

# The SSDI “Benefit Offset” Experiment: Landing the Pilots

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October 22, 2010

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## Abstract

The Social Security Disability Insurance (SSDI) program includes transitory incentives that promote the return to work by beneficiaries, but after these incentives end, beneficiaries lose their entire disability benefit for the first dollar earned above a monthly level termed “Substantial Gainful Activity” (SGA). It is widely presumed that this “cliff” restrains employment among beneficiaries. Congress included requirements within the Ticket to Work and Work Incentives Improvement Act of 1999 for a demonstration project that examines the costs and benefits of replacing (“offsetting”) the SGA cliff with a more gradual reduction in benefits: the SSDI benefit is to be reduced by \$1 for every \$2 earnings beyond that threshold. Skeptics questioned the ability of the Social Security Administration to administer such an experiment, and even SSA stakeholders acknowledged that conducting the experiment within the context of regular SSA operations posed substantial design and management challenges. SSA launched a pilot project intended to provide experience with new benefit offset administration procedures as well as with the conduct of a randomized trial of the new program. Volunteers for the experiment were recruited from beneficiaries likely to be in the process of returning to work in order to ensure that the pilot would produce experience administering the new program. However, this target group also poses the most serious cost issues because many of them would return to work even in the absence of the incentive and thus would receive a partial benefit under the new program instead of no benefit under the current program. This paper reviews challenges faced in implementing the pilot and the results. The pilot “delivered”: Random assignment was completed successfully and the revised benefits computation system was successfully performed for the treatment group, albeit with some informative missteps. The offset produced an increase in the prevalence of earnings above SGA. However, it also increased benefit payments because the savings generated from increases in employment above SGA were smaller than the costs of partial benefit payments to those who would have worked above SGA under the existing program rules. The outcomes underscore the need for a larger experiment that includes a representative sample of all SSDI beneficiaries. Such an experiment, called the Benefit Offset National Demonstration (BOND), will begin in 2011.

## **The SSDI “Benefit Offset” Experiment: Landing the Pilots**

Robert Weathers, Jeffrey Hemmeter, and Michael Wiseman\*

### **Introduction**

The Social Security Disability Insurance (SSDI) program provides cash payments and other benefits to people who meet the program’s definition of disability and insured status requirements (see SSA 2009 for a compact description). Section 223(d)(1) of the Social Security Act defines “disability” as “inability to engage in any substantial gainful activity by reason of any medically determinable physical or mental impairment which can be expected to result in death or which has lasted or can be expected to last for a continuous period of not less than 12 months.” Benefits are paid only to persons who have worked long enough and recently enough to be fully and currently insured and thus obtain Social Security entitlement (for themselves or their dependents).<sup>1</sup> Payments are not means-tested and, for adults, can continue until the individual reaches full retirement age, when the disability benefit is automatically converted to retirement benefits.<sup>2</sup> However, for those able to return to work, the purpose of the program is not to support early retirement. When President Eisenhower signed the legislation creating the SSDI program in 1954, he said “We will . . . endeavor to administer the disability [program] efficiently and effectively, [and] . . . to help rehabilitate the disabled so that they may return to useful employment” (quoted in SSA 2009, 1). This paper describes the design, implementation, and outcome of a pilot experiment that tests whether removing financial disincentives within the SSDI program will increase the likelihood that beneficiaries will return to employment and increase expected earnings for those who work. The innovation is termed an “offset” by the Social Security Administration (SSA) because the incentive partially counteracts the elimination of benefits required by current law for beneficiaries who earn above a threshold amount. The pilot is important because it is a precursor to a major social experiment, the Benefit Offset National Demonstration, or BOND.

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<sup>1</sup> One exception to the insurance status provision is for those applicants who receive benefits based on statutory blindness.

<sup>2</sup> In addition to a monthly cash benefit, beneficiaries are also covered by Medicare after a 24-month waiting period.

## Some Background

SSA uses the term “Substantial Gainful Activity” (SGA) to describe a level of work activity and earnings. Work is “substantial” if it involves doing significant physical or mental activities (not necessarily on a full-time basis), or a combination of both. “Gainful” work activity is work performed for pay or profit (or work of a nature generally performed for pay or profit) or work intended for profit, whether or not a profit is realized.<sup>3</sup> For 2010, the SGA amount is \$1,000 per month;<sup>4</sup> the threshold is indexed to the Consumer Price Index. Over time, numerous changes have occurred in the legislation and regulations to facilitate and create incentives for recipients who medically recover or find ways to adapt to their disabling health condition to “return to useful employment” and earn above SGA. In practice, very few do. Less than five percent of beneficiaries leave the rolls within ten years of program entry, although a somewhat larger fraction of beneficiaries report some employment from time to time (Muller 1992, Liu and Stapleton 2010).

Low termination rates, lower ages at disability onset, and a growing number of claims have combined to produce rapid growth in the SSDI caseload. The solid line in Figure 1 shows trends in SSDI caseload growth over 40+ years normalized by the working age population that is insured for disability benefits. The broken line shows the trend when computed holding the gender and age distribution of the eligible population constant at the mix observed in 2000. Three things stand out. First, since the mid-1980s the prevalence of SSDI receipt has doubled from about 2.5 percent to 5.0 percent in 2009. Second, the growth cannot be fully explained by changes in the gender-age composition of the insured population. The gender-age adjusted data show a 50 percent increase in the prevalence of SSDI receipt from 1989 through 2009. Third, there was one interval, beginning in 1980, in which the caseload declined, but growth subsequently revived.<sup>5</sup> This growth is projected to exhaust the SSDI trust fund by 2018 (SSA 2010, 11). This trend has motivated a search for policy innovations with promise for reducing costs.

Figure SSDIRATE about here

**Figure 1:** SSDI Recipients as a Percentage of Insured Population Age 18-64, 1967 – 2009

<sup>3</sup> There may be instances where a self-employed individual has \$0 earnings but is found to be performing SGA.

<sup>4</sup> The SGA standard is higher for the blind (\$1,640 in 2010). For all SSDI recipients, some disability-related expenses are deducted from earnings before comparison to the standard.

<sup>5</sup> The 1980 episode is interesting because it illustrates an unsuccessful approach to slowing caseload expansion. Congress enacted legislation in 1980 that limited disability benefit levels, instituted a system of review of initial disability decisions and continuing disability status, established a periodic review of continuing disability requirements, enhanced the rehabilitation and work incentive provisions of the law, and withheld payment of benefits to incarcerated felons. Implementation of the review provisions led to closure of a substantial number of cases and critical public reaction. Congress responded in 1982 and 1984 by relaxing eligibility standards and requirements for continuing disability reviews in ways that, in the judgment of many, led to the resumption of caseload growth evident in the figure (cf. Autor and Duggan 2003, Autor and Duggan 2006, Duggan and Imberman 2008, Singleton 2009).

## **The Ticket and the Mandate**

Confronted with continuing caseload growth, Congress passed the Ticket to Work and Work Incentives Improvement Act in 1999. The purpose of this ambitious legislation was to improve the disability program's work incentives by giving beneficiaries greater choice in seeking rehabilitation and employment services, extending access to Medicare for those who became employed, providing improved employment-oriented counseling, and facilitating return to benefits for former recipients unable to maintain employment. The Ticket legislation also mandated experimentation with a completely different approach to work incentives. Whereas under existing law, beneficiaries who had completed a nine-month Trial Work Period (TWP) and were earning more than the SGA threshold lost all benefits, the Ticket legislation called for an exploration of an alternative policy of graduated benefit decline—a “benefit offset” in SSA parlance.

The legislation was explicit about the policy to be investigated, specifying the benefit reduction rate to be trialed (“\$1 for each \$2 of the beneficiary's earnings that is above a level to be determined by the Commissioner”), scale ambition (“sufficient to adequately evaluate the appropriateness of national implementation of such a program”), and the outcome to be assessed (“reductions in Federal expenditures that may result from the permanent implementation of such a program”).<sup>6</sup>

## **The Two Unknowns**

This mandate posed substantial challenges to the Social Security Administration. One was parametric: At what level of earnings should the benefit offset begin? Another concerned methodology: It was clear that credible estimates of the consequences for federal expenditures would require evaluation by randomized trial. The Social Security Administration had little institutional experience with randomized control trials and no experience with projects of the size likely required for projecting the impact of national implementation. The reference to “reductions in Federal expenditures that may result from the permanent implementation of such a program” seemed to allude to an outcome that economic theory judged questionable (Hoynes and Moffitt 1999) and the agency's own actuaries believed unlikely.<sup>7</sup>

Some authorities doubted that the agency could implement and administer such a program. In 1996, the National Academy of Social Insurance (NASI) released a report discussing the possibility of introducing a work incentive similar to that contained in the subsequent Ticket to Work legislation. After citing the actuaries' estimates of the “significant cost of the proposal,” the Panel report cited “grave doubts about whether SSA has the methods and capacity to administer a monthly, quarterly, or even annual benefit offset for DI in a way that would act as a work incentive” (Mashaw and Reno 1996, 162). A footnote clarified the meaning of “administer” as including “explaining how the offset works to interested beneficiaries,

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<sup>6</sup> Ticket to Work and Work Incentives Improvement Act of 1999, Public Law 106-170, 106th Congress, codified at U.S. Code 42 (1999), §434.

<sup>7</sup> Social Security Administration, Office of the Actuary, memorandum, July 13, 1994. Cited in Mashaw and Reno (1996), page 162, note 34.

processing reports of earnings received by beneficiaries timely and adjusting benefits promptly” (Ibid, note 37).

The implication was that any demonstration would confront both a behavioral unknown and a capability unknown. The behavioral unknown was that a benefit offset would affect the behavior of SSDI beneficiaries in such a way that, on balance, employment would be enhanced and costs would be reduced when compared to outcomes without the innovation. But testing the behavioral unknown required first confronting the NASI Panel’s reservations about SSA capacity and demonstrating that, indeed, the agency could administer such an incentive. If SSA could neither administer the benefit offset nor implement a classical experiment (the capacity unknown), there would be no point to attempting the full-scale demonstration.

### **This Paper**

SSA addressed the capacity issues by fielding a pilot demonstration to test administrative procedures for incorporating the benefit offset incentive in SSDI operation and for studying the consequences for beneficiary behavior. This paper reviews the set-up of the pilot demonstration, the issues addressed, and the outcomes. While the focus of the pilot demonstration was on administration of the incentive, the study was conducted using random assignment and produced estimates of effects for a select group of beneficiaries. In our conclusions, we comment on how the pilot provided the groundwork for the larger multi-site Benefit Offset National Demonstration (BOND), which is scheduled to begin enrolling participants in 2011.

In the next section, we review how SSDI works and what standard economic analysis suggests will be the consequences of introduction of a benefit offset. We then review the issues that arose in implementing the offset in context of the current SSDI administrative set-up and the pilot implementation. This is followed with a review of impacts as revealed by the differences between labor supply outcomes and benefit costs for the offset treatment groups and the corresponding control group. We close the paper with a review of lessons drawn from the pilots for the BOND.

### **The Issues: The Incentives**

To gain a better understanding of the issues addressed by the benefit offset initiative and the associated administrative challenges, it is essential to understand how SSDI works.

#### *SSDI and the Journey to Work*

The SSDI program is designed to provide benefits to individuals who, after a specified minimum history of gainful employment in a covered occupation, experience a change in health that either is expected to lead to death or, if not death, to prevent employment with earnings above the SGA standard for at least 12 months. Virtually all SSDI applicants are unemployed at the time SSDI benefits are awarded. The program contains incentives that are designed to help beneficiaries return to employment at or above SGA if they are capable of doing so. These incentives include: the Ticket to Work program, the Trial Work Period, the Extended Period of Eligibility (which includes a “Grace Period”), and the Extended Medicare Period. Figure 2 summarizes the provisions as they are encountered along a hypothetical “Journey to Work.”

Figure JOURNEY about here

**Figure 2:** The SSDI Journey to Work

An SSDI beneficiary can begin work and retain benefits as long as no substantial medical improvement occurs in his/her qualifying disability. Earnings below the SGA threshold have no effect on benefits. The Journey to Work can begin either with a job or by taking advantage of the Ticket to Work (TTW) program. The TTW is a voucher that a beneficiary may use to obtain employment services, vocational rehabilitation services, or other services that he or she sees as facilitating employment. Public and private organizations, referred to as Employment Networks (ENs), may accept a beneficiary's Ticket and provide the beneficiary with services. An EN receives payments from SSA once they help a beneficiary achieve either employment-related milestones or sustain earnings above the SGA standard. However most beneficiaries who begin work do not take advantage of Ticket services (Stapleton et al 2010).

Whether employment is begun independently or with Ticket assistance, once earnings rise above a certain cash threshold beneficiaries begin what is called a Trial Work Period (TWP). In 2010, work with a value of at least \$720 a month counted as a TWP month. Nine months of TWP are permitted with no effect on benefits, even if earnings exceed the SGA standard. The TWP months may be spread out over a long period, and the TWP continues until a beneficiary accumulates nine TWP months within a rolling 60-month period.

