regarding welfare participation specified that the respondent should not include SSI. From these questions, we create binary indicators of SSI receipt and welfare receipt. Note that the NHSDA/NSDUH question regarding receipt of federal disability benefits specifically mentions SSI, and does not include payments from the DI program.

We also create binary variables indicating whether the respondent is 1) employed now; 2)

in the labor force; and 3) currently disabled.[6] To measure health insurance coverage, we create three variables that indicate whether the respondent is currently covered by: (1) any type of health insurance; (2) Medicaid; and (3) any type of private insurance.[7] The interviewers specified that Medicaid is a public assistance program that pays for medical care, and also provided the name of the Medicaid program in the respondent's state. To measure utilization of health services, we create indicators of any visit to the emergency department in the past 12 months, and any inpatient hospital stay in the past 12 months. We also create two indicators specific to mental health services, given the likely high level of co-morbidity between mental and substance conditions in our sample. These indicators are the following: any psychiatric outpatient services in the past 12 months, and any psychiatric inpatient services in the past 12 months. The psychiatric inpatient question changed substantially after 1998, so we include only the first post-period (1997-98) in these models. Finally, our measure of involvement with the criminal justice system is a binary indicator of whether or not the respondent reports being arrested and booked for any crime in the past 12 months. Arrest questions began in the 1995 survey; thus, we exclude 1994 in arrest models.

To gauge the broad impact of the SSI policy change, we examine multiple outcomes that are likely to be related to this change and that are relevant to individuals with substance

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[6] From 1994-1998, the employment and labor force variables are based on questions about the respondent's "present work status." In 1999-2002, these variables are based on a question regarding work status "in the past week."

[7] The wording of the health insurance shifted slightly in the 1999 survey. In 1994 to 1998, respondents were asked whether they were covered by health insurance in the "current month." In later surveys, respondents were asked whether they had "current" health insurance.

problems. We acknowledge, however, that because the number of outcomes that we study is high, our inference must rely on a higher level of statistical significance. We have not explicitly corrected for these multiple comparisons here because standard corrections such as those suggested by Bonferroni will tend to understate the true significance of the results.

V. Results

Table 1 presents the propensity score-weighted characteristics of the broad treatment group, (heavy substance users, treatment in the past 12 months, or those with 3 or more symptoms of substance abuse or dependence) compared to the control group by time period. By construction, all of the observed demographic characteristics included in the propensity score model are perfectly balanced during the pre-period, so we show the outcome variables only. It is notable that measures of public program use and labor market outcomes are relatively similar between substance abusers and others during the pre-period. SSI use is 4.1 percent among substance abusers compared with 3.2 percent in the comparison group. The percent employed is very similar among substance abusers (74.5 percent) and the comparison group (73.3 percent). Reported rates of disability are also similar, 3.9 v. 4.5 percent. Health care utilization is similar across groups with the exception of psychiatric services, though substance abuse treatment would be an example of a psychiatric service, so the substance use in the treatment group could affect that. Arrest rates are considerably higher among substance abusers, 6.2 percent versus the comparison group, 1.6 percent. This is not surprising given high arrest rates estimated among the DA&A population. However, results on arrests should be interpreted with caution given the differences across groups. Otherwise, respondents in our broad treatment and comparison groups are highly likely to report exactly 12 years of education (45 percent) most are unmarried

(52 percent) and the plurality (36 percent) of respondents are aged 35-49 (Appendix Table 1).

Figure 2 complement these numbers by showing the rate of SSI receipt, employment, and arrest separately for our broad treatment and control groups among individuals with low and elevated predicted SSI receipt (greater than .05 which is over twice the population average rate of SSI receipt), weighted by the propensity-score based weights to balance the treatment and control groups during the pre-period. During the pre-period, within the elevated Pr(SSI) group, use of SSI is higher and both labor force participation and employment are lower among the treatment groups. By 1997-98, the pattern above was largely reversed. Figure 6 shows a dramatic rise in arrest rates, particularly among our treatment groups in the long-run. Changes in survey design from a paper and pencil survey to computer assisted survey instruments for "sensitive survey questions" in 1999 can partly explain the trend towards higher arrest rates in the final period, but this trend towards more arrests began in 1997-98.

Table 2 shows the estimated effects of the SSI policy change on public program use and health insurance outcomes. As expected, SSI use drops disproportionately among individuals predicted to use SSI among both our treatment groups during the 1997-98 period. Among the broad treatment group, SSI use drops by 70 percent in the short-run, a number that closely mirrors estimates from the literature suggesting that two thirds of former DA&A recipients had not re-qualified for SSI by December of 1997 (Stapleton et al. 1998). Among the narrow treatment group, the drop is about 64 percent, though it is less precisely measured. The drop in SSI receipt attenuates in the long run, however. Comparing SSI use in 1999-2002 relative to 1994-96, reductions in SSI use are no longer significant, and they are about half as big in magnitude as the 1997-98 effects. The logit models presented in the appendix document similar trends, though the implied magnitudes of SSI reductions are smaller and slightly more precisely

estimated in the logit models.

Table 2 confirms patterns documented elsewhere, that welfare use declined disproportionately among substance abusers, but only after 1998 (Pollack and Reuter 2006). There were no significant effects of rising SSI propensity and being in the treatment group in 1997-98 on welfare receipt, but by 1999-2002 compared with the pre-period, welfare use was 30 percent lower in the broad treatment group, and 42 percent lower in the narrow treatment group.

One aspect of welfare reform and reforms to SSI/DI that has received much attention is the potential loss of health insurance benefits as individuals exit public programs for work. This could happen explicitly, as in the case of terminating SSI benefits and linked eligibility for Medicaid, or indirectly, as a result of exceeding income limits for Medicaid eligibility once employed, but with no alternative source of coverage through an employer or privately. The rate of health insurance, and the form of insurance among those covered did not change disproportionately for individuals who were heavy substance users or those with symptoms of dependence. Medicaid coverage did not fall significantly for this group, although the sign of the effect was negative and large. In the longer run, Medicaid receipt differed little for substance abusers with a high propensity to use SSI compared with others, and there were no long-run changes in the share of individuals reporting any health insurance coverage. Logit models on health insurance, and welfare yield similar results, suggesting that among substance abusers likely to use SSI, welfare use fell, but only after 1998, and health insurance status did not change significantly. Summarizing the results on public program use, rates of receiving public assistance fell for each program examined, but SSI use rebounded somewhat in 1999-2002, particularly in the narrow treatment group. The reduction in welfare use in the latter post- period may have been partially offset by an increase in SSI use, as has been suggested elsewhere

(Schmidt and Sevak 2004).

