APPENDIX D

METHODOLOGICAL APPROACH TO ESTIMATING THE IMPACT OF TICKET TO WORK

A. OVERVIEW

This appendix summarizes our methodological approach to estimating the impact of the TTW program during the initial stages of rollout (through 2003). The program was rolled out to Phase 1 states in 2002 and to Phase 2 states in 2003.[1] Our impact estimates represent outcomes from the early stages of implementation.

Our preferred approach is based on a model that captures the impacts of TTW over its first two years of implementation. We include longitudinal data for a single cohort of 4.7 million beneficiaries covering the period from the year before the Phase 1 Ticket mailing in 2001 and continuing through the end of 2003. Our model uses temporal variation in the availability of Ticket in two ways. First, it uses pre-post rollout variation within the Phase 1 and 2 states (that is, states in which the rollout occurred in these years). Second, it compares pre-post variation within Phase 1 and 2 states to contemporaneous pre-post variation within states where the rollout had not yet occurred. In both years, the comparison states include the Phase 3 states, and the TTW states include the Phase 1 states. The role of the Phase 2 states in generating impacts changes from 2002 to 2003 because the program was rolled out in these states in 2003. Hence, in 2002, the Phase 2 states are comparison states, but in 2003 they are TTW states.

We present impact estimates for the core outcomes described in Chapter XIII that include SVRA-only service enrollment, total (SVRA and EN) service enrollment, earnings, and benefit amounts, as well as three supplemental outcomes not reported in the text that include annual employment, any annual benefit receipt, and left cash benefits due to work.

[1] Phase 1 states include Arizona, Colorado, Delaware, Florida, Illinois, Massachusetts, New York, Oklahoma, Oregon, South Carolina, Vermont, Wisconsin, and Iowa. Phase 2 states include Alaska, Connecticut, New Hampshire, New Jersey, Washington DC, Virginia, Georgia, Kentucky, Mississippi, Montana, Tennessee, Indiana, Michigan, Arkansas, Louisiana, New Mexico, Kansas, Missouri, North Dakota, South Dakota, and Nevada. Phase 3 states include Maine, Rhode Island, Maryland, Pennsylvania, West Virginia, Alabama, North Carolina, Minnesota, Ohio, Texas, Nebraska, Utah, Wyoming, California, Hawaii, Idaho, and Washington.

We are most likely to observe impacts on the core outcomes, especially total service enrollment, if the TTW program succeeded in creating a new market for employment services. The supplemental outcomes represent a more restrictive set of outcomes that requires a change in employment status (i.e., from no work to some work during a year) or benefit receipt (i.e., from benefit receipt to no benefit receipt) and, hence, change less frequently relative to the core outcomes above.

TTW appeared to have a small impact on promoting service enrollment during the first year of rollout. Our upper-bound estimates indicate that TTW increased service enrollment by up to 0.4 percentage points. Using a more restrictive set of assumptions for service enrollment, we obtain a lower-bound estimate of Ticket's impact of 0.1 percentage point. We find no compelling evidence of TTW affecting beneficiary earnings and benefits during the program's first two years. Our results show that mean earnings in Phase 1 states were increasing relative to Phase 2 and 3 states before TTW rollout and those trends persisted after rollout. Hence, although it is possible that relative trends in these variables after the rollout were partly or even completely caused by the rollout itself, it seems highly likely that the environmental factors behind the earlier relative trends explain a substantial share of the relative changes after the rollout—perhaps all of it. The relative trends for these variables observed before TTW rollout are consistent with SSA's selection of Phase 1 states based on their readiness for TTW rollout. In contrast, we do not find relative trends in service enrollment before TTW rollout, giving us confidence that the enrollment estimates represent TTW impacts. We speculate that relative trends in service enrollment are less sensitive than relative trends in earnings and benefits to state policy and economic changes. The effects of TTW on the three supplemental outcomes are all small and/or statistically insignificant.

Section B describes the initial approach for estimating impacts outlined in the Ticket evaluation design report by Stapleton and Livermore (2002). We use the suggestions in the design report to inform our selection of the econometric model for estimating impacts as well as to outline other potential approaches for estimating impacts. Section C provides an overview of the longitudinal research file created by MPR to conduct the TTW evaluation, using SSA program and earnings records and RSA administrative files, including our sample selection criteria and definition of key outcomes. This discussion is helpful for understanding the structure of the administrative data, which will likely become a valuable source of information for future SSA program evaluation projects. Section D provides full derivation of the econometric model used to estimate impacts, which is important in identifying all of the sources of variation captured in our approach and in motivating a set of sensitivity tests that we apply to our impact estimates. Section E provides a detailed summary of findings for each of our econometric specifications, including a summary of impact estimates and sensitivity tests. Finally, Section F summarizes the alternative approaches considered in the impact analysis based on the original suggestions outlined in the design report. We briefly describe our rationale for not using these models to generate impacts, discussing their limitations relative to our preferred approach.

B. BACKGROUND ON APPROACHES CONSIDERED IN ESTIMATING IMPACTS

The design report by Stapleton and Livermore (2002) summarized a general approach to estimating impacts using SSA and RSA administrative data by comparing outcomes for TTW eligibles and participants with outcomes for similar beneficiaries in states where the Ticket has not yet been implemented. Longitudinal SSA and RSA administrative data were the only feasible source of information for estimating impacts given the absence of pre–TTW survey data and the prohibitive costs of collecting enough survey data to identify meaningful contemporaneous differences in outcomes across states.

The general approach for estimating impacts addresses SSA's top evaluation priority--to assess whether TTW significantly reduces dependence on SSA benefits through increased earnings. If TTW is to achieve its objectives, it must increase the enrollment of eligible beneficiaries in employment services, which should subsequently translate into higher earnings and lower DI and SSI benefit amounts. Initial impacts should occur first on enrollment in services as beneficiaries assign their Ticket and/or become more aware of employment service options in their area. Any impacts on earnings and, especially, benefits are expected to take longer to emerge; earnings increases are not likely to occur for some time after Ticket assignment because it may take some time for those who assign a Ticket to find employment, and DI benefits will not be reduced until earnings have exceeded the SGA level for a period that can be as long as 12 months.

Within their general approach, Stapleton and Livermore proposed the three approaches listed below for estimating impacts of TTW on both participants and all eligibles. These approaches exploit variation over time in TTW rollout and across states in the three phases of program implementation.