Beneficiaries who complete the ninth TWP month enter the Extended Period of Eligibility (EPE). The first 36 months of the EPE is a re-entitlement period, which allows a beneficiary to remain connected to the SSDI program while earning more than SGA. (Within SSA, earning above the SGA amount is commonly termed “performing SGA.”)<sup>8</sup> The first month following the ninth TWP month that a person performs SGA and the two months following that month—are referred to as the “Grace Period,” and during this interval the beneficiary continues to receive his full benefit even while performing SGA. In each month after the Grace Period, through the first 36 months of the EPE, the beneficiary does not receive a benefit check when performing SGA but regains his full benefit when his monthly earnings fall below the SGA threshold.<sup>9</sup> After the first 36 months of the EPE, the beneficiary does not become automatically re-entitled to benefits when he performs SGA one month, but falls below SGA earnings the next month. However, if he never performs SGA again, he can remain in the EPE through the remainder of his tenure on SSDI.

The reduction of benefits to zero if earnings rise above SGA is called the “cliff.” In June 2010, the average SSDI benefit was \$1,065; the SGA threshold was \$1,000. As a result, for persons earning the mean amount, the first dollar that the average beneficiary earns above the SGA threshold results in a \$1,065 loss—the entire benefit—in monthly income. Not surprisingly, observers and recipients alike see this loss as a substantial barrier to the journey to work.

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<sup>8</sup> At the end of the TWP, a work continuing disability review (CDR) is conducted to determine whether a beneficiary's work amounts to SGA. The first month of the EPE immediately follows the completion of the 9<sup>th</sup> trial work month, whether the individual is currently working or not. In practice, the work CDR may not be completed for several months after the TWP has ended.

<sup>9</sup> During the EPE, the beneficiary must inform SSA that his monthly earnings are below SGA to receive his benefit.

Moreover, once the EPE is completed, a month with earnings above SGA leads to termination of cash benefit entitlement. However, beneficiaries are allowed to maintain Medicare benefits for an additional 57 months following EPE completion. Yet, even this does not exhaust all entitlement. Another provision, called “expedited reinstatement,” allows a person who has had benefits terminated to return to the program without going through the usual disability application process.<sup>10</sup>

*The Ambiguous Consequences of the Benefit Offset*

Total loss of benefits for beneficiaries who have completed their TWP and grace months is in effect a very large “tax” on earnings. The benefit offset is designed to replace the benefit discontinuity at SGA with a gradual reduction in benefits of \$1 for every \$2 earned above the SGA level, thereby removing the discontinuity (and lowering the implicit tax) and increasing the percentage of beneficiaries who chose to perform SGA. While policymakers focus on this aspect of the benefit offset, the benefit offset may have other important effects on labor supply.

We use a static labor supply model to illustrate the potential effects of a benefit offset on labor supply behavior. We focus on the following four outcomes: (1) any employment (defined as some annual earnings reported); (2) employment that exceeds the annualized SGA earnings amount; (3) annual earnings; and (4) SSDI benefits paid. The model is particularly useful for illustrating how the benefit offset has a non-negative impact on the percentage of persons performing SGA, but an indeterminate impact on earnings. The latter result is counterintuitive to some policymakers, and it occurs because some beneficiaries perform SGA under the current rules and have their cash benefits suspended or terminated. Under the benefit offset program, the partial benefit that they obtain subsidizes non-work activities (i.e., “leisure”) both through additional income (income effect) and by imposing a 50 percent tax rate on earnings, which makes non-work activity cheaper than under normal program rules (substitution effect).

Our illustration of the simple static labor supply model follows the presentation used in the welfare literature (Hoynes and Moffitt 1999). The bold lines in Figure 3 show the budget constraint for individuals on SSDI under the current program rules. In the absence of SSDI, an individual earning a wage of \$ $w$  per hour would face budget constraint  $WI$  with slope  $-w$  and would choose income level and number of non-work activity hours that maximizes his utility.

Figure OFFSETRESP about here

**Figure 3: Beneficiary Response to a Benefit Offset**

A beneficiary receives a cash benefit that he can keep as long as his the earnings are below SGA. Once monthly earnings exceed that level (after the TWP and Grace Period), he loses his entire benefit. The cash cliff following SGA hours of work provides a large disincentive to working more than SGA hours because the first dollar earned above the SGA results in the complete loss of cash benefits. As a result, working beneficiaries should tend to cluster at A in Figure 3,

<sup>10</sup> There are also periodic medical CDRs to ensure the continued disability of SSDI beneficiaries. During these reviews, entitlement can only be lost if there is substantial proof that the impairment has medically improved, even if beneficiaries have earnings above SGA



located below the notch at SGA hours of work. Researchers and policymakers refer to this behavior as “parking.” Some beneficiaries work above the SGA level under the current program rules, represented by points B and C.

The benefit offset meliorates this disincentive by reducing the marginal tax rate on earnings above SGA hours of work to 50% until cash benefits are exhausted. Beneficiaries who work above SGA gradually lose \$1 of benefits for every \$2 they earn, up to the break-even earnings level of 2 times the cash benefit amount plus the SGA earnings level. Thus, the new budget constraint after SGA hours of work is represented by the dashed line until the cash benefit is exhausted and the person returns to the original budget constraint represented by the bolded line.

Using this static labor supply model, we expect that the benefit offset will have a non-negative impact on the percentage of beneficiaries performing SGA. Some of those who were constrained by the existing program rules and who located just below the notch (point A) will choose to work additional hours under the benefit offset provisions.

The potentially counterintuitive result that the benefit offset can increase the percentage of beneficiaries performing SGA and not have a positive impact on mean earnings occurs because some beneficiaries work at levels above the SGA level, represented by points B and C. Individuals under the current program rules who are located at point B now find that they keep only \$1 for every \$2 they earn. The benefit offset increases their income because they receive a partial benefit, and it translates into a 50 percent marginal tax rate on their earnings. The income effect from the partial benefit and the substitution effect from the marginal tax rate on their earnings are expected to result in a shift toward less work. Similarly, some beneficiaries located at point C may prefer to reduce work effort under the benefit offset because they are not giving up as much income to do so, compared to the current program.

The static labor supply model also shows how the benefit offset program can increase benefits paid to beneficiaries. Beneficiaries who work above the SGA level, represented by points B and C, do not receive a cash benefit under the current program rules. The introduction of the benefit offset will result in the payment of a partial cash benefit that is represented by the difference in the bold line and the dotted line. The net impact on benefits paid will depend on the amount of additional benefits paid to beneficiaries located at either point B or C, as well as the reduction in benefits made to those who choose to perform SGA as a result of the benefit offset program represented by point A in the diagram.

As is common in analysis of transfer systems, Figure 3 abstracts from important analytical and practical issues. On the analysis side, the simple model covers only one period, yet work strategy involves assessment of benefit from and costs of working both currently and in the future. A multi-period path would seem to be part of the essence of *journey* to work. Practically thinking in multi-period terms points to an important issue for demonstration planning: What should the horizon for the demonstration be? What are the consequences for the external validity and utility of a test of a benefit offset when beneficiaries in the experiment know the incentive will end but a similar time frame is not contemplated for actual implementation?

On the practical side, issues arise with linking payments to work. As depicted, the benefit seems to be calculated and delivered simultaneously with determination of the beneficiary’s labor

supply decision—an unlikely accomplishment for even the most adept bureaucracies. Instead, in practice most income support systems adjust benefits to earning change only with a lag. Such disjunction between work and benefit surely has its own consequences for behavior. For example, a working beneficiary who experiences serial over- or under-payments may become discouraged with the policy. How can the connection be made reliable and intelligible to those on whom the program is tested?

### **The Issues: Delivering the Offset and the Demonstration**

In setting the demonstration up, SSA confronted issues of benefit structure and timing of the “offset opportunity.”

#### *Structuring the Offset*

Early on, the Social Security Administration decided that the major administrative challenge posed by the offset mandate was incorporating it into normal benefit delivery systems. Broadly speaking, two alternatives were considered. One was to imitate Supplemental Security Income (SSI) and to calculate payments retrospectively, based on monthly earnings reports. In abstract, this system corresponds most closely to the monthly calculation implicit in Figure 3, but the SSI system begins benefit reduction at a much lower level of earnings. In practice, payment adjustments occur with a lag, so earnings in one month, when reported, result in benefit adjustment two months later. The SSI system requires considerable administrative oversight, but the costs are accepted because the prevalence of employment is very low (about 6 percent).

The second option was to imitate benefit calculation in the Social Security retirement system. Persons who opt for early retirement are subject to benefit reduction in years prior to reaching their Full Retirement Age (FRA). Benefits are withheld at \$1 for every \$2 of earnings in excess of a substantial exemption amount (\$14,160 in 2010). The benefit adjustment is made based on a prediction made by the beneficiary of the amount of earnings for the year. This prediction may be changed in the course of the year, and if incorrect payments occur, benefits are adjusted in the following year to compensate. Special rules apply for persons who begin retirement benefits or reach full retirement age mid-year. The key features of this system are payments based on an annualized, rather than monthly, income figure and annualized reconciliation.

SSA opted for following the early retirement example and building the benefit offset around annualized earnings and an annualized earnings exclusion amount based on the SGA earnings level. This option takes advantage of pre-existing agency familiarity with retirement earnings contributions and retains differentiation between insurance-based SSDI payment system and the welfare-based SSI procedures. At the same time, the system retained most features of SSDI, including the TWP, for which earnings would be excluded from the annualized computation. Under this system, beneficiaries are required to provide SSA with an earnings estimate for the calendar year when they return to work, and would be allowed to change the estimate as their employment situation changed. SSA determines the total amount to be offset by: (1) using the difference between the beneficiary’s earnings estimate and the earnings exclusion amount; (2) computing an average monthly benefit offset amount for the remainder of the calendar year; and (3) deducting the average monthly benefit offset from the monthly benefit checks over the remainder of the year. At the end of the year, SSA used the Internal Revenue Service (IRS)

master earnings files to identify discrepancies between the amount that should have been offset and the amount that was actually offset. Beneficiaries would receive an additional benefit check if SSA offset too much during the calendar year or owe the difference if SSA offset too little. In Table 1, we provide examples of the benefit offset calculation for four different scenarios in order to illustrate how this system works.

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**Table 1:** Examples of Benefit Calculation in the Benefit Offset Pilot Demonstration

In the first example, Person A completes her trial work period and Grace Period before the beginning of 2010. She estimates that her earnings for 2010 will be \$24,000. In order to determine her offset, her SSA claims representative (or other administrator tasked with this responsibility) takes the \$24,000 annual estimate and subtracts the standard earnings disregard of \$12,000 (the value of SGA times 12 calendar months). This brings her countable earnings for the offset calculation down to \$12,000. The offset is equal to half of her estimated countable earnings, which would be \$6,000 for the year. \$6,000 divided by 12 months means that her SSDI benefit is reduced by \$500 per month. However, at the end of the year, Person A's 2010 IRS records show that she ended up making only \$20,000 for the year. In this case, SSA has to reimburse her the difference between the amount that was deducted and the amount that would have been deducted had the estimate been accurate. Using the same procedure as before, the claims representative calculates that the benefit offset should have been \$4,000, and so, at the end of the year, SSA reimburses Person A the \$2,000 difference in benefit offset amounts.

Person B does not complete his Grace Period until February 2010. He estimates that his earnings for March through the end of the year will be \$20,000. Because Person B will only receive the benefit offset for ten months, the earnings disregard is \$10,000 (ten times SGA). After subtracting the \$10,000 earnings disregard from his \$20,000 estimate and dividing the difference by two, Person B's claims rep determines that his annual deduction should be \$5,000. This amount is divided by ten, because there are only ten months remaining in the year, which means that Person B has a \$500 reduction in benefits per month for the rest of 2010. At the end of the year, Person B's IRS records show that he made \$20,000, as he expected. Therefore, there is no difference between his estimated offset and actual offset based on earnings, and no balance is owed.

Like Person A, Person C completes his Grace Period before the beginning of 2010, but he does not provide his earnings estimate to his SSA claims representative until the end of February 2010. He estimates that for 2010, he will make \$24,000, which means that his annual offset is \$6,000. He receives the entire earnings disregard because, even though he made his estimate late in the year, he will receive the benefit offset for the entire year. However, his annual offset will be divided by ten instead of 12 (for the ten months remaining in the year), reducing Person C's benefits by \$600 per month for March through December.<sup>11</sup> At the end of the year, the IRS

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<sup>11</sup> During BOPD, it was possible to adjust Person C's offset for the ten months remaining by the year because the calculations were performed manually. However, under BOND, which has an automated benefit offset calculation system, Person C's annual offset would be divided by 12, and thus his benefits would be reduced by \$500 per month for March through December. However, he would need to reimburse SSA for the \$500 per month that should have  
*(continued on next page)*

shows that Person C actually made \$25,000, which means that his annual offset should have been \$6,500. In this case, Person C owes SSA the difference of \$500 between the offset amounts, which will be withheld from the next benefit check after his IRS reported earnings are input into the SSA system.