Table 3 presents estimates of the labor market effects of terminating disability insurance for substance abusers. The SSI policy change is associated with a substantial short-term increase in employment for individuals regardless of the treatment group used. For the broad treatment group, Table 3 suggests the policy change is associated with a one for one increase in employment as the probability of SSI use under pre-period rules rises. Among the broad treatment group, the employment effects persist, although they are much smaller in magnitude by 1999-2002. The linear probability models in table 3 suggest that long-run employment was 80 percent higher among likely SSI recipients in our treatment group compared with 1994-96.

The effect is somewhat smaller among the narrow treatment group, about 60 percent in the long run. Consistent with these patterns, both broad and narrow treatment groups showed a disproportionate rise in labor force participation. The magnitude of these effects closely mirrors that of the employment effects, implying that the elevated employment represents individuals moving into the labor force rather than simply more successful job search during the booming economy of the late 1990s. Another interesting and intuitive result in table 3 is that the employment effects seem to stem from full-time work in the short run in both treatment groups, but part-time employment plays a more significant role among individuals in the narrow treatment group. Over the long run, 1999-2002, the full-time employment effect in the narrow group reversed. In other words, individuals with symptoms of substance abuse or dependence entered the labor force and successfully obtained full-time and part-time employment in the short-run, but only part-time employment persisted, and the long-run effects were not as large for this group compared with the broad treatment group. Given that individuals in this group report current or recent symptoms of abuse or dependence, it is not surprising that this group could

have had difficulty maintaining full-time employment.

Logit models confirm that employment and labor force participation rose substantially and disproportionately among likely SSI users. Furthermore, logit models suggest that the likelihood that respondents reported being "too disabled to work" fell markedly and significantly, 5 to 6 percentage points for each 10 percentage point rise in the probability of SSI use, in 1997-98. Unlike in the linear probability models, estimates of these effects are significant at conventional levels based on the bootstrapped variance estimates associated with the logit models. Finally, the importance of part-time work among the narrow treatment group also holds in the logit models.

The large magnitude and persistence of the employment effects among the broad treatment group are somewhat surprising given more moderate effects on SSI receipt that attenuate over time. As discussed before, the policy change affected both SSI and DI programs, and we are unable to directly observe DI receipt. It is possible that the employment effects reflect changes in the population of substance abusers likely to use DI (which one would expect to be correlated with the propensity to use SSI). This could potentially explain the large employment effects in the face of a more moderate effect on SSI. An alternative explanation is that other time varying factors disproportionately affected the target group, high propensity SSI users who also report heavy substance use. Although multiple factors could create the pattern work and SSI receipt, it seems likely that individuals were influenced by the termination of disability, since these policies signaled a change in attitude towards individuals with SUDs.

Because we documented no disproportionate change in health insurance status or source of coverage, it is unclear whether one should expect large changes in health care utilization. In Table 4, the likelihood of reporting an ER visit, a hospital or psychiatric admission all increased

in 1997-2002, though only psychiatric inpatient visits were significantly higher among substance abusers with high propensity to use SSI in the linear probability models. The logit models suggest that hospitalizations increased among the broad treatment group. On balance, given little power to detect differences in these relatively rare events, we have no evidence to suggest systematic change in utilization after termination of SSI benefits for substance abuse.

Finally, in Table 4 we present estimates for the arrest outcome. This analysis must be interpreted with caution because the assumption that arrests should move together in the treatment and control groups is strong given the difference in levels of arrest rates before the policy change between these groups. Because we lack arrest data before 1995, we were unable to compare trends in arrests during the pre-period. With this caveat in mind, however, the results in Table 4 suggest a disproportionate rise in arrests in both treatment groups in the longer term, with the effect being most immediate and largest among the narrow treatment group. These arrest effects are statistically significant only in the logit-based estimates presented in the appendix. In sum, our results suggest that termination of SSI benefits for substance abuse may increase criminal activity among individuals with symptoms of substance abuse or dependence.

VI. Conclusions

In the mid-1990s, multiple reforms aimed to increase self-sufficiency among potential recipients of public income support. In this spirit, the 1997 termination of federal disability benefits for individuals with disabling substance disorders was no different in that it eliminated potential disincentives to work related to the availability of disability benefits. There has been concern, however, that these benefits are the only legal means by which addicted persons can obtain income to satisfy their basic needs and access health and treatment services. Recent

studies have failed to uncover changes in substance abusers' outcomes in response to the policy shift, but these studies primarily analyze small, geographically constrained samples, and available evidence to date lacks a control group against which to compare outcomes for individuals affected by the termination of benefits. Furthermore, former literature in this area offers limited evidence on variables measuring self-sufficiency such as labor market participation and employment, and no existing study attempted to assess effects of the policy change beyond 24 months. In this paper, we examined trends in labor market outcomes, public program participation, health care access and utilization, and self-reported arrests among substance dependent individuals using a much larger national sample over a period that began three years before the policy change and extends until 2001 (survey year 2002), allowing for the estimation of both short-run and longer term effects.

Our findings confirm earlier work showing few measurable negative consequences of the policy change, though with one possible, and potentially important exception. The use of SSI fell disproportionately for the targeted groups, and there appears to have been no replacement of SSI benefits with welfare benefits, which is not surprising given coincident reforms to welfare. Labor force participation and employment rose substantially faster for our treatment groups compared with others, and in some specifications respondents appear to have reduced their likelihood of viewing themselves as "too disabled to work." The persistent rise in employment among heavy substance users was fueled by increases in full-time work. Among individuals with three or more symptoms of substance abuse or dependence, the labor market effects differed. Individuals with symptoms of abuse or dependence increased employment in 1996 and 1997, but they were less likely to hold onto employment gains, and two years after the termination of SSI benefits for DA&A, disproportionate employment gains among these

individuals were due to part-time, rather than full time work. Health insurance status and source of coverage did not change measurably. Consistent with this, the analyses of health care utilization yield few, if any, systematic effects related to the policy change, although the pattern of results suggest that psychiatric outpatient visits fell following the change while ER visits and hospital admissions rose. Most disturbingly, arrests rose disproportionately among likely SSI users with symptoms of drug dependence.

During the 1990s, the labor market reached unprecedented levels of employment, especially among those with relatively few skills or labor market experience. In such an era, we document that employment rose, and remained elevated among a group of individuals likely to use SSI and with moderate to heavy substance use. Among our narrow treatment group, individuals with symptoms of dependence or in substance abuse treatment, there was a relatively short-lived employment response. That is, both full-time and part-time employment increased in the months following the policy change, but by 1999-2002, employment fell for these individuals, and only part-time employment remained high. The one other study of terminated benefits suggests that most employment among former DA&A recipients failed to meet levels of Substantial Gainful Activity established for federal disability programs (Campbell et al. 2003).

Our results suggest that disability programs have provided income support to individuals unable to maintain attachment to the labor force even during a period of strong economic conditions. Fortunately, the expected disastrous consequences of lost of health insurance, and heavier utilization of inpatient or emergency medical services did not occur or were too small to observe. Of course, we were unable to assess other important indicators of well-being, like income, changes in drug use, changes in the rate or severity of mental health disorders, or changes in eviction and/or homelessness, among this population. Aside from productivity losses,

the largest cost of substance abuse stems from crime. We find that arrests increased following termination of disability for individuals with heavy substance use or symptoms of SUDs.