- ***Within-State Contemporaneous Comparisons of Participant Outcomes.*** Contemporaneous comparisons of outcomes for TTW participants to contemporaneous outcomes for selected non-participants in the same state.

- ***Within-State Pre-Post Comparisons.*** Comparisons of outcomes for beneficiaries in the period after TTW rollout to outcomes for beneficiaries in the same state before TTW rollout.

- ***Across-State Contemporaneous Comparisons of Changes in Outcomes.*** Contemporaneous comparisons of changes in beneficiary outcomes in the early-implementation states to corresponding changes in matched late-implementation states, especially during the period from rollout in the early-implementation states to rollout in the late-implementation states.

Stapleton and Livermore argued for testing several comparison groups to examine the sensitivity of impact findings, which is important in a non-experimental framework in which other factors, especially changes in state policy and the economic environment, could influence key TTW outcomes. They indicated that the models should carefully control for observed differences in beneficiary characteristics and compare findings across subgroups of beneficiaries defined by their likely participation in TTW.

The impact evaluation team has since refined the original design outlined in Stapleton and Livermore in consultation with SSA. Our early work in the project specified several opportunities for estimating impacts based on the three approaches by using alternative model specifications for TTW-eligibles and TTW participants as well as for subgroups within each of these beneficiary groups (see Fraker and Stapleton, 2004).

We determined that the strongest approach was to incorporate the pre-post and contemporaneous comparisons of outcomes into a longitudinal fixed effects model to track outcomes for a single beneficiary cohort of Ticket-eligibles before and after TTW rollout. We assumed that TTW might affect all Ticket-eligibles regardless of whether they assign their Ticket and participate in the program. That is, TTW might be associated with general changes in attitudes of SSA staff, participants, and providers regarding return-to-work activities for disability beneficiaries as well as with changes in SSA administrative procedures related to beneficiary earnings and employment. Our process findings from the first two reports indicate that TTW did have some effect in changing the culture in providing return-to-work service in ways that likely affect non-participants as well as participants.

The strategy incorporates and builds on the ideas in the Stapleton and Livermore design report while providing a framework within which methodological decisions are apparent. We measure impacts as the differences in the values of the outcome measures for the treatment group (beneficiaries who were eligible for TTW and were living in states where TTW had already been rolled out) and the contemporaneous values for the comparison group (beneficiaries who were eligible for TTW but were living in states where the program had not yet been rolled out), after controlling for characteristics in the pre-rollout year.

The model uses data for a 2001 cohort of beneficiaries for whom we track changes in outcomes through 2002 and 2003 and compare changes across the different phases of the rollout schedule (Appendix Exhibit D.1). During this period, some states had implemented TTW (Phase 1 states in 2002 and 2003 and Phase 2 states in 2002), and some had not (Phase 2 states in 2002 and Phase 3 states in 2002 and 2003) (Exhibit XIII.3).

It is important to note that our model primarily captures changes in Phase 1 states relative to the remaining states. To the extent that the impacts of TTW vary across the states included in each phase, our confidence in extrapolating the Phase 1 impact estimates to the other rollout phases is diminished. Moreover, the generalizability of the Phase 1 results could be compromised if TTW was rolled out differently in Phase 2 or Phase 3 states.

As discussed in more detail in Section F, we also considered other approaches for estimating impacts that are variants of the approaches in the Stapleton and Livermore report, including participant comparisons and alternative pre-post and contemporaneous comparison models that incorporated several cohorts of beneficiaries. However, for two reasons, these approaches were less feasible than originally envisioned in the Stapleton and Livermore report. First, Ticket participation rates, which our first report showed as less than 1 percent in Phase 1 states, were much lower than the 5 percent participation rate assumed in the design report by Stapleton and Livermore. Second, the TTW program rolled out during a period of economic recession and large SSA caseload growth, posing difficulties in making comparisons across several cohorts. For these reasons, we determined that the fixed

effects longitudinal model would be best suited for producing credible impact estimates relative to the alternative options during the early rollout periods covered in our data.

C. DATA DESCRIPTION

We developed a multiyear longitudinal file for the purpose of generating impact estimates that includes administrative data from several SSA and RSA administrative data sources. We created a single multiyear longitudinal analysis file by using three data sources: the Ticket Research File, which contains data from several SSA files on DI and SSI beneficiaries; SSA's Summary Earnings Records (SER) file; and a file on closed SVRA cases maintained by RSA.

We selected a sample of DI and SSI beneficiaries between age 18 and 57 in January 2001 (one year before Ticket rollout) whose outcomes we tracked through 2003. We included in our sample only beneficiaries who met the Ticket eligibility requirements once the program was implemented in their state following rollout. Given our expectation that impacts would vary with age and program groups based on our findings in the participation analysis (Chapter III), we stratified the sample by nine program-age subgroups.

We estimated TTW impacts for the core outcome measures—SVRA-only service enrollment, total service enrollment, annual earnings, and annual benefit amounts--that are reported in Chapter XIII. We also estimated TTW impacts for three supplemental measures that captured a more restrictive measure of the core earnings and benefit outcomes requiring a change in overall benefit and/or employment status. The supplemental measures include annual employment, any annual benefit receipt, and left cash benefits due to work.

One important limitation of the longitudinal data file is that core service enrollment measures from RSA data are available only through 2002 (the first rollout year), whereas the core and supplemental employment, earnings, and benefit variables are available through 2003 (the second rollout year). The amount of information on service enrollment was limited because of a two-year lag in obtaining case closure information for SVRA cases. At the time of the analysis, we had SVRA data through calendar year 2004 such that we could confidently use the file to identify nearly all SVRA participants only through 2002. In contrast, the lag in obtaining SSA earnings and benefit amount outcomes was shorter, allowing us to estimate impacts for these outcomes through 2003.

The implication is that we can estimate TTW impacts on all outcomes in the year of Ticket mailing and on selected outcomes (earnings, benefit amounts, and each of the supplemental outcomes) in the year after Ticket mailing. Below, we provide a brief description of the three data sources for the longitudinal file, describe our sample selection for the impact analysis, and present descriptive statistics on each of the outcomes measures.

1. Ticket Research File (TRF) Includes SSA Program and TTW Participation Information

The TRF is an analytic file constructed by MPR to support the research needs of the TTW evaluation. It contains longitudinal and one-time administrative program data on approximately 16 million beneficiaries between age 18 and 64 who participated in SSI or DI programs at any time between 1994 and 2004. The data are housed on the mainframe computer at SSA's data center and are available on a restricted basis.