The final example illustrates what happens when a beneficiary's circumstances change mid-year. Person D completes her base period before the beginning of 2010 and gives her claims representative an earnings estimate of \$24,000 for the year. She makes \$2,000 per month through June, but then her health worsens. She submits a new earnings estimate of \$750 per month to her claims rep in June for the period of July through the end of the year. Person D's annual offset is \$6,000 at the beginning of the year, meaning that her benefits are reduced by \$500 per month. However, when she submits her revised estimate, her benefit offset for the entire year is recalculated based on annual earnings of \$16,500 (\$12,000 plus \$4,500). The benefit offset for the year should have been \$2,250, which would have been a monthly reduction of \$187.50. Beginning in July and through the end of the year, \$187.50 will be withheld from her check. However, because SSA has already withheld \$1,875 too much ( $[\$500 - \$187.5] \times 6$  months), Person D is owed a balance. She will be reimbursed the \$1,875 as soon as the estimate change is input (usually in about ten days, but occasionally subject to administrative delays in BOPD practice). At the end of the year, her IRS records show that she did earn a total of \$16,500 for the year, meaning her benefit reduction should have been \$2,250. This is the same as the adjusted amount calculated by SSA and, therefore, no balance is owed.

### *Bounding the Opportunity*

As mentioned above, using time-limited experiments to test the behavioral response to alterations in income support systems is complicated by the possibility of "horizon effects" (Metcalf 1973, Moffitt 2004). People may respond differently to an innovation known to be time limited than to a change expected to last indefinitely. Choice of horizon for the pilot was complicated by the TWP. Unlike the situation in many work-and-benefit-related experiments, SSDI beneficiaries in the pilot treatment group would not gain immediate access to the offset simply by beginning work. Rather, the offset could be utilized only after completion of the nine-month TWP and three-month Grace Period. As a result, in selecting a horizon for the demonstration, it was necessary to recognize that potential participants without post-award work history might take a year or more before they could experience the offset. At the other extreme, the offset would be useless to any person beginning the TWP less than 13 months before the end of the demonstration. Since the focus of the pilot was on gaining experience with delivering the offset, an effort was made both to control costs and to encourage work effort by limiting the window of opportunity for gaining the offset while at the same time promising it for a long time. Treatment group beneficiaries were given at least two years after they were enrolled into the demonstration project to complete a trial work period. If they did not complete a TWP during the period, they were no longer entitled to the benefit offset. If they completed the TWP before December 31, 2008, they were eligible for the benefit offset for a 72-month period following the

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been withheld in January and February. Because this overpayment by SSA was the result of an "incorrect computation" by the beneficiary, the balance would be taken out of his March check, plus the regular \$500 benefit reduction for March. If his full SSDI benefit is less than \$1,500, then he would not receive a check for March and the balance would be taken from his April check.

completion of the ninth TWP month. This combination of TWP opportunity plus benefit offset duration means that pilot project operations will end after the 2014 calendar year.

## **The Pilots**

The focus of the pilots was to assess the feasibility of the administrative structure selected for the benefit offset. This assessment required a sample of beneficiaries likely to use the offset. SSA policy makers decided that such beneficiaries were most likely located among the clients of grantees under the Work Incentives, Planning, and Assistance (WIPA), vocational rehabilitation, and other state-managed service activities. In 2004, SSA contracted with four states—Connecticut, Utah, Vermont, and Wisconsin—in a partnership for operating the pilot demonstration.

### *Recruitment*

The states identified a sample of volunteers for the project from a variety of sources who were then randomly assigned to either a group that received the benefit offset incentive or a control group that did not. The volunteers selected for the project had to meet two eligibility rules established by SSA. First, participants selected for the pilot could not be collecting an SSA disability benefit based on another person's earnings record.<sup>12</sup> Second, participants selected for the project could not be concurrently collecting Supplemental Security Income (SSI) payments. Both rules were designed to simplify the informed consent process that the states used to enroll participants into the project. The second rule removed a group of beneficiaries that is unlikely to gain from a benefit offset because the SSI benefit formula is affected by the SSDI benefit amount.<sup>13</sup>

The state contractors recruited volunteers meeting the eligibility criteria from various state- and local-level programs designed to improve employment opportunities for SSDI beneficiaries. Connecticut recruited beneficiaries from their Medicaid Buy-In program<sup>14</sup> and from WIPA service providers. Utah recruited participants from various sources: their state vocational rehabilitation program, WIPA providers, the Medicaid program, and mental health treatment centers. Vermont recruited beneficiaries from WIPA service providers and from their vocational rehabilitation program. Wisconsin recruited beneficiaries from various community-based agencies and not-for-profit entities that were working with individuals across disability groups and had developed expertise in networking across systems at the local level. Recruitment was

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<sup>12</sup> Individuals can collect disability benefits on their own record, as a Disabled Worker, or on another person's record, as a Disabled Adult Child or a Disabled Widow(er).

<sup>13</sup> Beyond a small set-aside, benefits in SSI are reduced a dollar for every dollar an SSI recipient receives from SSDI. This effectively sterilizes the incentive created by the benefit offset. Since calculation of benefits under SSDI is not affected by SSI status, and SSI benefit calculation readily accommodates variation in SSDI benefits, it was doubtful that whatever might be learned from including concurrent recipients in the pilot would justify the cost. See Livermore and Stapleton (2003) for a good description of how the addition of an SSDI benefit offset has little or no impact on total income for beneficiaries who receive SSI payments. Roughly one million (14 percent) of SSDI disabled workers also receive SSI (SSA 2008).

<sup>14</sup> The Medicaid Buy-In program allows adults with disabilities who are working to obtain or retain Medicaid coverage with earnings higher than allowed under regular Medicaid rules. Participants typically pay premiums based on income.

generally not statewide; concern about operational issues led to focus on particular labor market areas. As a result, references made to the states actually mean those substate areas in which the contracting states chose to operate the program.

### *Random Assignment*

For all locations, the first step in recruitment was to describe to potential volunteers the opportunity and the experimental context. After presentation, potential recruits were invited to participate and sign a consent form. When this step was completed, volunteers were randomly assigned to the treatment or control group after administrators phoned the project office for designation. Both groups received work incentive counseling services that they could use to obtain information on how work behavior would impact SSDI benefits. Enrollment into the project occurred over a 17-month period beginning in August of 2005 and ending in December 2006. This extended period allowed for maintaining balance between recruitment and service delivery. The benefit offset was available for a six-year period that began the month after the individual completed the nine-month trial work period and was paid for the grace period months.

### *Operational Issues*

As expected, various problems were encountered in implementing the pilot.<sup>15</sup> The benefit offset was initially administered manually by SSA employees who were responsible for completing their regular workloads as well as processing the BOPD cases. Staff members were required to switch between applying the rules of the current program and the benefit offset program, and this led to some early delays in administering BOPD payments, errors in the notices sent to BOPD participants, and errors in the payment amounts. SSA recognized the problems relatively early on in the process and set up a special unit to work solely on BOPD cases. However, even with the special unit, this process relied on staff performing the calculations using a worksheet, manually stopping the usual notices that are sent to beneficiaries on how employment affects their benefit amount, manually preparing and sending new notices for the benefit offset, and then inputting the resulting monthly benefit amount into the SSA system used to make benefit payments. All of this was quite cumbersome.

Some of the early problems in administering the \$1 for \$2 offset may have affected the employment behavior of beneficiaries. For example, some members of the treatment group received notices from SSA that did not provide correct information on their benefit amount, and in a few of these cases, several months passed before correct information was transmitted. The notices were eventually fixed, all payment errors were corrected, and procedures were put in place to limit such problems.

Many issues arose in connection with forecasting of earnings estimates. The adjustment process description given to participants may have over-emphasized the importance of precision in making these estimates. As a result, some participants changed estimates on virtually a month-to-month basis. Eventually a system developed where payment adjustments were made quarterly whenever participant forecasts changed by more than \$1,000.

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<sup>15</sup> Far more detail is provided by the rich set of individual site reports. See Chambless et al. (2009) for Utah, Delin et al. (2010) for Wisconsin, Porter et al. (2009) for Vermont, and Porter et al. (2010) for Connecticut.

More serious problems arose because of communication issues within SSA itself. Early in the experiment, SSA staff unaffiliated with the special units set up to handle benefit offset cases manually overrode the benefit offset payment in some cases. This was remedied by incorporating within the payments system a special provision for flagging benefit offset cases and alerting technicians to contact BOPD operations before altering payments.

Developing and fielding a benefit offset computation procedure and uncovering problems with its administration was the objective of the BOPD. Though the number of problems was small, they were generally encountered just as members of the treatment group were introduced to the practical details of the offset and procedures for its administration. It is possible that these early missteps reduced the employment response of treatment group volunteers to the offset. Nevertheless, the pilot ultimately demonstrated the feasibility of delivering the offset as a modification of current SSDI policy. The handling of these operational issues has been codified for the national demonstration.

### *Data Collection and Analysis*

The data for the analysis come from two sources. The first source is records from each of the state contractors that SSA hired to implement the benefit offset. These data contain individual-level identifiers necessary to identify the participant's administrative record (e.g., SSN), the date that the individual enrolled in the project, and an indicator used to identify whether the individual was randomly assigned by the contractor to the benefit offset group or the control group.

SSA administrative records are the second data source. The Social Security Master Earnings File (MEF) was used to construct three outcome measures for each year of demonstration participation: (1) an indicator for any earnings; (2) an indicator for earnings above the current annualized SGA level; and (3) mean earnings.<sup>16</sup> An extract from the SSA Master Beneficiary Record was employed to obtain data on each individual's age, gender, race, primary diagnosis used for the disability decision, date of disability onset, date of entitlement, date of TWP completion, and an additional outcome measure—benefit amount.

Random assignment was conducted by the contractors selected by the states to run the program. Evidence from other experiments suggests that randomization must be carefully monitored, since if the treatment is attractive, incentives are created to promote access for privileged candidates (Gueron 2000, 9). For the BOPD this problem was attenuated by two factors. First, selection was managed externally, and no candidate could be submitted more than once. Second, operating agencies were encouraged to “cream,” that is to select volunteers from persons judged likely to benefit from the offset. Thus in principle everyone in the pool was a desirable candidate, and making the decision based on luck of the draw was, from the operator's standpoint, the fairest solution given that the number of slots was limited.

If randomization was successful, the treatment and control groups should be identical save for random variations attributable to chance. Table 2 presents data on basic demographic and disability characteristics for the control and treatment groups. The overall sample size was

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<sup>16</sup> The MEF includes earnings records from W-2 forms from the IRS. Federal Insurance Contribution Act (FICA)-covered income is included as well as non-FICA covered income.

1,820, with 50.7 percent of the sample in the treatment group, a difference from the expected 50 percent that is not statistically significant. Most of the participants were in Vermont (31.3 percent); the fewest were in Connecticut (14.1 percent). Utah and Wisconsin each had about 27 percent of participants. There are some significant differences on particular characteristics (for example, more treatment group members report disability due to “infections and parasitic” diseases than is true for the control group), but chi-square tests for differences in the distribution of the two groups within characteristic categories (disability type, number of months since first EPE, etc.) reveal no differences approaching statistical significance. We conclude that there were no apparent problems with the random assignment process.

Table DESCSTATS about here

**Table 2:** Treatment and Control Group Characteristics

Table 2 reveals some important features of the sample: The median and mean age of participants is in the 40s; these are generally people with potential for more than a decade of additional work. The parity between women and men among BOPD volunteers is a bit of a surprise, given the national SSDI beneficiary population at the time of recruitment was, according to SSA’s online beneficiary database, 54 percent male. On average, volunteers had been beneficiaries for over five years, and fully half of qualifying disabilities involved mental health or capacity. Consistent with the focus of recruitment, the volunteers came to the project with recent workforce connection: 64 percent had reported earnings in the year before random assignment.

### **The Effects: Earnings**

Despite problems encountered in pilot implementation of the offset, there proved to be useful information in the estimated effects of the demonstration. We divide our discussion between the estimated effect of the offset on earnings and the effect on benefits. In reviewing effects estimates, it is important to keep in mind that the underlying data are for SSDI beneficiaries actively engaged in both work and rehabilitation. These results cannot be used for drawing inference about the national impact of general introduction of a similar incentive.

#### *Combined*

We begin with earnings results for all sites combined. In accordance with the SSDI and BOPD procedural emphasis on annual accounting on a calendar year basis, we report outcomes with reference to calendar years. Table 3 presents results for earnings status before, during, and after the year of random assignment. Reported earnings for the two years prior to random assignment are reported for reference in discussing earnings trajectories.