Because many individuals with a history of substance use did increase self-sufficiency through increased employment and a reduction in the use of public programs, the results shown here suggest, on balance, that the individuals targeted by the termination of SSI/DI benefits for substance abuse, had relatively few disasters. The short term responses, however, suggest an interesting set of new questions. In particular, with these data we do not know how individuals entered the labor market, and what services, if any, aided them in their search for employment. Additional information on the nature of jobs held by individuals during this unique period, and the circumstances under which individuals left, are crucial for understanding the complete ramifications of policy changes aiming to increase self-sufficiency entirely through incentives.

Questions regarding the employment histories of individuals with SUDs are increasing in importance as the recent reauthorization of Temporary Assistance to Needy Families has further limited states' ability to address the needs of welfare recipients with substance abuse issues. States are no longer at liberty to exempt women from work requirements while they obtain treatment for substance use or mental health disorders. Furthermore, the new regulations require more hours of work, and higher participation requirements. It is difficult to forecast the effects of these limitations, but they signal a new era under which vulnerable, low-skilled individuals with SUDs increasingly have fewer sources of support as they seek the very self-sufficiency that reforms of the mid-1990s aimed to achieve.

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Table 1: Weighted Outcomes by Treatment Group and Time Period

	Pre-Period: 1994-1996		1997-1998		1999-2002	
	Treatment	**Control**	**Treatment**	**Control**	**Treatment**	**Control**
Dependent variables:	n =5,756	n =21,950	n =4,517	n =20,140	n =20,123	n =83,555
Labor market & program participation:						
SSI	0.041	0.032	0.025	0.039	0.042	0.035
Welfare	0.050	0.051	0.045	0.034	0.030	0.028
Employed	0.745	0.733	0.743	0.757	0.762	0.796
Full-time	0.596	0.596	0.617	0.633	0.588	0.643
Part-time	0.145	0.128	0.118	0.117	0.131	0.115
In Labor Force	0.816	0.785	0.813	0.806	0.815	0.830
Disabled	0.039	0.044	0.034	0.051	0.050	0.047
Health Insurance:						
Any health insurance	0.739	0.766	0.745	0.788	0.752	0.795
Private health insurance	0.641	0.655	0.652	0.693	0.638	0.691
Medicaid	0.075	0.083	0.070	0.071	0.087	0.076
Health care utilization:						
ER visit	0.211	0.200	0.225	0.198	0.351	0.299
Inpatient stay	0.079	0.091	0.086	0.086	0.105	0.088
Inpatient psychiatric stay	0.010	0.005	0.013	0.005	--	--
Psychiatric outpatient services	0.069	0.052	0.080	0.051	0.116	0.059
Arrested: past year	0.061	0.017	0.069	0.016	0.128	0.045

[A] Sample limited to respondents with <16 years of education weighted by both sampling and propensity-score based weights. [B] (Broad) Treatment = respondents reporting moderate/heavy substance use, and/or 3+ symptoms of disorder, and/or substance treatment (including self-help groups) in last year. See text for details.[C] Information on mental health inpatient stays is not comparable during 1999-2002 period. [D] Employment is based on "present work status." [E] Health insurance refers to coverage at survey (or in survey month).

Table 2: Public Program Participation and Health Insurance

	SSI		Welfare		Medicaid		Private Insurance		Any HI	
	Broad	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**
SSI propensity	1.09***	1.09***	.253***	.246***	1.09***	1.07***	-1.85***	-1.76***	-.374***	-.337**
(1994-1996)	(.105)	(.121)	(.063)	(.065)	(.104)	(.120)	(.119)	(.130)	(.113)	(.128)
After 1996	.015*	.024	-.015**	-.019**	-.029***	-.034***	.090***	.068***	.053***	.036
	(.009)	(.015)	(.006)	(.009)	(.009)	(.013)	(.019)	(.025)	(.017)	(.023)
After 1998	.020***	.026***	.009	.007	.010*	.012	-.007	-.006	.004	.014
	(.007)	(.011)	(.006)	(.008)	(.009)	(.013)	(.019)	(.024)	(.016)	(.021)
SSI propensity*	.158	.115	-.163**	-.160**	.143	.239	-.015	-.105	.217	.324*
After 1996	(.192)	(.233)	(.074)	(.077)	(.190)	(.234)	(.200)	(.231)	(.164)	(.187)
SSI propensity*	-.356**	-.364*	.086*	.093*	-.127	-.294	.059	-.041	-.109	-.317**
After 1998	(.171)	(.209)	(.049)	(.054)	(.172)	(.213)	(.176)	(.206)	(.133)	(.155)
Treatment Group	-.002	.005	-.007	-.005	-.019***	-.016*	-.010	-.020	-.028**	-.034**
	(.007)	(.011)	(.005)	(.008)	(.007)	(.009)	(.013)	(.018)	(.013)	(.017)
Tx*After 1996	.001	-.007	.011	.014	.021**	.024*	-.039*	-.033	-.012	.003
	(.009)	(.015)	(.007)	(.012)	(.010)	(.014)	(.022)	(.031)	(.020)	(.029)
Tx*After 1998	.004	.013	.0002	.001	-.001	.005	.004	-.012	.0002	-.012
	(.007)	(.011)	(.006)	(.010)	(.009)	(.013)	(.019)	(.028)	(.017)	(.025)
SSI propensity * Tx	.328	.492	.210	.219	.408*	.437	-.350	-.446	-.102	-.085
	(.266)	(.326)	(.138)	(.190)	(.248)	(.302)	(.252)	(.347)	(.233)	(.301)
SSI propensity *	-.700**	-.642	.008	.044	-.480	-.494	.518	.204	-.029	-.281
Tx*After 1996	(.347)	(.451)	(.196)	(.276)	(.366)	(.470)	(.405)	(.496)	(.343)	(.464)
SSI propensity *	.417	.180	-.300**	-.416**	.210	.326	-.181	.279	.071	.550
Tx*After 1998	(.257)	(.345)	(.150)	(.214)	(.304)	(.399)	(.349)	(.399)	(.284)	(.387)

NOTES: Based on linear probability models using propensity score-based weights to match observed characteristics between treatment and control groups. Robust, Huber-White corrected SEs shown in ( ) correct for correlation within sampling units. *=p-value<=.10, **<=.05, ***<=.01. Models also include controls for race-gender specific national unemployment rate, national arrest rate per 100,000 and national incarceration rate per 100,000 (coefficients not shown). Analyses are based on a sample size of 156,041.