The data are culled from various SSA files, including:

- Disabled Beneficiaries and Dependents (DBAD) and Master Beneficiary Record14 (MBR14), which includes information on DI beneficiaries characteristics, payments and address information

- Quarterly ZIP files, which provide historical snapshots of MBR; the files save information about previous places of residence because MBR address information is over-written when new information is obtained

- SSI Longitudinal file, which provides information about SSI receipt and payments from the Supplemental Security Record (SSR)

- REMICS and SORD files, which record historical snapshots of SSR for retaining information on earlier use of SSI work incentives and previous places of residence

- NUMIDENT file, which provides information about beneficiary deaths

- Disability 831/832/833 files, which include information on disability determinations and other characteristics, such as education (information on continuing disability reviews also can be obtained from DBAD for DI beneficiaries)

- Integrated Data Management System (formerly called the Disability Control File or DCF), which includes information on participation in the TTW program and other earnings and post-entitlement actions

- Vocational Rehabilitation Reimbursement Management System (VRRMS), which includes data on payments that SSA has made to state vocational rehabilitation agencies for the purpose of assisting beneficiaries in returning to work

MPR staff worked with SSA staff to link these files across systems and to produce TRF for use in this evaluation. The longitudinal TRF variables include monthly benefit payments, program eligibility, EN service enrollment, state of residence, and disability diagnosis codes. The one-time variables include SSN, date of birth, and race/ethnicity. Data from SSI and

DI sources are combined in a single TRF record per beneficiary. Hildebrand, Loewenberg, and Phelps (2005) provide full documentation for TRF.

2. Summary Earnings Records File (SER) Includes Annual Earnings Information

We supplemented the program information in TRF with information on annual earnings by using data from SER. We accessed SER by following protocols developed by SSA and MPR staff that allowed our team to include analyses of earnings trends that would otherwise have been impossible to conduct or would have required substantial effort on the part of SSA staff. Contractors do not have direct access to SER because the Internal Revenue Service (IRS) collects the data, which are then subject to IRS access rules rather than SSA access rules. A formal agreement between IRS and SSA authorizes the linking of SER and SSA data for the TTW evaluation. The agreement stipulates that non-SSA evaluation staff would not have direct access to SSN-identified linked data and that the data would remain in a secure site in an SSA facility.

SER provides person-level annual data on Social Security–taxable earnings, with one record in the file for each person. IRS wage records are the primary source of information for SER. A record contains the annual FICA earnings amount for each year from 1937 to the present.

3. Rehabilitation Services Administration 911 Data (RSA-911) Includes Information on SVRA Service Enrollment

To obtain information on use of SVRA services, we included data from the RSA-911 Case Service Report, a data file containing information on all closed SVRA cases. RSA updates the file annually to include an additional record for each SVRA case that closed during the most recently completed federal fiscal year. An individual may receive SVRA services repeatedly over a lifetime, resulting in several case records in the file. A record includes the individual's Social Security number (SSN) and information on his or her disability characteristics, services, health insurance, and employment.

Through a formal data-sharing agreement between SSA and RSA, MPR obtained a 100 percent extract of the RSA-911 file containing records for SVRA cases closed in fiscal years 1997 through 2004 (October 1997 through September 2004).

For purposes of the TTW impact analysis, the key data elements in an RSA-911 record are the date of SVRA eligibility determination and the date of case closure. These two pieces of information allow us to create a complete timeline of eligibility by disability beneficiaries for SVRA services during the period covered by the RSA data.

4. Research File for TTW Impact Estimates

The longitudinal analysis file contains annual individual-level data on 24 variables from the three data sources described above. An SSI or DI beneficiary was included in the file if the following criteria were satisfied:

- The individual would have been eligible for benefits in at least one month between January 1996 and December 2004 if TTW had been in effect throughout that period. Ticket eligibility is defined as a DI or SSI beneficiary in current pay status who is not classified as Medical Improvement Expected (MIE) and is not a former child SSI recipient awaiting an adult redetermination.

- The individual was 18years old or older on January 1, 2004.

- The individual was alive and less than 65 years old on December 31, 2004.

Some 9.6 million beneficiaries satisfied these initial criteria, each of whom has at least one record in the longitudinal file. The file contains a record for each year, 1996 through 2004, for each beneficiary who was at least 18 years old on January 1. The file contains 83,898,010 records.

Most of the variables in the longitudinal analysis file come from TRF and include date of birth, gender, race and ethnicity, years of education, date of first eligibility for disability benefits, primary disabling condition, annual combined DI and SSI benefit, DI and SSI payment status, whether the beneficiary left cash benefits due to work, and Ticket mailing and assignment dates. The one RSA data variable is an indicator of eligibility for SVRA services or of an actively assigned Ticket at any time during the year. The analysis file does not include a measure of annual earnings from SER, but protocols consistent with data security requirements were developed by SSA staff to link that variable temporarily to the file for specific analyses.

Our sample for the impact estimates includes beneficiaries from the longitudinal analysis file who were between age 18 and 57 in January 2001. We excluded older beneficiaries because they generally had low TTW participation rates during rollout relative to younger beneficiaries and relatively fewer prospects for using TTW to return to work. For example, Chapter III reports that beneficiaries age 18 to 24 were 5.7 times more likely to participate than those age 55 or older. We track outcomes for the younger cohort through the end of 2003. We imposed the age restriction to ensure that beneficiaries in our sample were under age 60 through the end of the observation period and, presumably, far enough away from retirement age to benefit from TTW. Hence, we assume the impact for the population who were over age 58 in 2001 was zero.[2] We will verify this assumption in future reports.

With all of our outcomes measured in annual terms, we also exclude new beneficiaries who started receiving benefits in calendar year 2001. This restriction allows for full comparisons of annual outcomes since calendar year 2001. In addition, it is difficult to assign base-year earnings and benefit amounts for new beneficiaries. For example, it is likely that many new beneficiaries, especially DI beneficiaries, will have reported at least some

[2]In future reports, we could test this restriction. However, the size of the beneficiary subgroup over age 57 is large, making the costs of generating impacts for this subgroup particularly costly.

annual earnings according to SER, but we cannot determine what portion of the earnings came before versus after benefit receipt (or before or after the onset of disability). New beneficiaries could have received substantial base-year earnings before program enrollment, which could introduce a downward bias in estimates of the earnings impacts of TTW in later years. In future reports, we can relax these assumptions about base-year earnings so as to develop impact estimates for new beneficiaries by using modified versions of the outcome measures.