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**Table 3:** Estimated Earnings Effects, Benefit Offset Pilot Demonstration

Given the apparent success of random assignment, differences in mean outcomes between the benefit offset and control groups provide an unbiased estimate of the effects of the offset incentive. However, controlling for residual variation in beneficiary characteristics improves the



precision of the estimates. Accordingly, we present impact estimates based on multivariate models of the general form

$$Y_i = \alpha + \beta D_i + X_i' \gamma + \eta_i \quad (1)$$

where  $Y_i$  is the outcome of interest,  $\alpha$  is the constant,  $D_i$  is an indicator equal to 1 if individual  $i$  is randomly assigned to the benefit offset and 0 otherwise,  $\beta$  is the estimated coefficient for  $D_i$ ,  $X_i$  is a vector of individual level characteristics with  $\gamma$  representing the vector corresponding coefficients, and  $\eta_i$  is the error term for individual  $i$ . The individual level characteristics ( $X_i$ ) used as controls are defined in Appendix 1. We use a logit model to estimate equation (1) for the models that involve binary outcomes (any earnings and earnings that exceed the annualized SGA level). We use ordinary least squares (OLS) regressions to estimate the earnings amount model and the SSDI benefits paid model. The full equation estimates appear in Appendix 1. For outcomes measures that take on a value of 0 or 1, we compute the estimated effect of the benefit offset for each individual and then compute the mean of the individual effects.<sup>17</sup> Differences based on OLS estimates are simply the coefficient of the treatment indicator ( $\beta$ ) in the earnings regression.

Consider first the impact of the incentive on the presence of any earnings in the Master Earnings File for the year of random assignment and the two years following. Panel A of Table 3 indicates there is no statistically significant impact, whether measured with or without regression adjustment. Panel B indicates that there was also no appreciable impact of the intervention on mean earnings among those with earnings. Panel B of the table shows that those assigned to the benefit offset group earned about \$14,800 in the two years before random assignment and the control group had earned slightly but not statistically significantly less, about \$14,400. The unadjusted impact estimates show that mean annual earnings among those with earnings increased over the next three years for both groups, but somewhat more so for those in the benefit offset group. However, the difference is not statistically different from zero, with or without adjusting via regression for the residual differences between groups.

Given lack of effect on the prevalence of employment or earnings given employment, it may be surprising that, as indicated by the results in Panel C, the demonstration had a positive effect on the likelihood of earnings above SGA in the two years following random assignment. Note that two years before the year of random assignment about 15 percent of the sample had annualized earnings above SGA. (This could occur, for example, in cases in which disability onset occurred only recently or when the individual was in the TWP.) In the year prior to random assignment this rate fell, possibly because of engagement in vocational rehabilitation—VR programs were an important source of BOPD enrollees. In the two years following random assignment, the rate of employment above SGA increased for both treatment and control groups, but significantly more in the former. Thus, while the BOPD innovation appears to have had no impact on the

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<sup>17</sup> The individual level effects are computed as

$$E(Y_i|X_i, D_i = 1) - E(Y_i|X_i, D_i = 0) = \Lambda(\alpha + \beta + X_i\gamma) - \Lambda(\alpha + X_i\gamma)$$

where  $\Lambda(\cdot)$  is the logistic cumulative density function. We use the delta method to compute standard errors for the estimated effect. See Bartus (2005) for a discussion of this method.

likelihood of employment in the years subsequent to project initiation and random assignment, the likelihood that such employment would produce earnings above the SGA threshold increased. Given the straight-forward comparative statics of the labor supply model, the combination of positive effects on work above SGA but muted impact on earnings or the likelihood of employment likely results from the reduction in earnings among beneficiaries who would have performed SGA under the existing rules but chose to reduce their labor supply under the benefit offset rules.

We included pre-enrollment earnings data in Table 3 to show an important feature of the earnings history for this sample. Both the control and treatment groups show significant increases in earnings between the year prior to random assignment and the year after for those working. While detail on involvement of BOPD participants in vocational rehabilitation and counseling is not available, anecdotal evidence and the nature of the organizations that were the focus of recruitment suggest that significant numbers were involved in VR. The implication is that some part of the earnings gain may be attributable to services, and the net effect of the offset may be influenced by the high-service environment. A positive service impact would be consistent with the results of non-experimental studies of other service demonstrations, notably the State Partnership Initiative (SPI) projects (Trembley et al., 2004). In retrospect, it is clear that it would have been useful to collect better data on service receipt by BOPD treatment and control members, and this will be done in BOND.

#### *Variation across States*

The logit and OLS equations employed for making the regression adjustments for Table 3 all include indicator variables for state effects, and there is some evidence of meaningful differences across states in demonstration outcomes. In particular, Connecticut appears to have had a larger effect on the likelihood of some earnings in years subsequent to random assignment than did other states. To explore this further, we also ran state-specific analyses. However, they proved largely fruitless, in part because of low power. While we did find increasing earnings for both the treatment and control groups after random assignment in all states, the impact was almost uniformly statistically insignificant. Additionally, all states showed positive but statistically insignificant impacts on the percentage of beneficiaries with earnings above the annualized SGA two years after random assignment, ranging from 1.4 percent in Vermont to 7.3 percent in Connecticut. A complete set of impact estimates by state is available in Appendix 6.

#### **The Effects: Benefits**

The Congressional work incentive improvement mandate called for evaluation of the “reductions in Federal expenditures that may result from the permanent implementation of such a program.” The volunteer sample developed for the pilot is inappropriate for estimating expenditure effects of national implementation, but it is interesting to examine the impact of the offset pilot on expenditures for this exceptional group. The many operational issues previously described generate administrative costs that would not be incurred with revised design and seasoned management. Because of this, we analyze only cash benefits and not costs for administration.

The benefit offset program will decrease benefit payments among beneficiaries who are induced to work above the SGA level by the benefit offset provisions (those at point A in Figure 3); they

will receive a reduced benefit instead of the full cash benefit they would have received by not performing SGA under the current program rules. The benefit offset program will increase benefit payments among beneficiaries who would have had benefits suspended or terminated under the current program rules (those at points B or C in Figure 3). The net impact on benefit payments is determined by whether the decrease in payments from those induced to perform SGA is sufficient to cover the increase in payments due to paying partial benefits to those who would not receive benefits under the existing rules.

The results are presented in Table 4. We present only the regression-adjusted outcomes. We find that the benefit offset led to a net increase in benefits paid among volunteers in our sample. We estimate that for the population represented by the BOPD sample, SSA would payout about \$508 more in benefits per beneficiary in the year following the random assignment year, and \$546 more in the second year after random assignment as shown in Panel A. Thus, for the volunteers participating in the pilot, we find that the reduction in benefits associated with the impact of the benefit offset on the likelihood of working above SGA is not sufficient to offset the cost of additional benefits paid to those who would have worked about SGA under the current program rules.

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**Table 4:** Regression-Adjusted Estimates of Benefit Offset Effects on Benefits Paid, Combined Sample and by State

There are large differences by site. In Utah, Vermont, and Wisconsin, we estimate additional payouts of \$56, \$913, and \$743, respectively, two years after random assignment (although only the results for Vermont and Wisconsin are statistically significant at the 5 percent level). In Connecticut, we find a small decrease in benefits paid (-\$43), but the estimate is not precise.

### **Conclusions**

We began this paper by discussing a behavioral and a capacity unknown. The behavioral unknown concerned the net effect of introduction of the SSDI employment incentive proposed by Congress in 1999. The capacity unknown involved the ability of the Social Security Administration to deliver the “benefit offset” incentive in an experimental situation. The capacity unknown was sensibly tested with the Benefit Offset Pilot Demonstration. The evidence is that SSA successfully crafted an operational version of the incentive that was consistent with other Social Security programs and managed, in partnership with four states, to conduct a randomized control trial of the innovation. The pilot provided information on administrative issues certain to be encountered in a more general evaluation. In making these judgments, we acknowledge that we are not neutral observers.

Regarding the behavioral unknown, the results of the experiment underscore what would be predicted based on standard economic analysis: The effect on employment, benefits, and costs of an incentive structured like the offset experiment is ambiguous. For the select population used for the BOPD, the prevalence of employment above the SGA threshold increased, but there does not appear to be an effect on general likelihood of working. However, the BOPD sample targeted persons with a high probability of using the offset (and other employment services); this meant that those persons for whom the offset was likely to increase costs were overrepresented

compared to their numbers in the entire SSDI beneficiary pool. Beneficiaries not yet engaged in rehabilitation and other efforts to obtain work are underrepresented. If the offset opportunity accelerates the completion of the Trial Work Period and the move to earnings levels above SGA among the yet-to-be engaged, the results may be different.

The results of the pilot have had implications for the Benefit Offset National Demonstration, soon underway. First, these results underscore the importance of proceeding with BOND: The effects of the offset are by no means pre-ordained. Additionally, BOND will collect data on service receipt and other aspects of the environments in which beneficiaries are making employment decisions. Such information will help in identifying the sources of inter-site variation in outcomes. Finally, SSA is taking great care in crafting and delivering information both about the incentive and about the procedures through which the policy will be administered. At the same time, what participants actually experience must conform to what policy intends. Here, as always, policy analysis and program evaluation cannot be divorced from the details of implementation.

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**Figures and Tables**

Figure 1: SSDI Recipients as a Percentage of Insured Population Age 18-64, 1967 – 2009