Table 3: Labor Market Outcomes

Outcome:	Employed		Full-time		Part-time		Disabled		In labor force	
	Broad	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	Broad	**Narrow**	**Broad**	**Narrow**
SSI propensity	-1.68***	-1.58***	-1.48***	-1.32***	-.244***	-.282***	1.51***	1.40***	-1.58***	-1.43***
(1994-1996)	(.125)	(.140)	(.122)	(.137)	(.064)	(.067)	(.121)	(.132)	(.129)	(.142)
After 1996	.032*	.042*	.032	.049*	-.003	-.005	.001	.006	.020	.019
	(.017)	(.024)	(.021)	(.028)	(.013)	(.015)	(.008)	(.011)	(.016)	(.023)
After 1998	.023	.019	.010	.006	-.024**	-.016	.015**	.026**	.011	-.004
	(.015)	(.020)	(.019)	(.024)	(.011)	(.014)	(.008)	(.011)	(.014)	(.019)
SSI propensity*	-.414**	-.404*	-.312*	-.289	-.069	-.103	.486***	.526**	-.416**	-.470**
After 1996	(.184)	(.225)	(.178)	(.223)	(.090)	(.090)	(.197)	(.231)	(.192)	(.231)
SSI propensity*	.431***	.561***	.462***	.540***	-.051	.021	-.330**	-.427**	.388**	.516***
After 1998	(.153)	(.192)	(.146)	(.191)	(.071)	(.076)	(.170)	(.202)	(.159)	(.198)
Treatment Group	.033***	.009	.014	-.013	.022**	.023*	-.008	-.009	.046***	.019
	(.013)	(.019)	(.015)	(.021)	(.010)	(.014)	(.006)	(.007)	(.012)	(.018)
Tx*After 1996	-.063***	-.083***	-.045	-.064**	-.021	-.022	.006	.004	-.059***	-.084***
	(.021)	(.029)	(.024)	(.032)	(.015)	(.020)	(.009)	(.011)	(.019)	(.027)
Tx*After 1998	-.005	.027	-.022	.023	.013	.001	.008	.023**	-.007	.036
	(.018)	(.024)	(.020)	(.027)	(.012)	(.016)	(.008)	(.011)	(.016)	(.023)
SSI propensity *	-.624***	-.786***	-.340	-.372	-.237*	-.361***	.121	.272	-.424*	-.706**
Tx	(.228)	(.280)	(.218)	(.280)	(.128)	(.121)	(.271)	(.324)	(.254)	(.311)
SSI propensity *	1.07***	1.34***	.770**	.906**	.240	.370*	-.584	-.650	1.04***	1.69***
Tx*After 1996	(.356)	(.460)	(.345)	(.461)	(.171)	(.193)	(.386)	(.465)	(.375)	(.464)
SSI propensity *	-.259	-.732*	-.287	-.785**	.058	.089	.192	.234	-.288	-.994***
Tx*After 1998	(.312)	(.409)	(.304)	(.405)	(.145)	(.193)	(.308)	(.372)	(.313)	(.390)

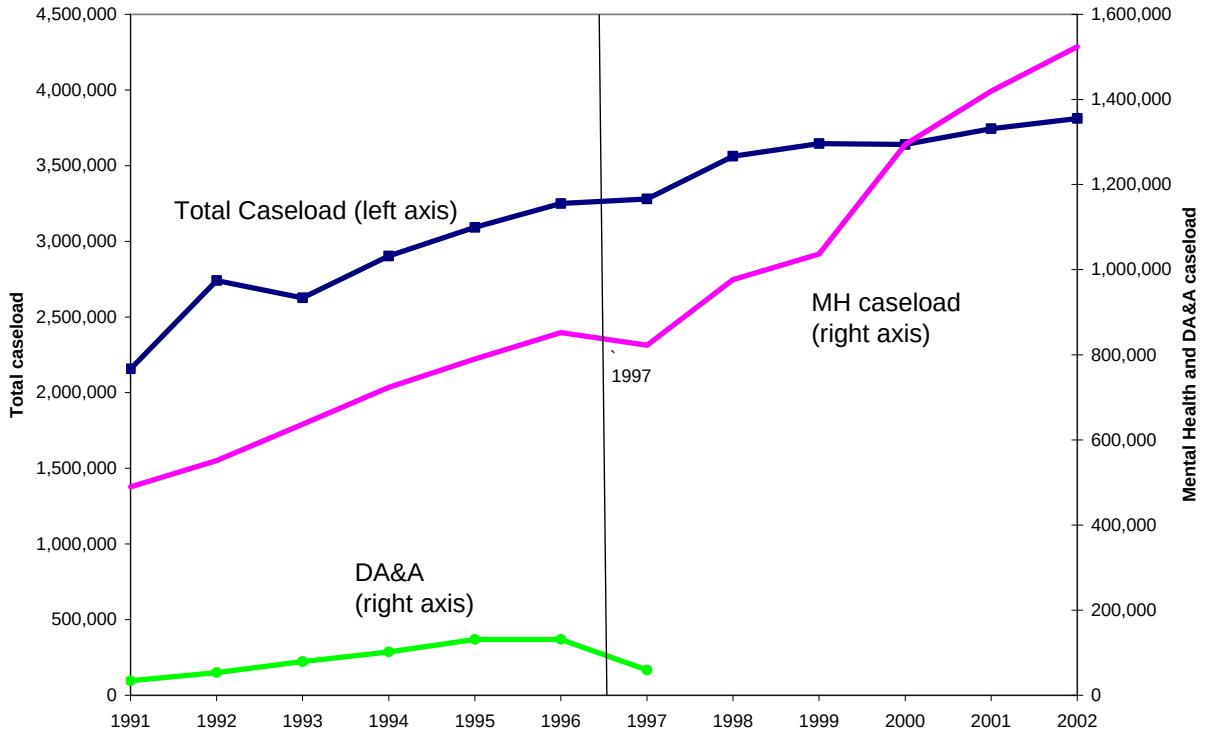
NOTES: Based on linear probability models using propensity score-based weights to match the distribution of observed characteristics of treatment and control groups. Robust, Huber-White corrected SEs shown in ( ) correct for correlation within sampling units. *=p-value<=.10, **<=.05, ***<=.01. Models also include controls for race-gender specific national unemployment rate, national arrest rate per 100,000 and national incarceration rate per 100,000 (coefficients not shown). Analyses are based on a sample size of 156,041.