As discussed in more detail in Section D, we also included data from earlier cohorts to generate sensitivity tests for our impact estimates. We tracked outcomes for these cohorts based on the criteria noted above. We conducted sensitivity tests with samples from the 1999 cohort (1999–2001), 1998 cohort (1998–2000), 1997 cohort (1997–1999), and 1996 cohort (1996–1998).

5. Impacts Estimated for Program-Age Groups

Based on our selection criteria, the impact analysis sample includes 4.7 million beneficiaries. We stratified the sample by nine program-age groups to allow for projected differences in outcomes across age and, to a lesser extent, program titles. The stratification is consistent with our findings of differences in participation rates across age and, to a lesser extent, program groups in the participation analysis in Chapter III. The age categories are 18 to 39, 40 to 49, and 50 to 57. The Title groups, which are mutually exclusive, are DI-only, SSI-only, and concurrent (DI and SSI) beneficiaries. The concurrent group includes beneficiaries who received DI and SSI benefits at some time during the course of the base year and serial beneficiaries who receive first SSI and then DI in the course of a year (defined as 2001 for the impact analysis). The SSI-only group includes only beneficiaries who received SSI during the year. Finally, the DI-only group includes only beneficiaries who received DI during the year. The sample sizes for each program-age group are particularly large, ranging from a minimum of 193,000 (concurrent beneficiaries age 50 to 57) to 1.1 million (DI-only beneficiaries age 50 to 57).

6. Core Outcomes Included in Impact Analysis

We assessed the TTW's impact on annual measures of:

- SVRA-only service enrollment
- Total (SVRA and EN) service enrollment- upper bound
- Total service enrollment- lower bound
- Benefit amounts
- Earnings

The SVRA-only measure was of interest to assess whether the Ticket had any impact in either inducing or crowding out SVRA enrollment by beneficiaries. This impact could be negative because some beneficiaries who, under TTW, only receive services from ENs after the rollout would have enrolled for services at an SVRA in the absence of TTW. It could be positive if TTW stimulated enrollment at SVRAs. The estimate of the impact on SVRA enrollment might also be downward biased if the TTW rollout increased the number of Phase 1 SVRA enrollees who were not included in the RSA data available for the analysis because their cases were still open.

The first total service enrollment measure (upper bound) captured SVRA and EN participation as measured in the RSA-911 and/or TRF data files. This measure included beneficiaries who had assigned their Ticket or had an open SVRA case sometime during the course of that calendar year. It addressed a limitation of the SVRA-only measure by capturing impacts on the private rehabilitation market through the inclusion of EN service enrollment information. In years before the TTW rollout in a phase group, a beneficiary was counted as enrolled for services in a calendar year only if the beneficiary had an open case at an SVRA in at least one month as measured in the RSA-911 data. In the first rollout year for Phase 1 (calendar 2002), a beneficiary was considered to be enrolled for services if, in at least one month, the beneficiary had an open SVRA case and/or has a Ticket assigned to an EN or SVRA as measured in the RSA-911 and/or TRF data files.

We refer to impact estimates using this first total service enrollment measure as an "upper bound" because we were concerned that it included an upward bias related to a change in the methods used to account for SVRA and, to a lesser extent, non-SVRA participants after the Ticket rollout. In 2002, Phase 1 beneficiaries enrolled for services under a Ticket assignment to an SVRA would be counted as enrolled in the TRF even if their SVRA case had not closed, whereas before the rollout, only closed cases are counted. Thus, this total service enrollment impact estimates might capture increases in measured enrollment that reflects only changes in measurement that coincided with the TTW rollout. It might also miss some beneficiaries who used non-SVRA rehabilitation service providers before the rollout in each phase. However, we believe the bias associated with non-SVRA participation is minimal based on a finding from our process analysis that suggests that the vast majority of ENs had not served beneficiaries prior to the TTW rollout, except possibly under contract to provide services to SVRA clients (Thornton et al. 2004).

To address this potential upward bias, we created a second total service enrollment variable (lower bound) that measured SVRA participation using the SVRA-only measure and added in the proportion of Phase 1 beneficiaries who had assigned a Ticket to an EN during at least one month in 2002.[3] We use this measure to generate a "lower bound" impact estimate because it assumed that, if anything, the SVRA-only estimates had a downward bias, and the non-SVRA providers rarely gave services to beneficiaries except under contract to

[3] Unlike the upper bound measure, the lower bound measure did not include open SVRA participants measured in the TRF file in any month of 2002.

SVRAs. Our qualitative findings from the first Ticket evaluation report suggest that this assumption is reasonable (Thornton et al. 2004).

The earnings measure came from SER and the benefit amount measure from TRF. Both variables were topcoded at the 99.5 percentile values. This restriction was more important for the benefit amount variable because some beneficiaries receive substantial retroactive payments during the course of the year that can make their annual benefit amounts large.

Our constructed benefit amount variable includes the sum of the federal SSI amount paid and the DI benefit amount due. The amount paid represents the benefit actually received by the beneficiary in a particular month and the amount due is the amount that SSA is scheduled to pay the beneficiary. The two amounts can differ if there are changes in the beneficiary's status. For example, if SSA retroactively has adjusted a beneficiary's record for an overpayment due to excess earnings, the amount due will be less than the amount paid. In later months, collection of overpayments will reduce amounts paid relative to amounts due.

We would have preferred to use the amount paid variables for both SSI and DI, because the amount paid accurately captures SSA's benefit cost experience. At the time of our analysis, however, the DI benefit amount paid was not available. The implication for the measurement of this outcome is likely limited given that there generally are only relatively small differences between the amount paid and amount due variables in DI.[4] The differences between amount due and amount paid are larger for SSI beneficiaries because, unlike DI, the benefit offset schedule reduces benefit amounts for lower levels of earnings. We will include the amount paid and due fields for both SSI and DI in future TRF extracts. For future reports, we plan to estimate impacts on benefit amounts using the amount paid and amount due fields to test whether substantial differences exist.