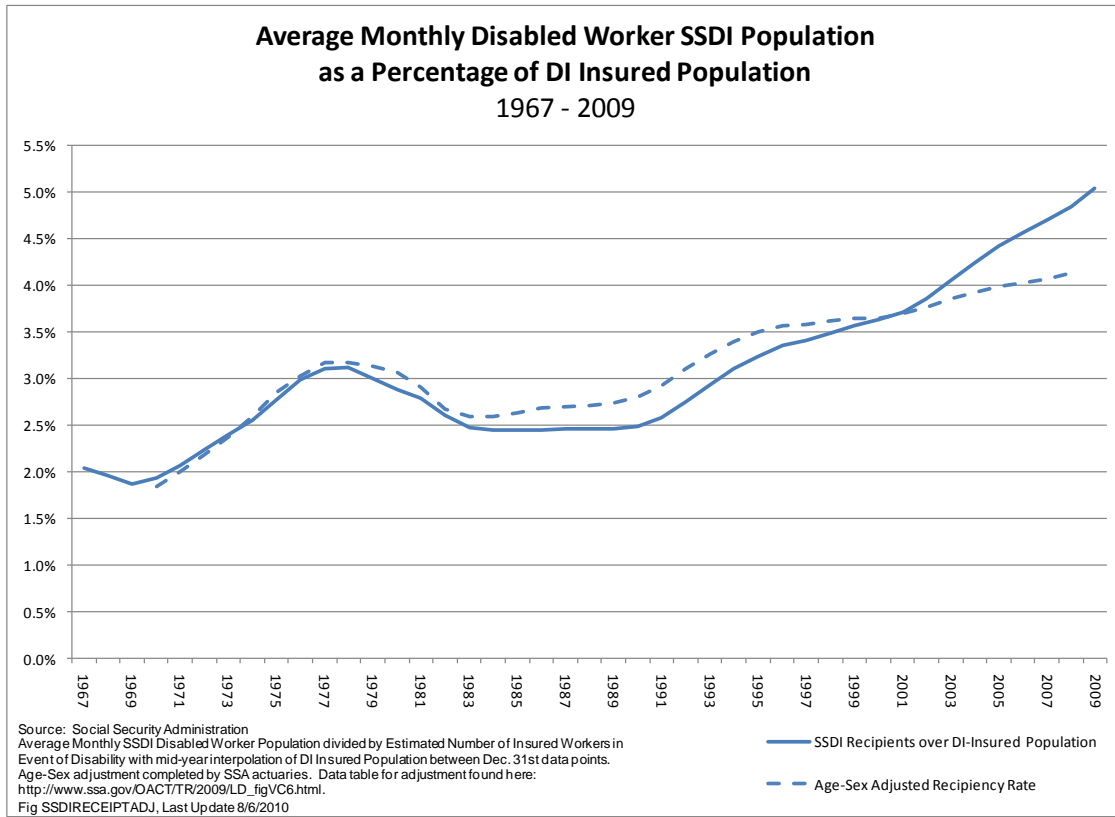


Figure 2: The SSDI Journey to Work

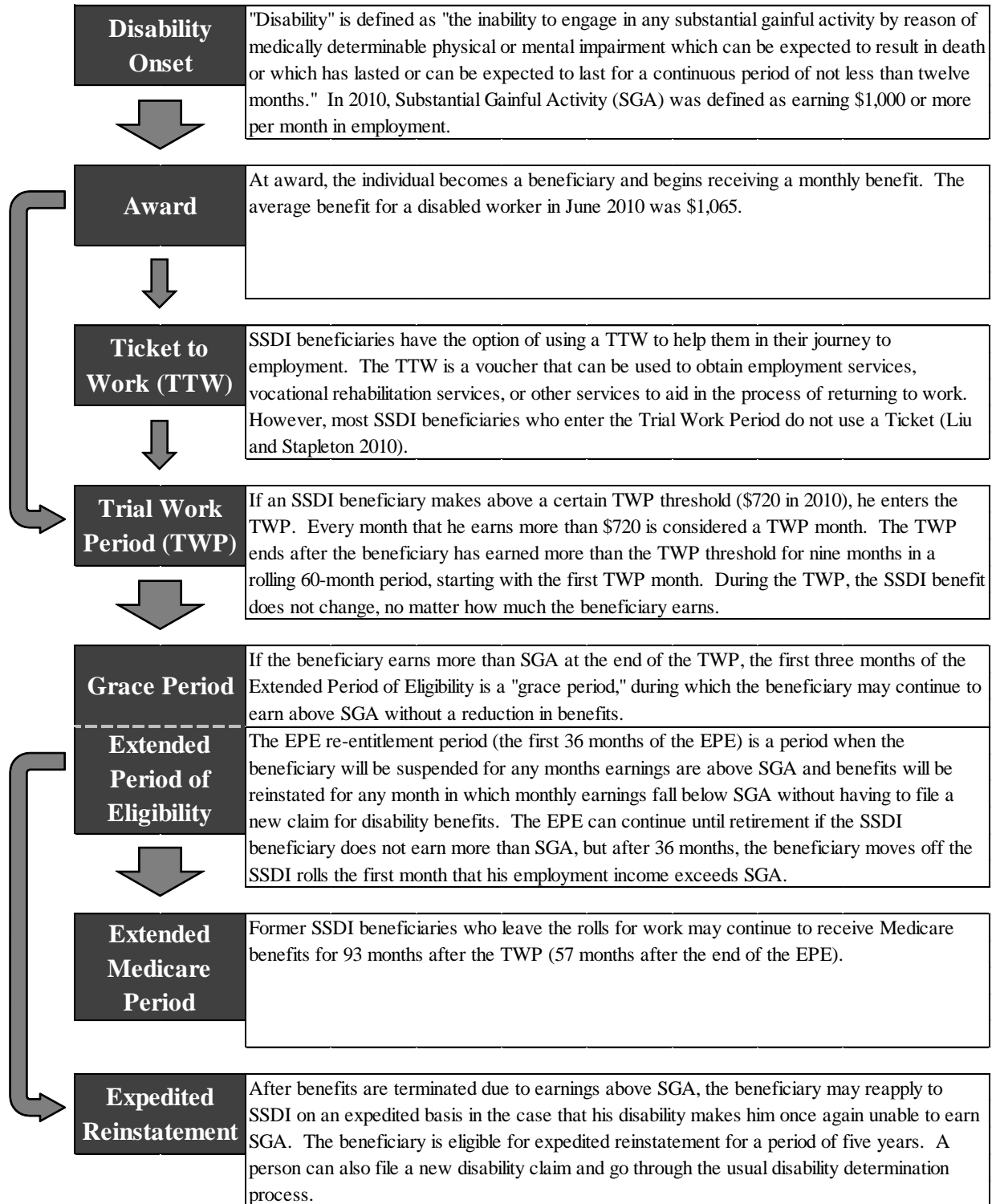


Figure JOURNEY, 24 August 2010



Figure 3: Beneficiary Response to a Benefit Offset

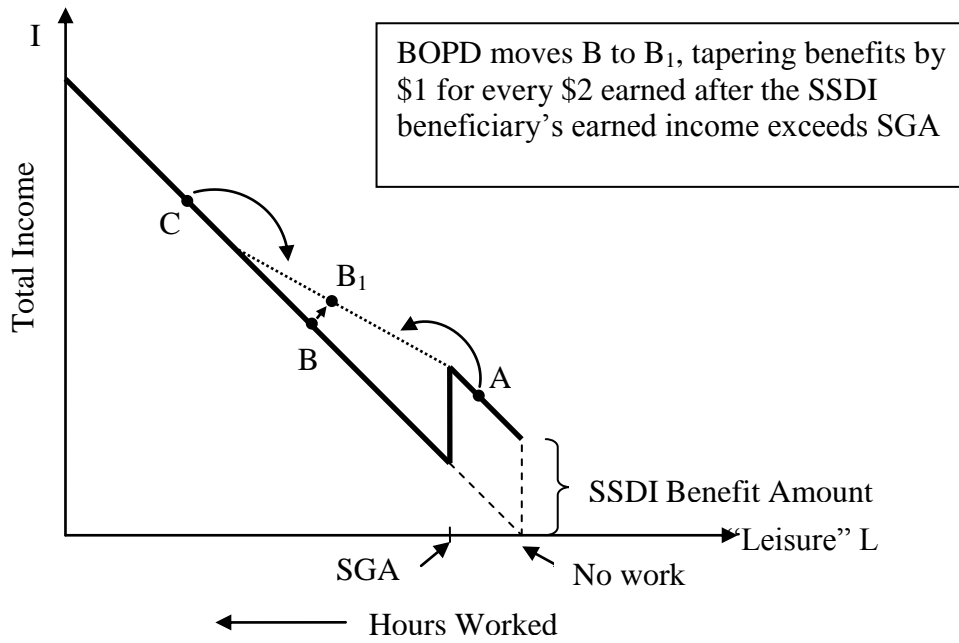


Table 1: Examples of Benefit Calculation in the Benefit Offset Pilot Demonstration

<b>Examples of Benefit Calculation in the Benefit Offset Pilot Demonstration</b>				
	Person "A"	Person "B"	Person "C"	Person "D"
Status at Beginning of the Year	Working with Earnings Greater than SGA Threshold and:			
	Trial Work Period and Grace Period complete	Grace Period not completed until February	Trial Work Period and Grace Period complete, but annual estimate not provided until the end of February	Trial Work Period and Grace Period complete
Last Month of Grace Period	Completed before January	Completed in February	Completed before January	Completed before January
Earnings Estimate	\$24,000 for the year	\$20,000 for March through December	\$24,000 for the year	\$24,000 for the year
Subtract Earnings Disregard	-\$12,000	-\$10,000	-\$12,000	-\$12,000
Countable Earnings	\$12,000	\$10,000	\$12,000	\$12,000
Benefit Offset (½ x Countable Earnings)	\$6,000	\$5,000	\$6,000	\$6,000
Monthly Benefit Reduction	-\$500	-\$500	-\$600	-\$500
Mid-Year Adjustment	None	None	None	Beneficiary's health worsens in June; she submits new earnings estimate of \$750 per month for July through the end of the year. SSA reimburses her \$1,875
Mid-Year Change in Benefit Reduction	N/A	N/A	N/A	-\$187.50
At the End of the Year, IRS Records Show	Beneficiary only made \$20,000	Minus pay stubs for January and February, beneficiary made \$20,000	Beneficiary made \$25,000	Beneficiary made \$16,500
Benefit Offset (1/2*Countable Earnings)	\$4,000	\$5,000	\$6,500	\$2,250
End-of-Year Reconciliation	SSA owes beneficiary \$2,000	No balance	Beneficiary owes SSA \$500	No balance
Table BENCALC; 24 August 2010				

Table 2: Treatment and Control Group Characteristics

	Total	Treatment	Control	Difference (T-C)		
Total (N)	1820	923	897			
	%	%	%	Estimate	SE	p-value
<b>State</b>						
Connecticut	14.12	13.65	14.60	-0.95	1.633	0.56
Utah	27.36	27.19	27.54	-0.34	2.090	0.87
Vermont	31.32	30.44	32.22	-1.77	2.175	0.41
Wisconsin	27.20	28.71	25.64	3.07	2.086	0.14
<b>Age at Enrollment</b>	45.95	45.91	45.99	-0.08	0.439	0.86
<b>Age</b>						
Under 29	6.21	6.07	6.35	-0.29	1.131	0.80
30 to 39	17.80	19.28	16.28	3.01	1.794	0.09
40 to 49	37.03	35.10	39.02	-3.92	2.264	0.08
50 to 59	33.30	34.02	32.55	1.47	2.210	0.51
60 or over	5.66	5.53	5.80	-0.27	1.083	0.80
<b>Age Entitled</b>	39.66	39.70	39.62	0.08	0.463	0.87
Years Since Entitlement	5.78	5.72	5.85	-0.13	0.230	0.58
<b>Gender</b>						
Male	50.60	51.90	49.28	2.62	2.344	0.26
Female	49.40	48.10	50.72	-2.62	2.344	0.26
<b>Earnings History</b>						
Had Earnings in Second Year before RA	61.21	60.89	61.54	-0.65	2.285	0.78
Had Earnings in First Year before RA	64.12	62.62	65.66	-3.04	2.249	0.18
<b>Year of Assignment</b>						
2005	22.20	22.54	21.85	0.68	1.948	0.73
2006	77.80	77.46	78.15	-0.68	1.948	0.73
<b>Disability Type</b>						
Other Mental	48.74	46.80	50.72	-3.92	2.344	0.09
Retardation	2.64	2.60	2.68	-0.08	0.751	0.92
Musculoskeletal	14.45	14.52	14.38	0.14	1.649	0.93
Nervous System	12.20	12.13	12.26	-0.13	1.534	0.93
Other	21.98	23.94	19.96	3.99	1.942	0.04
Infectious and Parasitic	1.04	1.63	0.45	1.18	0.477	0.01
Neoplasms	1.48	1.95	1.00	0.95	0.567	0.09
Endocrine	3.24	2.93	3.57	-0.64	0.830	0.44
Blood	0.11	0.00	0.22	-0.22	0.155	0.15
Circulatory System	4.40	4.77	4.01	0.75	0.961	0.43
Respiratory System	1.59	1.95	1.23	0.72	0.587	0.22
Digestive System	0.77	0.98	0.56	0.42	0.410	0.31
Genitourinary System	1.98	2.28	1.67	0.60	0.653	0.36
Skin	0.05	0.11	0.00	0.11	0.110	0.32
Congenital Anomalies	0.38	0.54	0.22	0.32	0.290	0.27
Injuries	4.84	4.55	5.13	-0.58	1.006	0.57
Other	0.16	0.11	0.22	-0.11	0.190	0.55
Unknown	1.92	2.17	1.67	0.49	0.644	0.44

Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data.

Table DESCSTATS; 12 August 2010

Table 3: Estimated Earnings Effects, Benefit Offset Pilot Demonstration

Panel A: Any Earnings	Treatment		Control		Difference (T-C)		
	%	SE	%	SE	Estimate	SE	p-value
<b>Pre-Enrollment Characteristics</b>							
Two years prior to Random Assignment (RA)	60.89	1.61	61.54	1.63	-0.65	2.29	0.78
One year prior to RA	62.62	1.59	65.66	1.59	-3.04	2.25	0.18
<b>Unadjusted Impacts</b>							
Year of RA	74.15	1.45	73.01	1.49	1.14	2.08	0.58
One year after RA	73.46	1.46	73.90	1.47	-0.44	2.07	0.83
Two years after RA	64.75	1.59	66.25	1.59	-1.50	2.25	0.51
<b>Regression Adjusted Impacts</b>							
Year of RA	74.53	1.29	72.61	1.34	1.92	1.87	0.30
One year after RA	73.60	1.39	73.76	1.41	-0.16	1.99	0.94
Two years after RA	64.80	1.52	66.20	1.53	-1.40	2.16	0.52
Panel B: Mean Annual Earnings (If Any Earnings)	Treatment		Control		Difference (T-C)		
	\$	SE	\$	SE	Estimate (\$)	SE	p-value
<b>Pre-Enrollment Characteristics</b>							
Two years prior to RA	8,141	414	7,855	412	286	584	0.63
One year prior to RA	6,703	321	6,516	342	187	469	0.69
<b>Unadjusted Impacts</b>							
Year of RA	7,056	278	7,748	331	-692	432	0.11
One year after RA	9,292	362	8,985	382	307	526	0.56
Two years after RA	11,179	676	9,902	454	1,277	815	0.12
<b>Regression Adjusted Impacts</b>							
Year of RA	7,053	90	7,752	90	-699	399	0.08
One year after RA	9,200	96	9,079	96	122	502	0.81
Two years after RA	11,014	109	10,067	109	946	805	0.24
Panel C: Earnings above Annualized SGA	Treatment		Control		Difference (T-C)		
	%	SE	%	SE	Estimate	SE	p-value
<b>Pre-Enrollment Characteristics</b>							
Two years prior to RA	15.17	1.18	14.16	1.16	1.01	1.65	0.54
One year prior to RA	10.94	1.03	10.70	1.03	0.24	1.46	0.87
<b>Unadjusted Impacts</b>							
Year of RA	15.81	1.21	14.67	1.18	1.14	1.69	0.50
One year after RA	22.37	1.38	17.32	1.27	5.05	1.88	0.01
Two years after RA	21.22	1.36	16.70	1.26	4.52	1.85	0.02
<b>Regression Adjusted Impacts</b>							
Year of RA	15.75	1.03	14.70	1.05	1.05	1.48	0.48
One year after RA	22.07	1.27	17.56	1.20	4.51	1.75	0.01
Two years after RA	20.86	1.24	17.00	1.19	3.86	1.72	0.03
Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data. See text for detail on calculation method.							
Table EARNEFFECTS; 27 August 2010							

Table 4: Regression-Adjusted Estimates of Benefit Offset Effects on Benefits Paid, Combined Sample and by State