Table 4: Health Care Utilization and Arrests in Last Year

Outcome:	ER visit		Hospital admission		Psychiatric visit		Psych. admission[A]		Arrested/booked	
	Broad	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**
SSI propensity	.822***	.669***	.750***	.661***	.454***	.491***	.087***	089***	.024	.025
(1994-1996)	(.110)	(.124)	(.100)	(.105)	(.080)	(.104)	(.028)	(.028)	(.049)	(.060)
After 1996	-.047***	-.040**	.003	.012	-.017**	-.025**	.0004	.001	-.003	-.004
	(.015)	(.020)	(.010)	(.013)	(.009)	(.013)	(.003)	(.006)	(.010)	(.019)
After 1998	.033**	.031*	.025***	.030**	-.017*	-.019			.050***	.066***
	(.015)	(.019)	(.009)	(.013)	(.010)	(.015)			(.008)	(.014)
SSI propensity*	-.021	-.090	-.070	.010	.111	.108	-.001	.076	-.003	-.050
After 1996	(.170)	(.199)	(.143)	(.158)	(.132)	(.157)	(.047)	(.068)	(.062)	(.074)
SSI propensity*	.171	-.239	-.014	-.113	-.042*	-.108			.135	.178
After 1998	(.149)	(.175)	(.118)	(.133)	(.118)	(.131)			(.055)	(.066)
Treatment Group	.022**	.034**	-.008	.001	.012*	.028***	.002	.005	.041***	.083***
	(.011)	(.016)	(.008)	(.011)	(.007)	(.011)	(.002)	(.004)	(.006)	(.013)
Tx*After 1996	.004	-.002	-.001	-.001	.015	.015	-.007*	-.006	.007	.002
	(.018)	(.026)	(.013)	(.016)	(.014)	(.021)	(.004)	(.007)	(.010)	(.020)
Tx*After 1998	.025	.022	.009	.030**	.022*	.044**			.021**	.021
	(.016)	(.023)	(.011)	(.015)	(.013)	(.020)			(.009)	(.017)
SSI propensity *	-.210	.024	-.062	.107	.179	.403	.103	.189	.091	-.068
Tx	(.231)	(.306)	(.204)	(.275)	(.170)	(.261)	(.091)	(.158)	(.115)	(.150)
SSI propensity *	.217	.175	.342	.213	-.155	-.335	.296*	.200	.036	.220
Tx*After 1996	(.358)	(.455)	(.306)	(.417)	(.261)	(.360)	(.180)	(.278)	(.160)	(.246)
SSI propensity*	-.115	-.063	.125	.174	.107	.129			.242	.151
Tx*After 1998	(.314)	(.385)	(.271)	(.362)	(.236)	(.298)			(.153)	(.245)

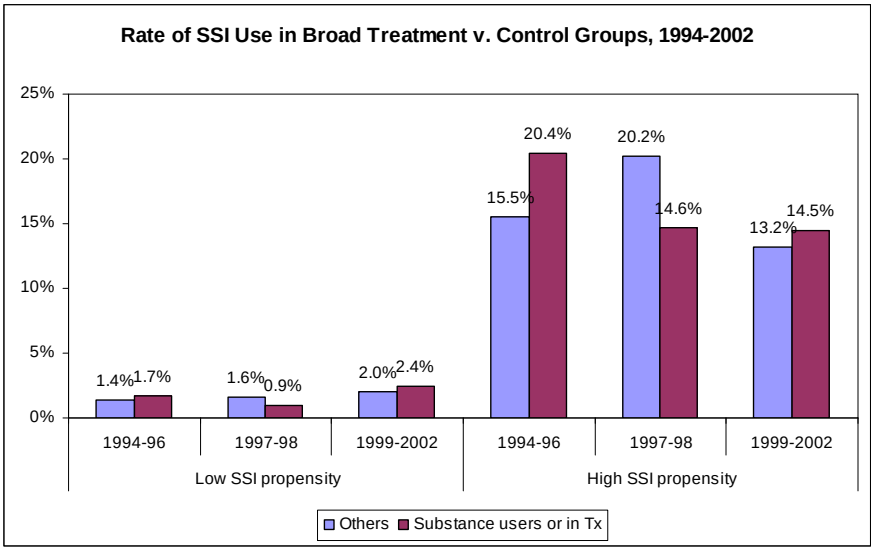
NOTES: Based on linear probability models using propensity score-based weights. Robust, Huber-White corrected SEs shown in ( ) correct for correlation within sampling units. *=p-value<=.10, **<=.05, ***<=.01. Models also include controls for race-gender specific national unemployment rate, national arrest rate per 100,000 and national incarceration rate per 100,000 (coefficients not shown). Analyses are based on a sample size of 156,041.

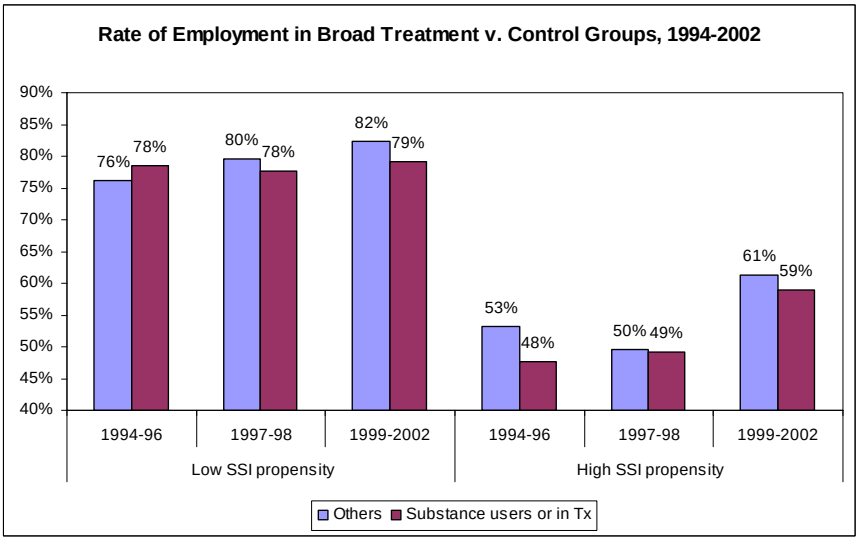
Figure 1: Trends in SSA caseload among Mental Health and DA&A Population

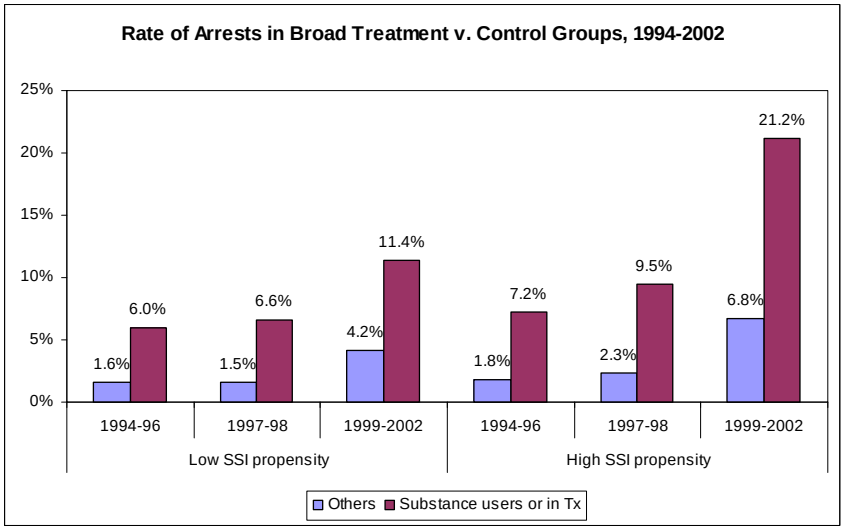


Sources for Figure 1: for Total and Mental Health Caseload: Social Security Administration, Social Security Bulletin, Annual Statistical Supplement, 1991-2004 (Washington: Government Printing Office, 1991-2004). For DA&A population: Barber, S.L. Supplemental Security Income for whom Alcoholism and Drug Addiction Provisions Apply (DA&A Recipients). (Washington: Office of Program Benefits Policy, 1996) and for 1996, 1997 DA&A figures :Schmidt, Lucie. 2004. *Effects of Welfare Reform on the Supplemental Security Income (SSI) Program*, Policy Brief #4, National Poverty Center (available at http://www.npc.umich.edu/publications/policy_briefs/brief4/brief4.pdf)