We also modified the benefit amount variable so that its values in 2002 and 2003 were fixed at 2001 levels unless the beneficiary was employed at some time during the analysis period. This adjustment was necessary because benefit amounts can vary for several administrative reasons. For example, DI and SSI benefits may fluctuate if a beneficiary's check was reduced as a consequence of a previous overpayment or a change in living arrangement. Because we do not have enough information to identify all the reasons for administrative changes in benefit checks, we control for this variation by allowing benefit amounts to change only when a person has reported earnings from SER. This eliminates annual variation in benefit amounts for those with no earnings in any sample year as a source of estimation error.

[4] New beneficiaries are an exception, because in their award month they often receive a retroactive payment for earlier months of DI eligibility. As discussed in Section C.4, we excluded new beneficiaries from our analysis. The timing could also be an issue because benefits due can be retroactively adjusted while benefits paid is not generally retroactively adjusted.

7. Three Supplemental Outcomes in Impact Analysis

We examined three supplemental outcome measures—annual employment status, annual benefit receipt, and an indicator from SSA administrative records of beneficiaries who left the DI and SSI programs because of work ("left benefits due to work"). These measures are more restrictive than the core measures of benefit and earnings outcomes. For this reason, we expected the impacts on the supplemental measures to be smaller than those on core annual earnings and benefit amount measures. The annual employment status measure came from SER and was defined as any earnings during the calendar year. The annual benefit receipt and left-rolls-due-to-work measures came from TRF. The annual benefit receipt measure was defined as the receipt of any DI or SSI benefits in the calendar year. The left-rolls-due-to-work measure is an SSA-defined concept that identifies beneficiaries who leave DI or SSI for a full year because of work.

8. Descriptive Statistics on Core and Supplemental Outcome Measures

Exhibit D.2 summarizes the core and supplemental outcome measures for the sample during the period of our analysis, defined as calendar years 2001 through 2003. The summary consists of a brief definition followed by mean values across each of the nine program-age groups. For all measures except service enrollment, the values are averaged over the three years for the impact analysis (2001–2003). In addition, all dollar-denominated values were adjusted for inflation to reflect January 2004 real dollars.

D. ECONOMETRIC MODEL

Our longitudinal fixed effects model for estimating impacts has been commonly used to estimate non-experimental impacts in the econometrics literature (Wooldridge 2002 Chapter 10). Our model identifies TTW program impacts through variation in the outcomes at the individual and state levels as well as variation over time. A key to identifying TTW impacts is disentangling the effects of the TTW program from other state programmatic and economic changes.

We present a full derivation of our model to illustrate our assumptions for generating impact estimates. The derivation is important in identifying all sources of variation captured in our impact estimates, including potential confounding state programmatic and economic factors that could bias our estimates. We use the derivation to specify a general econometric model for generating impacts and to motivate our sensitivity tests of the assumptions underlying the model. We test these assumptions by using sensitivity tests originally proposed by Heckman and Hotz (1989), where we apply our econometric model to earlier cohorts of beneficiaries when TTW was not available. If our assumptions are valid, the estimated coefficient on our treatment indicator should be zero during the periods before rollout given that the TTW program did not exist.

1. Derivation of Longitudinal Fixed Effects Model for Estimating Impacts

The regression model for estimating net TTW impacts can be summarized using the following general specification:

Equation (1) $$Y_{isy} = \alpha + \lambda T_{sy} + \gamma M_i + \omega N_{iy} + \sigma P_s + \delta X_{sy} + c_y + d_i + e_{iy} + h_s + k_{sy} + u_{isy}$$

where,

Y_{isy} = outcome variable (service enrollment, earnings, benefit amounts, employment, benefit receipt, and left due to work) for individual *i* , in state *s*, during year *y*

T_{sy} = TTW treatment indicator in state *s* in year *y*

M_i = time invariant observed characteristics for individual *i*

N_{iy} = time variant observed characteristics for individual *i* during year *y* (such as education)

P_s = time invariant observed characteristics in state *s* (such as major industries /employment laws/immigration levels within the state)

X_{sy} = time variant observed characteristics in state *s* during year *y* (such as unemployment rate)

c_y = unobserved national fixed effect

d_i = time invariant unobserved characteristics for individual *i*

e_{iy} = time variant unobserved characteristics for individual *i* during year *y* (such as health)

h_s = time invariant unobserved state characteristics in state *s*

k_{sy} = time variant unobserved state characteristics in state *s* during year *y* (such as the state disability program environment)

u_{isy} = random disturbance for individual *i*, in states, during year *y* assumed to be uncorrelated with T_{sy}, M_i, N_{iy}, P_s, X_{sy}, and c_y.

The coefficient λ is the mean impact of TTW, and γ, ω, σ, and δ are vectors of parameters associated with the respective vectors of observed and unobserved characteristics M_i, N_{iy}, P_s, and $X_{sy.}$

To consistently estimate λ , the TTW impact, we utilize the individual level longitudinal data and estimate equation (1) using fixed effects estimation technique. We can transform Equation (1) into a fixed effects model as follows:

Equation (2) $$Y_{isy} = a_i + b_s + c_y + \omega N_{iy} + \delta X_{sy} + \lambda T_{sy} + \varepsilon_{isy}$$

where,

$a_i = (M_i \gamma + d_i)$ = individual (observed and unobserved) fixed effects for individual *i*

$b_s = (P_s\sigma + h_s)$ = state (observed and unobserved) fixed effects for state *s*

c_y = annual national fixed effect for year *y*, and

$\varepsilon_{isy} = e_{iy} + k_{sy} + u_{isy}$ = unobserved disturbance term that incorporates unobserved time variant individual and state characteristics and random disturbance for individual *i* in state *s* in year *y*.

The fixed effect estimation approach we used eliminates the time-invariant unobserved and observed individual, state, and time effects by analyzing variation around the temporal mean for the individual. [5] Only the time-variant observed and unobserved individual and state effects remain. Because the disturbance term in equation (2) incorporates time-variant unobserved components (e_{iy} and k_{sy}) along with a random component (u_{isy}), the key identifying fixed effects assumption for our purposes is that the changes in the time variant unobserved components are uncorrelated with the changes in Ticket eligibility status indicator (T_{sy}) (see Wooldridge, 2002, chapter 10, section 10.5 for a discussion of this assumption in fixed effects models).