	Treatment	Control	Difference (T-C)		
	\$	\$	Estimate	SE	p-value
<b>All States</b>					
Regression Adjusted Impacts					
Year of random assignment (RA)	9,567	9,510	57	46	0.71
One year after RA	10,641	10,132	508	41	0.00
Two years after RA	10,648	10,102	546	42	0.00
<b>Connecticut</b>					
Regression Adjusted Impacts					
Year of RA	10,012	9,966	45	157	0.92
One year after RA	10,348	10,473	-125	142	0.82
Two years after RA	10,202	10,245	-43	139	0.94
<b>Utah</b>					
Regression Adjusted Impacts					
Year of RA	9,380	9,504	-124	87	0.66
One year after RA	10,582	10,296	286	86	0.38
Two years after RA	10,264	10,208	56	94	0.88
<b>Vermont</b>					
Regression Adjusted Impacts					
Year of RA	9,659	9,618	41	80	0.88
One year after RA	10,892	10,226	666	73	0.03
Two years after RA	11,011	10,098	913	75	0.01
<b>Wisconsin</b>					
Regression Adjusted Impacts					
Year of RA	9,334	9,240	94	92	0.75
One year after RA	10,415	9,823	593	79	0.08
Two years after RA	10,755	10,012	743	79	0.03
Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data. Table BENPAID; 12 August 2010					

{END}

# The SSDI "Benefit Offset" Experiment: Landing the Pilots

Robert Weathers, Jeffrey Hemmeter, Michael Wiseman

## Appendix: Detailed Estimation Results

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**Appendix Table A1. Variable Names and Definitions Used in Regression-Adjusted Impact Estimates**

<b>Variable</b>	<b>Definition</b>
<b><u>Outcome Variables</u></b>	
Any Earnings	Indicator variable equal to 1 if person has earnings greater than 0 reported in the Master Earnings Record, 0 otherwise.
Annualized SGA Earnings	Indicator variable equal to 1 if person has earnings greater than the annualized SGA earnings amount from the Master Earnings Record, 0 otherwise.
Earnings Amount	Earnings amount reported in the Master Earnings Record.
Benefits Paid	Benefits paid amount reported in SSA administrative data.
<b><u>Explanatory Variables</u></b>	
Treatment	Indicator variable equal to 1 if the participant is in the benefit offset group and equal to 0 if in the control group.
<b>Pre-enrollment</b>	
<b>Dependent Variables:</b>	
1 year prior to RA	Value of dependent value in the calendar year before the calendar year that random assignment occurred.
2 years prior to RA	Value of dependent value in the calendar year that occurs two years before the calendar year of random assignment.
3 years prior to RA	Value of dependent value in the calendar year that occurs three years before the calendar year of random assignment.
Male	Indicator variable equal to 1 if participant is male, 0 if female.
<b>Age at Assignment:</b>	
Under age 30	Participant is under age 30. (reference category)
30 to 39	Indicator equal to 1 if participant is between the ages of 30 and 39, and 0 otherwise.
40 to 49	Indicator equal to 1 if participant is between the ages of 40 and 49, and 0 otherwise.
50 to 59	Indicator equal to 1 if participant is between the ages of 50 and 59, and 0 otherwise.
60 or older	Indicator equal to 1 if participant is between the ages of 60 or older, and 0 otherwise.
<b>Years Since Onset:</b>	
Within first 2 years	Participant experienced disability within 2 years. (reference category)
3 years	Indicator equal to 1 if participant experienced disability onset 3 years ago, and 0 otherwise.
4 years	Indicator equal to 1 if participant experienced disability onset 4 years ago, and 0 otherwise.
5 years	Indicator equal to 1 if participant experienced disability onset 5 years ago, and 0 otherwise.
6 years	Indicator equal to 1 if participant experienced disability onset 6 years ago, and 0 otherwise.
7 years	Indicator equal to 1 if participant experienced disability onset 7 years ago, and 0 otherwise.
8 years	Indicator equal to 1 if participant experienced disability onset 8 years ago, and 0 otherwise.
9 years	Indicator equal to 1 if participant experienced disability onset 9 years ago, and 0 otherwise.
10 or more years	Indicator equal to 1 if participant experienced disability onset 10 years ago or longer, and 0 otherwise.

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**Appendix Table A1. Variable Names and Definitions Used in Regression-Adjusted Impact Estimates, continu**

<b>Variable</b>	<b>Definition</b>
<b>State:</b>	
CT	Indicator equal to 1 if participant is from Connecticut site, 0 otherwise.
UT	Indicator equal to 1 if participant is from Utah site, 0 otherwise.
VT	Participant is from Vermont Site. (reference category)
WI	Indicator equal to 1 if participant is from Wisconsin site, 0 otherwise.
<b>Impairment:</b>	
Musculoskeletal	Participant has Musculoskeletal impairment. (reference category)
Other Mental	Indicator equal to 1 if participant has mental impairment that is not mental retardation, 0 otherwise.
Mental Retardation	Indicator equal to 1 if participant has mental retardation, 0 otherwise.
Nervous System	Indicator equal to 1 if participant has nervous system impairment, 0 otherwise.
Other Disability	Indicator equal to 1 if participant has other impairment, 0 otherwise.
EPE at Enrollment	Indicator equal to 1 if participant is in the Extended Period of Eligibility at enrollment, 0 otherwise.
<b>Outcome Year :</b>	
2006	Indicator equal to 1 if the calendar year for the outcome variable is in 2006, 0 if it is in 2005. Because enrollment occurred between 2005 and 2006, the enrollment year will be 2005 for some participants and 2006 for others.
2007	Indicator equal to 1 if the calendar year for the year following enrollment is 2007, 0 if it is 2006. Because enrollment occurred between 2005 and 2006, the year following enrollment will be 2006 for some participants and 2007 for others.
2008	Indicator equal to 1 if the calendar year for the outcome variable is in 2008, 0 if it is in 2007. Because enrollment occurred between 2005 and 2006, the second year following the enrollment year will be 2007 for some participants and 2008 for others.

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Source: Authors' construction using SSA administrative data linked to BOPD enrollment data.

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**Appendix Table A2. Logit Results for Impact on Annual Earnings Greater than 0, All States**

<b>Category</b>	<b>Variable</b>	<b>Enrollment Year</b>	<b>Year Following Enrollment</b>	<b>Two Years Following Enrollment</b>
	Treatment	0.12 (0.12)	-0.01 (0.11)	-0.07 (0.11)
<b>Pre-Enrollment Earnings:</b>	1 year prior to RA	2.96 *** (0.28)	1.12 *** (0.19)	0.74 *** (0.15)
	2 years prior to RA	-0.04 (0.16)	0.09 (0.15)	-0.02 (0.13)
	3 years prior to RA	0.12 (0.10)	0.06 (0.10)	0.15 (0.09)
<b>Gender:</b>	Male	-0.26 ** (0.12)	-0.11 (0.12)	-0.02 (0.11)
<b>Age at Assignment:</b>	30 to 39	-0.79 ** (0.37)	-0.43 (0.31)	-0.52 * (0.27)
	40 to 49	-1.00 *** (0.35)	-0.44 (0.29)	-0.46 * (0.26)
	50 to 59	-1.38 *** (0.35)	-0.64 ** (0.30)	-0.80 *** (0.26)
	60 or older	-1.65 *** (0.42)	-1.02 *** (0.36)	-0.87 *** (0.33)
<b>Years Since Onset:</b>	3 years	0.11 (0.24)	0.24 (0.23)	0.16 (0.22)
	4 years	0.50 ** (0.24)	0.43 * (0.23)	0.30 (0.21)
	5 years	0.52 ** (0.26)	0.54 ** (0.25)	0.19 (0.23)
	6 years	1.14 *** (0.31)	0.88 *** (0.28)	0.47 * (0.25)
	7 years	0.21 (0.29)	0.44 (0.28)	0.11 (0.26)
	8 years	0.21 (0.34)	0.14 (0.32)	-0.10 (0.30)
	9 years	0.20 (0.37)	0.20 (0.35)	0.30 (0.33)
	10 or more years	0.37 (0.23)	0.21 (0.22)	0.09 (0.21)

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**Appendix Table A2. Logit Results for Impact on Annual Earnings Greater than 0, All States, continued**

Category	Variable	Enrollment Year	Year Following Enrollment	Two Years Following Enrollment
<b>State:</b>	CT	0.38 *	0.38 *	0.62 ***
		(0.22)	(0.20)	(0.19)
	UT	-0.17	-0.08	-0.05
		(0.15)	(0.15)	(0.14)
	WI	0.28 *	0.36 **	0.22
		(0.16)	(0.15)	(0.14)
<b>Disability:</b>	Other Mental	0.22	0.44 ***	0.21
		(0.18)	(0.17)	(0.16)
	Mental Retardation	0.88	0.95 **	0.31
		(0.53)	(0.48)	(0.38)
	Nervous System	-0.30	0.25	0.21
		(0.23)	(0.22)	(0.21)
	Other Disability	0.03	0.21	0.00
		(0.20)	(0.18)	(0.17)
<b>EPE at Enrollment:</b>	Yes	0.74 ***	0.76 ***	0.69 ***
		(0.26)	(0.22)	(0.18)
<b>Year:</b>	2006	-0.15		
		(0.15)		
	2007		-0.33 **	
			(0.14)	
	2008			-0.38 ***
			(0.13)	
	Constant	1.02 **	0.64	0.72 **
		(0.45)	(0.40)	(0.36)
<b>Number of Observations</b>		1810	1801	1785
<b>Predicted Probability</b>		0.74	0.74	0.66

Source: Authors' calculations from SSA administrative data linked to BOPD enrollment data.

Notes: Standard errors are in parentheses. \* indicates statistically significant at 0.10 level, \*\* indicates statistically significant at the 0.05 level, and \*\*\* indicates statistically significant at the 0.01 level.

Appendix Table A3. Logit Results for Impact on Annual Earnings Greater than Annualized SGA Level, All States

Category	Variable	Enrollment Year	Year following Enrollment	Two Years following Enrollment
	Treatment	0.11 (0.15)	0.33 ** (0.13)	0.30 ** (0.13)
<b>Pre-Enrollment Earnings:</b>	1 year prior to RA	1.53 *** (0.20)	0.78 *** (0.20)	0.57 *** (0.20)
	2 years prior to RA	0.05 (0.24)	-0.05 (0.21)	-0.02 (0.22)
	3 years prior to RA	0.13 (0.23)	0.31 (0.20)	0.14 (0.21)
<b>Gender:</b>	Male	-0.30* (0.16)	0.00 (0.13)	-0.13 (0.14)
<b>Age at Assignment:</b>	30 to 39	-0.11 (0.32)	-0.34 (0.27)	-0.25 (0.28)
	40 to 49	-0.01 (0.31)	-0.29 (0.25)	-0.19 (0.26)
	50 to 59	-0.48 (0.32)	-0.78 *** (0.27)	-0.66 ** (0.27)
	60 or older	-0.65 (0.47)	-1.86 *** (0.48)	-2.00 *** (0.54)
<b>Years Since Onset:</b>	3 years	-0.31 (0.31)	-0.09 (0.25)	-0.16 (0.26)
	4 years	-0.44 (0.31)	-0.22 (0.26)	-0.15 (0.26)
	5 years	-0.25 (0.32)	-0.19 (0.28)	-0.33 (0.28)
	6 years	-0.14 (0.34)	-0.23 (0.30)	-0.09 (0.30)
	7 years	0.06 (0.35)	-0.30 (0.32)	-0.74 ** (0.35)
	8 years	-0.66 (0.45)	-0.17 (0.36)	-0.71 * (0.40)
	9 years	-0.42 (0.53)	0.17 (0.39)	-0.33 (0.43)
	10 or more years	-0.29 (0.30)	-0.25 (0.25)	-0.16 (0.26)

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**Appendix Table A3. Logit Results for Impact on Annual Earnings Greater than Annualized SGA Level, All States, continued**

<b>Category</b>	<b>Variable</b>	<b>Enrollment Year</b>	<b>Year following Enrollment</b>	<b>Two Years following Enrollment</b>
<b>State:</b>	CT	0.33 (0.22)	0.15 (0.20)	0.10 (0.21)
	UT	-0.24 (0.21)	0.08 (0.17)	0.02 (0.17)
	WI	-0.06 (0.20)	-0.17 (0.18)	-0.29 (0.18)
<b>Disability:</b>	Other Mental	-0.05 (0.24)	-0.10 (0.21)	0.17 (0.23)
	Mental Retardation	0.10 (0.48)	-0.51 (0.46)	-0.66 (0.52)
	Nervous System	0.15 (0.30)	0.38 (0.25)	0.90 *** (0.27)
	Other Disability	0.05 (0.27)	0.30 (0.22)	0.78 *** (0.24)
<b>EPE at Enrollment:</b>	Yes	1.97 *** (0.17)	1.49 *** (0.16)	1.59 *** (0.16)
<b>Year:</b>	2006	-0.03 (0.18)		
	2007		-0.37 ** (0.15)	
	2008			-0.66 *** (0.15)
	Constant	-2.06 *** (0.47)	-1.26 *** (0.40)	-1.28 *** (0.41)
<b>Number of Observations</b>		1810	1801	1785
<b>Predicted Probability</b>		0.15	0.20	0.19
Source: Authors' calculations from SSA administrative data linked to BOPD enrollment data.				
Notes: Standard errors are in parentheses. *indicates statistically significant at 0.10 level, ** indicates statistically significant at the 0.05 level, and *** indicates statistically significant at the 0.01 level.				