Figure 2: Trends in SSI, Employment and Arrests, by Year







Rates apply to population 18-64 with <16 years of education. High SSI propensity = Pr(SSI) >.05.

Appendix Table 1: Weighted Summary Statistics

		Pre-Period: 1994-1996		1997-1998		1999-2002	
Covariates:		**Substance abuser** n =5,756	**Others** n =21,950	**Substance abuser** n =4,517	**Others** n =20,140	**Substance abuser** n =20,123	**Others** n =83,555
Covariates:							
Sex	Female	0.415	0.415	0.397	0.394	0.403	0.405
Age	18-21	0.152	0.152	0.178	0.154	0.199	0.159
	22-23	0.061	0.061	0.066	0.063	0.068	0.060
	24-25	0.056	0.056	0.050	0.054	0.050	0.049
	26-29	0.104	0.104	0.103	0.101	0.097	0.086
	30-34	0.142	0.142	0.135	0.130	0.114	0.114
	35-49	0.360	0.360	0.337	0.361	0.339	0.382
	50-64	0.125	0.125	0.131	0.136	0.133	0.151
Education	5 or fewer	0.011	0.011	0.017	0.009	0.015	0.012
	6	0.009	0.009	0.010	0.009	0.012	0.008
	7	0.011	0.011	0.007	0.008	0.007	0.007
	8	0.017	0.017	0.013	0.014	0.020	0.014
	9	0.033	0.033	0.026	0.028	0.037	0.029
	10	0.055	0.055	0.046	0.048	0.050	0.046
	11	0.078	0.078	0.090	0.081	0.088	0.082
	12	0.452	0.452	0.457	0.432	0.406	0.459
	13	0.097	0.097	0.118	0.111	0.131	0.112
	14-15	0.236	0.236	0.216	0.257	0.234	0.233
Marital Status	Married	0.486	0.486	0.459	0.475	0.386	0.447
	Widowed	0.015	0.015	0.016	0.019	0.018	0.019
	Divorced	0.159	0.159	0.137	0.145	0.161	0.177
	Never Married	0.340	0.340	0.388	0.361	0.435	0.356
Race/Ethnicity	White	0.723	0.723	0.728	0.709	0.694	0.693
	Black	0.134	0.134	0.125	0.133	0.134	0.132
	Latino	0.114	0.114	0.115	0.121	0.134	0.129
	Other	0.029	0.029	0.032	0.037	0.038	0.044
Language	Spanish speaker	0.036	0.036	0.034	0.039	0.044	0.042
Military Veteran		0.155	0.155	0.152	0.150	0.108	0.139
Health	Poor	0.024	0.024	0.017	0.022	0.024	0.022

	Fair	0.081	0.081	0.082	0.085	0.100	0.083
	Good	0.275	0.275	0.245	0.271	0.288	0.303
	Very Good	0.340	0.340	0.364	0.345	0.363	0.365
	Excellent	0.278	0.278	0.292	0.276	0.225	0.226
Prior substance use	Lifetime use of illicit drugs	0.604	0.604	0.668	0.628	0.781	0.687
	Alcohol before age 15	0.254	0.254	0.253	0.248	0.342	0.279
	Marijuana before age 17	0.271	0.271	0.308	0.290	0.406	0.319
	Smoke daily	0.641	0.641	0.673	0.637	0.587	0.633
Household Size	1	0.086	0.086	0.102	0.092	0.115	0.087
	2	0.291	0.291	0.292	0.295	0.257	0.287
	3	0.223	0.223	0.228	0.210	0.229	0.224
	4	0.219	0.219	0.192	0.215	0.215	0.220
	5	0.111	0.111	0.093	0.105	0.102	0.110
	6 or More	0.071	0.071	0.079	0.071	0.082	0.071
Urbanicity	In MSA >1 million	0.421	0.421	0.390	0.402	0.446	0.418
	In MSA <1 million	0.344	0.344	0.367	0.377	0.339	0.355
	Not in MSA	0.235	0.235	0.243	0.222	0.214	0.227

NOTES: [A] Sample limited to respondents with <16 years of education & percentages reflect both sampling and propensity-score based weights to balance treatment and control group in pre-period. [B] Broad Treatment = respondents reporting moderate/heavy substance use, 3+ symptoms of disorder, and/or substance treatment (including self-help groups) in last year. "moderate to heavy use"= any of the following: a) an individual "binges" (5 or more alcoholic beverages are consumed at one sitting) >=3 times/month; b) an individual uses marijuana >=6 times/ year; or c) an individual uses any illicit drug other than marijuana >=3 times/year. The "3 or more symptoms" = person has >= 3 of 6 symptoms for at least one single substance, based on questions regarding 10 substances including alcohol. [C] Information on mental health inpatient stays is not available in a usable format for the second post period.[D] Employment measure is based on "present work status." [E] Health insurance variables refer to coverage at time of the survey (or in the survey month).

Appendix Table 2: Public Program Participation and Health Insurance, Logit Models