As outlined in Section B, the advantage of this strategy is that it allows each source of variation—cross-state, pre-post, and within-period cross-person—to play a role in identification, where the relative influence of each is allowed to be determined by the data. This specification allows us to control for unobserved factors at the individual and state levels that do not change over time, as well as unobserved national time effects. Hence, this model maximizes opportunities to reduce bias from fixed individual confounding factors, such as motivation and severity of impairment, and fixed state confounding factors, such as differences in infrastructures for delivering services to people with disabilities.

2. Final Econometric Model for Impact Estimates

We modified our specification for Equation 2 based on the available data, which included limited options for specifying observable individual and state time variant terms (N_{iy} and X_{sy}). Possible controls that could be included for the individual time variant

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[5] Fixed effects estimation involves transforming equation (1) first by averaging over time-period *y* to get: Equation (1a):

$$\overline{Y}_{is} = \alpha + \overline{T}_s\lambda + M_i\gamma + \overline{N}_i\omega + P_s\sigma + \overline{X}_s\delta + \overline{c}_y + d_i + \overline{e}_i + h_s + \overline{k}_s + \overline{u}_{is}$$

where the variables with horizontal-bars over them indicate the mean over time of the original variable. Subtracting equation (1a) from equation (1) for each time-period *y* gives the fixed effects transformed equation, Equation (1b):

$$Y_{isy} - \overline{Y}_{is} = (T_{sy} - \overline{T}_s)\lambda + (N_{iy} - \overline{N}_i)\omega + (X_{sy} - \overline{X}_s)\delta + (e_{iy} - \overline{e}_i) + (k_{sy} - \overline{k}_s) + (u_{isy} - \overline{u}_{is})$$

As is apparent from equation (1b), the fixed effects transformation removes the time-invariant (fixed) observed and unobserved effects at individual (i..e., $M_i\gamma + d_i$), state ($P_s\sigma + h_s$) and time variant fixed effect at the national level (c_y).

characteristics, such as education, changes in health, and marital status, are either not well measured or completely unavailable in the SSA administrative data. We explored several options for controlling for state time variant characteristics, though we only identified the county unemployment rate as a viable option. The use of county data on unemployment is important given that economic conditions can vary substantially within states and likely influence the employment and benefit outcomes of SSA beneficiaries (Stapleton et al. 1995). We considered several other possible state time-variant factors correlated with Ticket outcomes, but concluded we could not adequately quantify these factors. In our process analysis and discussions with SSA, we did not identify any major state-specific policy change that directly influenced TTW outcomes and could be quantified in a meaningful variable (e.g., state terms interacted with year dummies that could be used to capture a major state policy change). Additionally, we did not find reliable quantitative data on other state time-varying factors that might influence outcomes, such as changes in the support infrastructure for people with disabilities (e.g., transportation, accommodation changes) and changes in the availability of support services (e.g., mental health services).

Based on the available data, we estimated the following econometric model:

Equation (3) $$Y_{icsy} = a_i + b_s + c_y + \delta X_{cy} + \lambda_1 T1_{sy} + \lambda_2 T2_{sy} + \varepsilon_{icsy}$$

Where:

Y_{icsy} = outcome for individual *i* in county *c* in state *s* during year *y* (use of employment and training services, benefit receipt and amount, and employment and earnings)

a_i = individual (observed and unobserved) fixed effects for individual *i*

b_s = state (observed and unobserved) fixed effects for state *s*

c_y = time fixed effects for year *y*

X_{cy} = unemployment rates in county *c* in year *y*

$T1_{sy}$ = mailing-year TTW treatment indicator in state s in year *y*

$T2_{sy}$ = year-after-mailing TTW treatment indicator in state *s* in year *y* (earnings and benefit amount equations only)

ε_{icsy} = unobserved disturbance term for individual *i* in county *c* in state *s* in year *y*

The replacement of the single treatment dummy (T_{sy}) with two dummies ($T1_{sy}$ and $T2_{sy}$), differentiated by rollout year, allows the impacts of TTW to differ across the first two years. The key coefficients of interest in the model are λ_1 and λ_2, which represent impacts in the year of Ticket mailing and the year after Ticket mailing, respectively. The impact estimates themselves are a relatively sophisticated version of differences-in-difference estimates—estimates based on comparison of mean changes for a treatment group to the corresponding changes for comparison group. Specifically, for earnings and benefits, the

impact estimate in the year of Ticket mailing, represented by λ_1, is a combination of a) mean changes in outcomes from 2001 to 2002 in Phase 1 states net of contemporaneous mean changes in the corresponding outcomes in both Phase 2 and 3 states, and b) mean changes in outcomes from 2002 to 2003 in Phase 2 states net of contemporaneous mean changes in outcomes in Phase 3 states, holding constant changes in other factors that are captured in the model.. Similarly, the impact estimates for these outcomes in the year after Ticket mailing, represented by λ_2, is the mean change in cohort outcomes from 2001 to 2003 in Phase 1 states net of the corresponding change in Phase 3 states, holding other factors captured in the model constant. With TTW fully implemented in all states after 2003, there is no comparison group in the year after Ticket mailing for Phase 3 states.

For the service enrollment outcome, the model is capable of estimating an impact only in the year of Ticket mailing (i.e., λ_1) because, as noted above, RSA administrative data on SVRA enrollment in calendar year 2003 were incomplete when the analysis was conducted. The first-year estimates are mean changes in cohort service enrollment in Phase 1 states net of mean changes in Phase 2 and 3 states only, holding other factors captured by the equation constant; the first year of the Phase 2 rollout does not affect these estimates.

3. Motivation for Heckman-Hotz Sensitivity Tests

A key assumption of our model is that our measures of TTW treatment status ($T1_{sy}$ and $T2_{sy}$) are uncorrelated with the error term (ε_{icsy}) in Equation 3. Given the limitations in our ability to identify individual and state time variant characteristics in the data, which are reflected in the e_{iy} or k_{sy} components of the error term, it is important to test whether this assumption holds.