**Appendix Table A4. Regression Results for Impact on Earnings if Earnings are Greater than 0, All States**

<b>Category</b>	<b>Variable</b>	<b>Enrollment Year</b>	<b>Year Following Enrollment</b>	<b>Two Years Following Enrollment</b>
	Treatment	-699 *	122	946
		(399)	(502)	(805)
<b>Pre-Enrollment Earnings:</b>	1 year prior to RA	3352 ***	1945 ***	-647
		(548)	(657)	(1083)
	2 years prior to RA	-202	-914	354
		(553)	(692)	(1143)
	3 years prior to RA	-1379 ***	-1675 **	-1762
		(530)	(663)	(1091)
<b>Gender:</b>	Male	-342	-514	-831
		(408)	(513)	(824)
<b>Age at Assignment:</b>	30 to 39	-1158	-1441	-475
		(858)	(1102)	(1744)
	40 to 49	-290	-872	1123
		(811)	(1040)	(1637)
	50 to 59	-1164	-3083 ***	-879
		(836)	(1066)	(1691)
	60 or older	-2342 **	-5185 ***	-3862
		(1186)	(1516)	(2400)
<b>Years Since Onset:</b>	3 years	-833	-2386 **	-217
		(753)	(943)	(1492)
	4 years	-1621 **	-2430 ***	204
		(697)	(878)	(1397)
	5 years	-901	-1945 **	-846
		(762)	(948)	(1519)
	6 years	-1181	-3022 ***	-2622
		(803)	(1014)	(1628)
	7 years	-2162 **	-3875 ***	-2977
		(933)	(1144)	(1857)
	8 years	-2126 **	-3281 **	-2917
		(1076)	(1359)	(2219)
	9 years	-1564	-970	-2060
		(1256)	(1550)	(2409)
	10 or more years	-1744 **	-2867 ***	-1772
		(699)	(875)	(1395)

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**Appendix Table A4. Regression Results for Impact on Earnings if Earnings are Greater than 0, All States, continued**

<b>Category</b>	<b>Variable</b>	<b>Enrollment Year</b>	<b>Year Following Enrollment</b>	<b>Two Years Following Enrollment</b>
<b>State:</b>	CT	577 (617)	900 (782)	268 (1240)
	UT	-323 (545)	132 (683)	-543 (1107)
	WI	-376 (521)	-552 (656)	-2034 * (1062)
<b>Disability:</b>	Other Mental	-720 (624)	-1512 * (797)	-367 (1266)
	Mental Retardation	-1870 (1271)	-3893 ** (1611)	-2251 (2609)
	Nervous System	226 (819)	-170 (1013)	2882 * (1594)
	Other Disability	599 (701)	664 (895)	3176 ** (1429)
<b>EPE at Enrollment:</b>	Yes	6482 *** (510)	6777 *** (645)	6927 *** (1023)
	<b>Year:</b>	2006 (476)	2007 (592)	2008 (941)
	Constant	7108 *** (1261)	13496 *** (1599)	12230 *** (2558)
<b>Number of Observations</b>		1332	1327	1169
<b>R-Squared</b>		0.178	0.134	0.073

Source: Authors' calculations from SSA administrative data linked to BOPD enrollment data.

Notes: Standard errors are in parentheses. \*indicates statistically significant at 0.10 level, \*\* indicates statistically significant at the 0.05 level, and \*\*\* indicates statistically significant at the 0.01 level.

Appendix Table A5. Regression Results for Impact on Benefits Paid, All States

Category	Variable	Enrollment Year	Year Following Enrollment	Two Years Following Enrollment
	Treatment	57 (151)	508 *** (173)	546.05 *** (188)
<b>Pre-Enrollment Earnings:</b>	1 year prior to RA	-136 (188)	-127 (215)	-75 (235)
	2 years prior to RA	-229 (208)	-116 (239)	-258 (261)
	3 years prior to RA	518 ** (201)	683 *** (231)	801 *** (252)
<b>Gender:</b>	Male	1334 *** (153)	1587 *** (176)	1609 *** (192)
<b>Age at Assignment:</b>	30 to 39	1230 *** (352)	1287 *** (404)	1247 *** (438)
	40 to 49	2536 *** (333)	2681 *** (381)	2570 *** (415)
	50 to 59	2841 *** (340)	3270 *** (389)	3303 *** (423)
	60 or older	3109 *** (450)	3994 *** (515)	4363 *** (563)
<b>Years Since Onset:</b>	3 years	-329 (284)	-605 * (325)	-549 (355)
	4 years	281 (267)	21 (306)	-78 (335)
	5 years	-75 (292)	-550 (335)	-535 (365)
	6 years	-137 (318)	-639 * (366)	-406 (399)
	7 years	-609 * (348)	-736 * (402)	-441 (438)
	8 years	-425 (404)	-1308 *** (463)	-889 * (505)
	9 years	-999 ** (460)	-1383 *** (526)	-1684 *** (572)
	10 or more years	-1048 *** (264)	-1446 *** (303)	-1218 *** (331)

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**Appendix Table A5. Regression Results for Impact on Benefits Paid, All States, continued**

<b>Category</b>	<b>Variable</b>	<b>Enrollment Year</b>	<b>Year Following Enrollment</b>	<b>Two Years Following Enrollment</b>
<b>State:</b>	CT	537 ** (245)	465 * (282)	310 (308)
	UT	-97 (201)	-143 (230)	-315 (251)
	WI	-290 (198)	-463 ** (228)	-152 (248)
<b>Disability:</b>	Other Mental	117 (231)	311 (264)	280 (288)
	Mental Retardation	-1322 ** (516)	-1362 ** (591)	-1162 * (642)
	Nervous System	675 ** (297)	634 * (340)	426 (370)
	Other Disability	587 ** (256)	534 * (294)	258 (321)
<b>EPE at Enrollment:</b>	Yes	-1316 *** (212)	-2452 *** (243)	-2575 *** (265)
<b>Year:</b>	2006	3315 *** (182)		
	2007		236 (209)	
	2008			317 (227)
	Constant	4214 *** (492)	7162 *** (563)	7083 *** (613)
<b>Number of Observations</b>		1810	1801	1785
<b>R-Squared</b>		0.278	0.194	0.172

Source: Authors' calculations from SSA administrative data linked to BOPD enrollment data.

Notes: Standard errors are in parentheses. \* indicates statistically significant at 0.10 level, \*\* indicates statistically significant at the 0.05 level, and \*\*\* indicates statistically significant at the 0.01 level.



Appendix Table A6. Estimated Earnings Effects, Benefit Offset Pilot Demonstration, by State																
	Connecticut				Utah				Vermont				Wisconsin			
	Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)	
Panel A: Any Earnings	%	%	Estimate	p-value	%	%	Estimate	p-value	%	%	Estimate	p-value	%	%	Estimate	p-value
<b>Pre-Enrollment</b>																
Two years prior to RA (standard error)	71.43 (4.04)	77.10 (3.69)	-5.67 (5.47)	0.30	49.40 (3.16)	48.18 (3.19)	1.22 (4.49)	0.79	64.06 (2.87)	65.40 (2.80)	-1.34 (4.01)	0.74	63.40 (2.96)	62.17 (3.20)	1.23 (4.36)	0.78
One year prior to RA	81.75 (3.46)	83.97 (3.22)	-2.22 (4.73)	0.64	56.57 (3.13)	53.85 (3.18)	2.72 (4.46)	0.54	61.57 (2.91)	65.74 (2.80)	-4.17 (4.04)	0.30	60.38 (3.01)	67.83 (3.09)	-7.45 (4.31)	0.09
<b>Regression Adjusted Impacts</b>																
Year of RA	86.91 (2.56)	81.66 (2.93)	5.25 (4.03)	0.19	65.36 (2.77)	65.68 (2.73)	-0.32 (3.91)	0.94	72.73 (2.40)	74.26 (2.36)	-1.53 (3.42)	0.66	78.05 (2.28)	72.77 (2.60)	5.28 (3.51)	0.13
One year after RA	80.29 (3.44)	84.42 (3.19)	-4.13 (4.83)	0.39	67.27 (2.86)	68.35 (2.81)	-1.08 (4.03)	0.79	71.36 (2.56)	72.39 (2.50)	-1.03 (3.62)	0.78	78.74 (2.37)	74.90 (2.91)	3.84 (3.61)	0.29
Two years after RA	78.69 (3.60)	77.68 (3.68)	1.01 (5.40)	0.85	57.64 (3.00)	62.11 (2.96)	-4.47 (4.24)	0.29	63.15 (2.77)	63.50 (2.71)	-0.35 (3.93)	0.92	65.35 (2.85)	68.53 (2.91)	-3.18 (4.14)	0.44
Panel B: Mean Earnings, if Any Earnings	Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)	
	\$	\$	Estimate	p-value	\$	\$	Estimate	p-value	\$	\$	Estimate	p-value	\$	\$	Estimate	p-value
<b>Pre-Enrollment</b>																
Two year prior to RA	6,666 (805)	7,909 (1,017)	-1,243 (1,297)	0.35	7,864 (832)	7,216 (949)	648 (1,262)	0.61	9,597 (817)	8,514 (654)	1,083 (1,047)	0.30	7,576 (762)	7,479 (805)	97 (1,108)	0.93
One year prior to RA	6,660 (524)	8,280 (945)	-1,620 (1,081)	0.14	6,030 (607)	5,728 (757)	302 (970)	0.75	7,441 (633)	7,221 (558)	220 (844)	0.79	6,529 (691)	5,084 (563)	1,446 (891)	0.11
<b>Regression Adjusted Impacts</b>																
Year of RA	9,125 (258)	9,383 (258)	-258 (1,161)	0.82	6,885 (210)	7,235 (210)	-350 (846)	0.68	6,744 (153)	7,875 (153)	-1,131 (700)	0.11	6,205 (180)	7,232 (180)	-1,027 (757)	0.18
One year after RA	11,771 (321)	9,658 (321)	2,113 (1,596)	0.19	9,407 (224)	9,366 (224)	41 (-1,002)	0.97	8,898 (177)	8,889 (177)	9 (846)	0.99	7,915 (195)	8,852 (195)	-937 (976)	0.34
Two years after RA	12,162 (417)	11,294 (417)	868 (1,964)	0.66	11,910 (271)	10,141 (271)	1,769 (-1,313)	0.18	11,577 (281)	10,207 (281)	1,370 (1,978)	0.49	9,033 (213)	9,043 (213)	-10 (1,201)	0.99
Panel C: Earnings above Annualized SGA	Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)		Treatment	Control	Difference (T-C)	
	%	%	Estimate	p-value	%	%	Estimate	p-value	%	%	Estimate	p-value	%	%	Estimate	p-value
<b>Pre-Enrollment</b>																
Two year prior to RA	14.29 (3.13)	19.85 (3.50)	-5.56 (4.70)	0.24	11.16 (1.99)	9.31 (1.85)	1.85 (2.72)	0.50	20.28 (2.40)	16.61 (2.19)	3.67 (3.25)	0.26	13.96 (2.13)	13.04 (2.23)	0.92 (3.08)	0.77
One year prior to RA	15.87 (3.27)	22.14 (3.64)	-6.27 (4.89)	0.20	8.37 (1.75)	7.29 (1.66)	1.08 (2.41)	0.65	12.10 (1.95)	12.11 (1.92)	-0.01 (2.74)	1.00	9.81 (1.83)	6.09 (1.58)	3.72 (2.42)	0.13
<b>Regression Adjusted Impacts</b>																
Year of RA	30.03 (3.67)	23.33 (3.29)	6.70 (5.12)	0.19	11.38 (1.65)	12.10 (1.81)	-0.72 (2.48)	0.77	15.36 (1.85)	15.38 (1.87)	-0.02 (2.67)	1.00	13.32 (1.78)	13.07 (2.05)	0.25 (2.77)	0.93
One year after RA	32.70 (3.77)	19.67 (3.29)	13.03 (5.22)	0.01	21.83 (2.41)	18.53 (2.35)	3.30 (3.39)	0.33	20.37 (2.16)	18.06 (2.09)	2.31 (3.05)	0.45	16.89 (2.08)	17.00 (2.43)	-0.11 (3.26)	0.97
Two years after RA	29.05 (3.73)	21.77 (3.53)	7.28 (5.37)	0.18	21.67 (2.35)	17.71 (2.26)	3.96 (3.29)	0.23	21.32 (2.26)	19.14 (2.19)	2.18 (3.18)	0.50	16.39 (2.13)	14.22 (2.29)	2.17 (3.19)	0.50

Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data. See text for detail on calculation method.