	SSI		Welfare		Medicaid		Private Insurance		Any HI	
	Broad	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**
SSI propensity	14.13***	14.02***	3.41***	3.20***	8.30***	8.03***	-8.30***	-7.88***	-1.88**	-1.59***
(1994-1996)	(.708)	(.825)	(.692)	(.699)	(.545)	(.629)	(.648)	(.723)	(.536)	(.572)
After 1996	.626**	.771*	-.394***	-.475***	-.489***	-.496***	.423***	.293***	.296***	.184
	(.314)	(.437)	(.148)	(.180)	(.151)	(.185)	(.094)	(.113)	(.097)	(.117)
After 1998	.481***	.571**	.246	.153	.165	.158	-.041	-.028	.025	.073
	(.195)	(.271)	(.175)	(.224)	(.129)	(.169)	(.091)	(.110)	(.094)	(.109)
SSI propensity*	-.286	-.587	-1.15	-1.08	1.73*	2.09*	.185	.729	.970	1.50*
After 1996	(1.14)	(1.45)	(1.04)	(1.07)	(.994)	(1.20)	(1.03)	(1.18)	(.834)	(.913)
SSI propensity*	-1.96**	-1.89	2.25**	2.24**	-.955	-1.77	.378	-.133	-.623	-1.61**
After 1998	(.987)	(1.29)	(.931)	(.988)	(.902)	(1.09)	(.865)	(.999)	(.718)	(.792)
Treatment Group	.225	.630*	-.103	-.041	-.259***	-.156	-.032	-.060	-.161**	-.175**
	(.275)	(.354)	(.100)	(.144)	(.097)	(.118)	(.068)	(.094)	(.068)	(.084)
Tx*After 1996	-.789**	-1.01*	.301*	.367*	.265*	.266	-.208**	-.167	-.074	.012
	(.386)	(.557)	(.172)	(.240)	(.155)	(.210)	(.108)	(.149)	(.109)	(.141)
Tx*After 1998	.723***	.836*	-.047	.003	.067	.166	.024	-.057	-.005	-.065
	(.293)	(.455)	(.170)	(.234)	(.137)	(.194)	(.092)	(.127)	(.094)	(.125)
SSI propensity *	.688	-.026	2.20*	2.06	2.87**	2.59*	-2.21	-3.17	-.312	-.248
Tx	(1.66)	(1.90)	(1.25)	(1.69)	(1.30)	(1.45)	(1.66)	(2.69)	(1.05)	(1.30)
SSI propensity *	-.593	.720	.155	.399	-326*	-3.15	3.12	1.69	-.157	-1.38
Tx*After 1996	(2.38)	(3.14)	(2.08)	(2.72)	(1.91)	(2.33)	(2.29)	(3.35)	(1.61)	(2.05)
SSI propensity *	-.562	-2.26	-4.21**	-5.93**	.551	.709	-.955	1.47	.408	2.62
Tx*After 1998	(1.93)	(2.73)	(2.01)	(2.67)	(1.57)	(2.02)	(1.74)	(2.22)	(1.37)	(1.76)
DDD 1997-1998	-.051***	-.049*	.0002	.002	-.043	-.043	.064	.024	-.002	-.026
v. 1994-1996[B]	(.019)	(.029)	(.011)	(.018)	(.036)	(.049)	(.043)	(.060)	(.031)	(.044)
DDD 1999-2002	-.015	-.027	-.022**	-.031*	-.029	-.020	.040	.059	.005	.026
v. 1994-1996[B]	(.016)	(.027)	(.010)	(.017)	(.028)	(.035)	(.034)	(.050)	(.025)	(.033)
DDD 1999-2002	.036**	.022	-.023**	-.033**	.014	.023	-.023	.035	.007	.053
v. 1997-1998[B]	(.019)	(.022)	(.011)	(.016)	(.025)	(.035)	(.038)	(.042)	(.026)	(.037)

NOTES: Based on logit models using propensity score-weighted data to match characteristics in the pre-period. Robust, Huber-White corrected SEs shown in ( ) correct for correlation within sampling units. *=p-value<=.10, **<=.05, ***<=.01. Logit models also include controls for race-gender specific national unemployment rate, national arrest rate per 100,000 and national incarceration rate per 100,000 (coefficients not shown). Analyses are based on a sample size of 156,041. [B] Because the coefficients in the model are on a nonlinear scale, for ease of interpretation, we present the calculation of the triple differences in predicted probabilities obtained based on coefficients from the logit models above: DDD = {(Post-Treatment rate– Pre-Treatment rate) – (Post-Control rate– Pre-Control rate)}$_{\text{SSI propensity=.15}}$. - {(Post-Treatment rate– Pre-Treatment rate) – (Post-Control rate– Pre-Control rate)}$_{\text{SSI propensity=.05}}$. We chose .15 and .05 as cutoff points for calculating DDD estimates since these span values of the Pr(SSI) that we observe in our data. Standard errors on DDD estimates are bootstrapped using 100 replications.

Appendix Table 3: Labor Market Outcomes, Logit Models

Outcome:	Employed		Full-time		Part-time		Disabled		In labor force	
	Broad	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**
SSI propensity	-7.39***	-6.96***	-6.18***	-5.48***	-2.73***	-.3.05***	1.51***	14.48***	-7.53***	-7.15***
(1994-1996)	(.555)	(.612)	(.573)	(619)	(.869)	(.918)	(.121)	(.786)	(.562)	(.624)
After 1996	.187*	.242*	.141	.213*	-.029	-.030	.001	.355	.134	.136
	(.101)	(.133)	(.094)	(.119)	(.117)	(.133)	(.008)	(.322)	(.111)	(.157)
After 1998	.168*	.142	.041	.023	-.222**	-.152	.015**	.449	.112	.008
	(.091)	(.114)	(.083)	(.106)	(.105)	(.134)	(.008)	(.280)	(.097)	(.130)
SSI propensity*	-2.13***	-2.06**	-1.46*	-1.31	-1.41	-2.00	.486***	2.07	-2.15***	-2.39**
After 1996	(.852)	(1.01)	(.863)	(1.03)	(1.40)	(.141)	(.197)	(1.32)	(.846)	(.130)
SSI propensity*	1.52**	2.10**	2.13***	2.37***	-.893	.680	-.330**	-1.47	1.35*	2.02**
After 1998	(.717)	(.873)	(.708)	(.884)	(1.29)	(.134)	(.170)	(1.14)	(.708)	(.871)
Treatment Group	.184**	.044	.065	-.045	.196**	.249**	-.008	-.153	.310***	.099
	(.076)	(.106)	(.066)	(.089)	(.089)	(.117)	(.006)	(.241)	(.088)	(.124)
Tx*After 1996	-.351***	-.449***	-.206**	-.288**	-.189	-.248	.006	-.802*	-.375***	-.517***
	(.120)	(.153)	(.105)	(.136)	(.134)	(.179)	(.009)	(.482)	(.137)	(.180)
Tx*After 1998	-.059	.104	-.092	.105	.095	.000	.008	1.52***	-.077	.186
	(.102)	(.125)	(.089)	(.115)	(.113)	(.153)	(.008)	(.450)	(.115)	(.147)
SSI propensity *	-.313***	-3.76***	-1.69	-1.86	-2.99	-5.99***	.121	1.95	-2.42**	-3.18**
Tx	(1.16)	(1.50)	(1.12)	(1.43)	(2.15)	(2.35)	(.271)	(1.57)	(1.15)	(1.45)
SSI propensity *	5.36***	6.63***	3.70**	4.24**	3.07	6.27*	-.584	.171	5.14***	8.04***
Tx*After 1996	(1.69)	(2.19)	(1.66)	(2.15)	(2.93)	(3.53)	(.386)	(2.66)	(1.69)	(2.13)
SSI propensity *	-.982	-3.02*	-1.36	-3.49**	1.58	1.28	.192	-5.06**	-1.01	-4.33***
Tx*After 1998	(1.39)	(1.79)	(1.37)	(1.80)	(2.44)	(3.13)	(.308)	(2.39)	(1.39)	(1.76)
DDD 1997-1998	.129***	.158***	.089**	.101*	.027	.043*	-.057**	-.062*	.112***	.176***
v. 1994-1996[B]	(.046)	(.060)	(.041)	(.056)	(.023)	(.022)	(.026)	(.034)	(.045)	(.056)
DDD 1999-2002	.097***	.078**	.055*	.012	.033	.051***	-.027	-.018	.077**	.076*
v. 1994-1996[B]	(.032)	(.038)	(.030)	(.038)	(.021)	(.018)	(.019)	(.026)	(.033)	(.044)
DDD 1999-2002	-.032	-.080*	-.035	-.088*	.006	.008	.030	.044*	-.035	-.100**
v. 1997-1998[B]	(.034)	(.047)	(.037)	(.048)	(.014)	(.021)	(.024)	(.026)	(.036)	(.051)