We are especially concerned that unobserved time variant state (k_{sy}) effects could influence outcomes based on the criteria used to select states for Phase l TTW rollout. Specifically, we are concern that (k_{sy}) may be correlated with our treatment indicators ($T1_{sy}$ and $T2_{sy}$). Stapleton and Livermore (2002) noted that the criteria for the selection of Phase 1 TTW states included the following:

- Whether the state is a recipient of a State Partnership Initiative Cooperative agreement
- Whether the state operated sites in the Employment Support Representative pilot
- Strength of the advocacy community
- Whether the state is a Disability Redesign "Prototype" State
- Strength of the provider community, including the vocational rehabilitation agency and U.S. Department of Labor "One-Stop" sites

- Recommendations of the regional commissioners
- Size of the beneficiary population (with the goal of including no more than 30 percent in the first round)

Because we have longitudinal data from pre-ticket rollout years for individuals in all three rollout-phase states, we can use the method suggested by Heckman and Hotz (1989) to test whether such differences in mean outcome changes existed across the phase groups prior to the Ticket rollout.

The test involves applying the model in Equation (3) to earlier cohorts of beneficiaries not exposed to the treatment. Specifically, for the cohort on the rolls r years prior to the year we used to determine the cohort for the impact estimates themselves (i.e., the 2001 - r beneficiary cohort), we estimated the following model, in which the TTW treatment indicators are all advanced by r years (i.e., as if the rollout had occurred r years earlier):

Equation (4) $$Y_{icsw} = a_i + b_s + c_y + \delta' X_{cw} + \lambda_1' T1_{sw} + \lambda_2' T2_{sw} + \varepsilon_{icsw}$$

Where the variables and coefficients are all the same as Equation 3, except the year subscript w, which represents the pre-Ticket cohort and is equal to y-r years. For example, given the 3 years covered in our model, the first pre-Ticket cohort would be 3 years prior to the 2001 cohort. Hence, w in this case would equal 1998 given that y=2001 and r=3.

Because the TTW was actually rolled out r years later than is implied by the treatment dummies in this specification, the "impact" estimates are expected to be insignificantly different from zero. That is, we hypothesize that:

$E(\lambda_1') = 0$ and $E(\lambda_2') = 0$

Estimates that are significantly different from zero for λ_1' or λ_2' would indicate the presence of significant variation in changes in mean outcomes across phase groups prior to TTW rollout. It would also indicate a potential violation of our assumption that $T1_{sy}$ and $T2_{sy}$ are uncorrelated with the error term (ε_{icsy}) in Equation 3. Hence, our confidence in the impact estimates would be undermined for the actual TTW rollout because differences found in the pre-TTW period might well persist after the TTW rollout – especially if they are found for several earlier cohorts. That is, the TTW impact estimates would be confounded with the effects of the factors that led to the significant results in the pre-TTW period.

As with our base set of equations, we estimate regression models by using three years of panel data, which include a baseline cohort year and two years of follow-up data. We chose the most recent cohorts when the follow-up period did not overlap with TTW rollout. Consequently, we selected beneficiary cohorts before 1999 to avoid any overlap and chose the most recent beneficiary cohorts available for each outcome in our data research file

(which dated back to 1996).[6] We created four pre–Ticket cohorts for the earnings and benefit outcomes (1999, 1998, 1997, and 1996) but were limited to two pre–TTW cohorts for service enrollment outcomes (1999 and 1998) because our linked RSA data include information on service enrollment only since 1998. We apply the sensitivity tests only for those outcomes and age-program groups for which we find large numbers of statistically significant impact estimates when the model is applied to the 2001 cohort.[7] The test is formally applied by checking whether the estimated coefficients on $T1_{sw}$ and $T2_{sw}$, represented by λ_1' and λ_2' are significantly different from zero.

E. FINDINGS

Exhibits D.3 through D.9 present our impact findings across each of the nine age-program groups for:

- SVRA-only service enrollment (Exhibit D.3)
- Total service enrollment- Upper Bound Estimates (Exhibit D.4)
- Annual earnings (Exhibit D.5)
- Annual benefit amounts (Exhibit D.6)
- Annual employment (Exhibit D.7)
- Any positive benefit amounts (Exhibit D.8)
- Left cash benefits due to work (Exhibit D.9)

We report impact estimates for each of the outcomes above, which are represented by the estimated coefficients for λ_1 and λ_2 in Equation 3. λ_1 and λ_2 represent the impact

[6] The method used to capture the county unemployment rate measure changed between 1999 and 2000 as part of the Local Area Unemployment Statistics (LAUS) redesign. The change created a break in county level unemployment rate measure time series in 2000. Because this change did not influence post-2000 cohorts, it has no effect on our impact estimates. More detailed information about these changes are available at www.bls.gov/lau. This change influences the coefficient estimate for the county unemployment rate in the 1998 and 1999 cohorts because we are measuring unemployment rates pre and post-1999. However, it does not affect our substantive conclusions from the sensitivity tests; we find the coefficient estimates on λ_1' and λ_2' do not substantively change when drop the county unemployment rate measure from our model. Our estimates presented in each of the exhibits include the county unemployment rate for all cohorts (including the problematic years), though we do not report the coefficient for this estimate. The separate estimates without the county unemployment rate are available upon request.

[7] In some of our findings below, we find impact estimates that are statistically insignificant for all but a small number of program-age subgroups (e.g., SVRA-only). In these cases, we conclude that TTW generally had an insignificant impact on the outcome; thus, we do not attempt to conduct further sensitivity tests for the smaller subgroups.

estimates in the year of Ticket mailing and the year after Ticket mailing, respectively. The sensitivity test results include estimates from earlier cohorts using the same econometric model from the impact analysis. The findings from these tests are represented by λ_1' and λ_2' in Equation 4. We report statistically significant results at the 1% level because the large sample sizes make it highly likely that we will find even small effects statistically significant.

1. SVRA-Only Service Enrollment Measures and Lower Bound Total Service Enrollment Impact Estimates

The estimates based on the SVRA-only enrollment measure indicate impacts close to zero for all age-program groups, and we do not find any evidence that TTW had a statistically significant negative impact on any group (Exhibit D.3). The largest negative statistically significant point estimate is –0.3 percentage points for age 18 to 39 concurrent beneficiaries and age 40 to 49 SSI-only beneficiaries had a positive impact estimate of 0.1 percentage point. All other groups had statistically insignificant point estimates. Given the generally insignificant findings, we did not conduct further sensitivity tests for these estimates.

As noted in Chapter XIII, based on the findings of a zero impact on SVRA-only services, we generate a lower-bound estimate of TTW's impact on total service enrollment under the assumption that the only increases in enrollment occurred through non–SVRA ENs. Our findings in the second report indicate that just under 0.1 percent of the Phase 1 caseload (approximately 10 percent of TTW participants in Phase 1 states) enrolled in a non–SVRA EN. Hence, a reasonable lower-bound estimate for the service enrollment impacts based only on non–SVRA ENs is 0.1 percentage point.