**Appendix Table A7. Pre-Enrollment Characteristics, Unadjusted Impacts, and Regression Adjusted Impact Estimates, Connecticut**

	Treatment		Control		Difference (T-C)		
	%	SE	%	SE	Estimate	SE	P-Value
<b>Connecticut</b>							
<b><u>Panel A: Any Earnings (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	71.43	4.04	77.10	3.69	-5.67	5.47	0.30
One Year prior to RA	81.75	3.46	83.97	3.22	-2.22	4.73	0.64
Unadjusted Impacts							
Year of RA	88.00	2.92	83.21	3.28	4.79	4.39	0.28
One year after RA	79.84	3.62	85.50	3.09	-5.66	4.76	0.23
Two years after RA	77.87	3.77	79.53	3.59	-1.66	5.21	0.75
Regression Adjusted Impacts							
Year of RA	86.91	2.56	81.66	2.93	5.25	4.03	0.19
One year after RA	80.29	3.44	84.42	3.19	-4.13	4.83	0.39
Two years after RA	78.69	3.60	77.68	3.68	1.01	5.40	0.85
<b><u>Panel B: Mean Earnings, if any Earnings (\$)</u></b>							
Pre-Enrollment							
Two years prior to RA	6,666	805	7,909	1,017	-1,243	1,297	0.35
One Year prior to RA	6,660	524	8,280	945	-1,620	1,081	0.14
Unadjusted Impacts							
Year of RA	9,033	754	9,475	855	-442	1,140	0.70
One year after RA	11,860	1,175	9,579	1,018	2,281	1,555	0.14
Two years after RA	12,476	1,332	10,999	1,348	1,477	1,895	0.44
Regression Adjusted Impacts							
Year of RA	9,125	258	9,383	258	-258	1,161	0.82
One year after RA	11,771	321	9,658	321	2,113	1,596	0.19
Two years after RA	12,162	417	11,294	417	868	1,964	0.66
<b><u>Panel C: Earnings above annualized SGA (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	14.29	3.13	19.85	3.50	-5.56	4.70	0.24
One Year prior to RA	15.87	3.27	22.14	3.64	-6.27	4.89	0.20
Unadjusted Impacts							
Year of RA	28.80	4.07	24.43	3.77	4.37	5.55	0.43
One year after RA	34.68	4.29	18.32	3.39	16.36	5.47	0.00
Two years after RA	30.33	4.18	19.69	3.54	10.64	5.48	0.05
Regression Adjusted Impacts							
Year of RA	30.03	3.67	23.33	3.29	6.70	5.12	0.19
One year after RA	32.70	3.77	19.67	3.29	13.03	5.22	0.01
Two years after RA	29.05	3.73	21.77	3.53	7.28	5.37	0.18

Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data. See text for detail on calculation method.

**Appendix Table A8. Pre-Enrollment Characteristics, Unadjusted Impacts, and Regression Adjusted Impact Estimates, Utah**

	Treatment		Control		Difference (T-C)		
	%	SE	%	SE	Estimate	SE	P-Value
<b>Utah</b>							
<b><u>Panel A: Any Earnings (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	49.40	3.16	48.18	3.19	1.22	4.49	0.79
One Year prior to RA	56.57	3.13	53.85	3.18	2.72	4.46	0.54
Unadjusted Impacts							
Year of RA	66.14	2.99	64.90	3.06	1.24	4.28	0.77
One year after RA	68.00	2.96	67.62	3.00	0.38	4.21	0.93
Two years after RA	58.63	3.13	61.16	3.14	-2.53	4.43	0.57
Regression Adjusted Impacts							
Year of RA	65.36	2.77	65.68	2.73	-0.32	3.91	0.94
One year after RA	67.27	2.86	68.35	2.81	-1.08	4.03	0.79
Two years after RA	57.64	3.00	62.11	2.96	-4.47	4.24	0.29
<b><u>Panel B: Mean Earnings, if any Earnings (\$)</u></b>							
Pre-Enrollment							
Two years prior to RA	7,864	832	7,216	949	648	1,262	0.61
One Year prior to RA	6,030	607	5,728	757	302	970	0.75
Unadjusted Impacts							
Year of RA	7,012	626	7,102	653	-90	905	0.92
One year after RA	9,594	764	9,174	708	420	1,042	0.69
Two years after RA	12,143	1,051	9,910	818	2,233	1,332	0.09
Regression Adjusted Impacts							
Year of RA	6,885	210	7,235	210	-350	846	0.68
One year after RA	9,407	224	9,366	224	41	1,002	0.97
Two years after RA	11,910	271	10,141	271	1,769	1,313	0.18
<b><u>Panel C: Earnings above annualized SGA (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	11.16	1.99	9.31	1.85	1.85	2.72	0.50
One Year prior to RA	8.37	1.75	7.29	1.66	1.08	2.41	0.65
Unadjusted Impacts							
Year of RA	11.55	2.02	11.02	2.00	0.53	2.84	0.85
One year after RA	22.80	2.66	17.62	2.44	5.18	3.61	0.15
Two years after RA	22.49	2.65	16.94	2.42	5.55	3.59	0.12
Regression Adjusted Impacts							
Year of RA	11.38	1.65	12.10	1.81	-0.72	2.48	0.77
One year after RA	21.83	2.41	18.53	2.35	3.30	3.39	0.33
Two years after RA	21.67	2.35	17.71	2.26	3.96	3.29	0.23

Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data. See text for detail on calculation method.

**Appendix Table A9. Pre-Enrollment Characteristics, Unadjusted Impacts, and Regression Adjusted Impact Estimates, Vermont**

	Treatment		Control		Difference (T-C)		
	%	SE	%	SE	Estimate	SE	P-Value
<b>Vermont</b>							
<b><u>Panel A: Any Earnings (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	64.06	2.87	65.40	2.80	-1.34	4.01	0.74
One Year prior to RA	61.57	2.91	65.74	2.80	-4.17	4.04	0.30
Unadjusted Impacts							
Year of RA	72.30	2.69	74.65	2.57	-2.35	3.72	0.53
One year after RA	70.91	2.74	72.82	2.63	-1.91	3.80	0.61
Two years after RA	63.14	2.92	63.51	2.86	-0.37	4.09	0.93
Regression Adjusted Impacts							
Year of RA	72.73	2.40	74.26	2.36	-1.53	3.42	0.66
One year after RA	71.36	2.56	72.39	2.50	-1.03	3.62	0.78
Two years after RA	63.15	2.77	63.50	2.71	-0.35	3.93	0.92
<b><u>Panel B: Mean Earnings, if any Earnings (\$)</u></b>							
Pre-Enrollment							
Two years prior to RA	9,597	817	8,514	654	1,083	1,047	0.30
One Year prior to RA	7,441	633	7,221	558	220	844	0.79
Unadjusted Impacts							
Year of RA	6,764	451	7,856	566	-1,092	724	0.14
One year after RA	8,894	614	8,894	618	0	871	1.00
Two years after RA	11,585	1,837	10,200	728	1,385	1,976	0.48
Regression Adjusted Impacts							
Year of RA	6,744	153	7,875	153	-1,131	700	0.11
One year after RA	8,898	177	8,889	177	9	846	0.99
Two years after RA	11,577	281	10,207	281	1,370	1,978	0.49
<b><u>Panel C: Earnings above annualized SGA (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	20.28	2.40	16.61	2.19	3.67	3.25	0.26
One Year prior to RA	12.10	1.95	12.11	1.92	-0.01	2.74	1.00
Unadjusted Impacts							
Year of RA	15.11	2.15	15.63	2.14	-0.52	3.03	0.86
One year after RA	20.36	2.43	18.12	2.28	2.24	3.33	0.50
Two years after RA	19.71	2.41	17.89	2.27	1.82	3.31	0.58
Regression Adjusted Impacts							
Year of RA	15.36	1.85	15.38	1.87	-0.02	2.67	1.00
One year after RA	20.37	2.16	18.06	2.09	2.31	3.05	0.45
Two years after RA	21.32	2.26	19.14	2.19	2.18	3.18	0.50

Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data. See text for detail on calculation method.

**Appendix Table A10. Pre-Enrollment Characteristics, Unadjusted Impacts, and Regression Adjusted Impact Estimates, Wisconsin**

	Treatment		Control		Difference (T-C)		
	%	SE	%	SE	Estimate	SE	P-Value
<b>Wisconsin</b>							
<b><u>Panel A: Any Earnings (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	63.40	2.96	62.17	3.20	1.23	4.36	0.78
One Year prior to RA	60.38	3.01	67.83	3.09	-7.45	4.31	0.09
Unadjusted Impacts							
Year of RA	77.19	2.59	73.80	2.91	3.39	3.90	0.38
One year after RA	78.33	2.55	75.33	2.87	3.00	3.84	0.43
Two years after RA	66.15	2.94	67.70	3.12	-1.55	4.29	0.72
Regression Adjusted Impacts							
Year of RA	78.05	2.28	72.77	2.60	5.28	3.51	0.13
One year after RA	78.74	2.37	74.90	2.65	3.83	3.61	0.29
Two years after RA	65.35	2.85	68.53	2.91	-3.18	4.14	0.44
<b><u>Panel B: Mean Earnings, if any Earnings (\$)</u></b>							
Pre-Enrollment							
Two years prior to RA	7,576	762	7,479	805	97	1,108	0.93
One Year prior to RA	6,529	691	5,084	563	1,446	891	0.11
Unadjusted Impacts							
Year of RA	6,310	476	7,105	650	-795	805	0.31
One year after RA	8,186	565	8,525	824	-339	999	0.73
Two years after RA	9,235	743	8,816	915	419	1,179	0.72
Regression Adjusted Impacts							
Year of RA	6,205	180	7,232	180	-1,027	757	0.18
One year after RA	7,915	195	8,852	195	-937	976	0.34
Two years after RA	9,033	213	9,043	213	-10	1,201	0.99
<b><u>Panel C: Earnings above annualized SGA (%)</u></b>							
Pre-Enrollment							
Two years prior to RA	13.96	2.13	13.04	2.23	0.92	3.08	0.77
One Year prior to RA	9.81	1.83	6.09	1.58	3.72	2.42	0.13
Unadjusted Impacts							
Year of RA	14.45	2.17	11.79	2.14	2.66	3.05	0.39
One year after RA	18.25	2.39	15.42	2.40	2.83	3.39	0.41
Two years after RA	17.31	2.35	13.27	2.26	4.04	3.26	0.22
Regression Adjusted Impacts							
Year of RA	13.32	1.78	13.07	2.05	0.25	2.77	0.93
One year after RA	16.89	2.08	17.00	2.43	-0.11	3.26	0.97
Two years after RA	16.39	2.13	14.22	2.29	2.17	3.19	0.50

Source: Authors' calculations using SSA administrative data linked to BOPD enrollment data. See text for detail on calculation method.

**Appendix Table A11. Sample Sizes for Calculations**

	<b>Treatment</b>	<b>Control</b>	<b>Total (T+C)</b>
<b>Total</b>			
Prior to random assignment (RA)	923	897	1820
Year of RA	917	893	1810
One year after RA	912	889	1801
Two years after RA	905	880	1785
<b>With earnings (Panel C)</b>			
Two years prior to RA	562	552	1114
One year prior to RA	578	589	1167
Year of RA	680	652	1332
One year after RA	670	657	1327
Two years after RA	586	583	1169
<b>Connecticut</b>			
Prior to RA	126	131	257
Year of RA	125	131	256
One year after RA	124	131	255
Two years after RA	122	127	249
<b>With earnings (Panel C)</b>			
Two years prior to RA	90	101	191
One year prior to RA	103	110	213
Year of RA	110	109	219
One year after RA	99	112	211
Two years after RA	95	101	196
<b>Utah</b>			
Prior to RA	251	247	498
Year of RA	251	245	496
One year after RA	250	244	494
Two years after RA	249	242	491
<b>With earnings (Panel C)</b>			
Two years prior to RA	124	119	243
One year prior to RA	142	133	275
Year of RA	166	159	325
One year after RA	170	165	335
Two years after RA	146	148	294
<b>Vermont</b>			
Prior to RA	281	289	570
Year of RA	278	288	566
One year after RA	275	287	562
Two years after RA	274	285	559
<b>With earnings (Panel C)</b>			
Two years prior to RA	180	189	369
One year prior to RA	173	190	363
Year of RA	201	215	416
One year after RA	195	209	404
Two years after RA	173	181	354

(continued on next page)

**Appendix Table A11. Sample Sizes for Calculations, continued**

	<b>Treatment</b>	<b>Control</b>	<b>Total (T+C)</b>
<b>Wisconsin</b>			
Prior to RA	265	230	495
Year of RA	263	229	492
One year after RA	263	227	490
Two years after RA	260	226	486
<b>With earnings (Panel C)</b>			
Two years prior to RA	168	143	311
One year prior to RA	160	156	316
Year of RA	203	169	372
One year after RA	206	171	377
Two years after RA	172	153	325