NOTES: Based on logit models using propensity score-weighted data to match characteristics in the pre-period. Robust, Huber-White corrected SEs shown in ( ) correct for correlation within sampling units. *=p-value<=.10, **<=.05, ***<=.01. Logit models also include controls for race-gender specific national unemployment rate, national arrest rate per 100,000 and national incarceration rate per 100,000 (coefficients not shown). Analyses are based on a sample size of 156,041. [B] Because the coefficients in the model are on a nonlinear scale, for ease of interpretation, we present the calculation of the triple differences in predicted probabilities obtained based on coefficients from the logit models above: DDD = {(Post-Treatment rate– Pre-Treatment rate) – (Post-Control rate– Pre-Control rate)}$_{\text{SSI propensity=.15}}$. - {(Post-Treatment rate– Pre-Treatment rate) – (Post-Control rate– Pre-Control rate)}$_{\text{SSI propensity=.05}}$. We chose .15 and .05 as cutoff points for calculating DDD estimates since these span values of the Pr(SSI) that we observe in our data. Standard errors on DDD estimates are bootstrapped using 100 replications.

Appendix Table 4: Health Care Utilization and Arrests in Last Year, Logit Models

Outcome:	ER visit		Hospital admission		Psychiatric visit		Psych. admission[A]		Arrested/booked	
	Broad	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**	**Broad**	**Narrow**
SSI propensity	4.32***	.669***	6.29***	5.97***	6.32***	6.09***	9.20***	8.79	2.27	1.69
(1994-1996)	(.501)	(.124)	(.605)	(.678)	(.793)	(.939)	(1.63)	(1.66)	(1.89)	(1.54)
After 1996	-.274***	-.040**	.005	.123	-.274*	-.359**	-.016	.210	-.073	-.005
	(.091)	(.020)	(.122)	(.169)	(.160)	(.183)	(.452)	(.546)	(.311)	(.336)
After 1998	.225***	.031*	.314***	.339**	-.200	-.165			1.43***	1.20***
	(.080)	(.019)	(.119)	(.159)	(.175)	(.215)			(.179)	(.190)
SSI propensity*	-.084	-.090	-.328	.085	1.04	1.16	-.154	2.23	-.282	-1.71
After 1996	(.783)	(.199)	(.901)	(1.03)	(1.19)	(.1.33)	(2.62)	(2.54)	(2.45)	(2.10)
SSI propensity*	-.0001	.239	-.151	-.712	-1.04	-1.48			.890	2.03
After 1998	(.672)	(.175)	(.766)	(.880)	(1.00)	(1.06)			(1.67)	(1.54)
Treatment Group	.134**	.034**	-.137	.034	.287**	.534**	.787***	1.25***	1.39***	1.53***
	(.068)	(.016)	(.109)	(.145)	(.124)	(.145)	(.314)	(.330)	(.151)	(.160)
Tx*After 1996	.035	-.002	-.328	.020	.263	.207	-.334	-.315	.122	-.033
	(.108)	(.026)	(.901)	(.214)	(.222)	(.265)	(.524)	(.533)	(.220)	(.236)
Tx*After 1998	.071	.022	-.151	.406**	.216	.354			-.392**	-.402**
	(.091)	(.023)	(.766)	(.179)	(.193)	(.234)			(.171)	(.184)
SSI propensity *	-1.08	.024	.118	.551	.796	1.28	.529	.158	-.752	-2.23
Tx	(1.08)	(.306)	(1.34)	(1.67)	(1.41)	(1.71)	(2.66)	(2.98)	(2.28)	(1.96)
SSI propensity *	.857	.175	1.54	.940	-2.19	-2.90	4.80	1.39	.545	3.23
Tx*After 1996	(1.63)	(.455)	(1.88)	(2.38)	(2.16)	(2.49)	(3.73)	(4.18)	(2.98)	(2.90)
SSI propensity*	-.391	-.063	.173	-.155	.416	.471			.825	-.562
Tx*After 1998	(1.37)	(.385)	(1.54)	(1.94)	(1.83)	(2.03)			(2.09)	(2.31)
DDD 1997-1998	.023	.018	.025	.017	-.021	-.042	.010	.009	.003	.019
v. 1994-1996[B]	(.039)	(.053)	(.029)	(.039)	(.030)	(.039)	(.012)	(.022)	(.011)	(.023)
DDD 1999-2002	.014	.014	.044**	.042	-.010	-.028			.029**	.036*
v. 1994-1996[B]	(.028)	(.037)	(.020)	(.027)	(.027)	(.037)			(.014)	(.021)
DDD 1999-2002	-.009	-.004	.018	.025	.011	.014			.026**	.018
v. 1997-1998[B]	(.033)	(.041)	(.025)	(.036)	(.019)	(.030)			(.013)	(.023)

NOTES: Based on logit models using propensity score-weighted data to match characteristics in the pre-period. Robust, Huber-White corrected SEs shown in ( ) correct for correlation within sampling units. *=p-value<=.10, **<=.05, ***<=.01. Logit models also include controls for race-gender specific national unemployment rate, national arrest rate per 100,000 and national incarceration rate per 100,000 (coefficients not shown). Analyses are based on a sample size of 156,041. [B] Because the coefficients in the model are on a nonlinear scale, for ease of interpretation, we present the calculation of the triple differences in predicted probabilities obtained based on coefficients from the logit models above: DDD = {(Post-Treatment rate– Pre-Treatment rate) – (Post-Control rate– Pre-Control rate)}$_{\text{SSI propensity=.15}}$. - {(Post-Treatment rate– Pre-Treatment rate) – (Post-Control rate– Pre-Control rate)}$_{\text{SSI propensity=.05}}$. We chose .15 and .05 as cutoff points for calculating DDD estimates since these span values of the Pr(SSI) that we observe in our data. Standard errors on DDD estimates are bootstrapped using 100 replications.