2. Upper-Bound Impacts of TTW on Total Service Enrollment

The estimated impacts of TTW for the Ticket-mailing year (represented by λ_1) on service enrollment are positive in all age-program groups and generally are larger among younger beneficiaries (Exhibit D.4, 2001 cohort). The impact estimates for beneficiaries age 18 through 39 imply an increase of less than 0.5 percentage points (concurrent beneficiaries) to just over 0.6 percentage points (DI-only beneficiaries) in enrollment in SVRA and EN services during the initial Ticket rollout year (2002). In contrast, the estimated impacts for the two older groups of beneficiaries are smaller, ranging from 0.1 percentage point (age 50 to 57 concurrent beneficiaries) to 0.4 percentage points (age 40 to 49 SSI-only recipients and age 40 to 49 concurrent beneficiaries).

In general, the magnitude of the impacts is consistent with the participation findings in Chapter III and, hence consistent with the expectation of relatively small relative service impacts. The magnitude of the impacts ranges from 0.1 to 0.6 percentage points, indicating a small increase in total service enrollment in each of the age-program groups. The largest point estimate is for DI-only beneficiaries age 18 to 39 and the smallest for concurrent beneficiaries age 50 to 57. The larger impacts for younger beneficiaries are consistent with higher TTW participation rates for this population. In general, we do not observe large

differences in impacts on service enrollment across program categories within each age group.

Our confidence in the impact estimates is bolstered by our findings of generally insignificant estimates for λ_1' in our Heckman-Hotz tests for earlier cohorts. We generally find small, statistically insignificant point estimates and, in some cases, small negative estimates for the estimate of λ_1' (e.g., DI beneficiaries age 40 to 49 and DI beneficiaries in the 1999 cohort). Because the coefficient estimates on λ_1' are generally small or insignificant, we conclude that trends in service enrollment changed appreciably across states only after rollout, thereby affirming our impact estimates above.

3. Annual Earnings Impacts Are Too Small to Differentiate from Historical Variation

The basic model yields estimated impacts of TTW on annual earnings that are negligible in the year when Tickets were mailed (i.e., the coefficients for λ_1) and generally positive, although small, in the year after mailing (i.e., the coefficients for λ_2) (Exhibit D.5, 2001 cohort). For the mailing year, the estimated impacts across all program-age groups are close to zero. Impacts on earnings in the year following Ticket mailing are larger among beneficiaries under age 50 regardless of program group. For those age 18 to 39, impact estimates in the year after rollout range from $25 (SSI-only) to $37 (DI-only) per TTW-eligible beneficiary. For beneficiaries age 40 to 49, impact estimates fall in the same range for DI-only and SSI-only beneficiaries, but the estimated impact on annual earnings by concurrent beneficiaries is not significantly different from zero. Finally, among beneficiaries age 50 to 57, the estimated impacts of TTW on earnings for DI-only and SSI-only beneficiaries are positive but small ($13 and $11 per eligible, respectively) while, again, the estimated impact on concurrent beneficiaries is not significantly different from zero. In general, these impact findings are as expected as there were no immediate effects in the year of mailing (while most TTW-eligible beneficiaries were presumably pursuing service and employment opportunities) and stronger positive effects in the year after Ticket mailing.

However, when we apply the Heckman-Hotz sensitivity test, we find that our estimates of λ_2' are statistically different from 0. In many cases, the estimate for λ_2' for those in the pre–Ticket cohorts are larger than the corresponding estimate for λ_2 in the 2001 cohort. For example, the λ_2 estimate for DI-only beneficiaries age 18 to 39 in our 2001 cohort is $37, which is lower than the estimates for λ_2' in each of the pre–Ticket cohorts ($54 in 1999, $94 in 1998, $123 in 1997, and $120 in 1996). In other cases, such as SSI-only beneficiaries age 40 to 49, the coefficients in the pre–Ticket cohorts sometimes are smaller than in the 2001 cohort (e.g., 1998 cohort) and sometimes larger (e.g., 1999 cohort). For this reason, we are skeptical that the point estimates for the 2001 cohort reflect true TTW impacts on earnings. Instead, the estimates from earlier cohorts indicate a persistently positive trend in earnings levels in Phase 1 states relative to Phase 2 and Phase 3 states before TTW rollout.

4. Annual Benefit Amount Impacts Are Too Small to Differentiate from Historical Variation

Similar to our earnings estimates, our basic model indicates that TTW had small negative impacts on disability benefit amounts during the year that Tickets were mailed and larger negative impacts in the following year (Exhibit D.6, 2001 cohort). The estimates for λ_1 are $19 or less for all program-age groups during the mailing year. The impacts in the year after Ticket mailing (λ_2) are larger for the youngest age groups and concurrent beneficiaries. For example, the impact estimates in the year after Ticket mailing for concurrent beneficiaries from youngest to oldest are -$60, -$30, and -$37, respectively. By comparison, the impact estimates from youngest to oldest for DI-only beneficiaries are smaller at -$41, -$21, and -$12, respectively; and those for SSI-only beneficiaries are smaller yet at $-23, -$19, and -$8.

However, as with the earnings equations, our estimates for λ_2' are positive and significant from the Heckman-Hotz sensitivity tests, indicating that our benefit amount impacts are not distinctly different from historical trends in these outcomes. The estimated values of λ_2' in the pre–Ticket cohorts generally are larger than the corresponding estimates of λ_2 from the 2001 cohort, although the reverse is true for some cases.

5. Phase-Specific, Time-Varying Factors More Strongly Influence Earnings and Benefit Amounts Than Service Enrollment

The differential trends in earnings and benefit amounts in the pre–TTW period across states were likely related to state differences with respect to policy and economic conditions. As noted, SSA picked the Phase 1 states on the basis of their readiness for TTW. Thus, the pre–TTW outcome trends likely reflect factors related to readiness for TTW. The findings also indicate that differences in state environments had a larger effect on earnings and benefit amounts than on service enrollment. It is plausible that differential trends in the policy and economic environment had a stronger effect on relative trends in earnings and benefit amounts than on relative trends in service enrollment, given the more direct effects associated with changes in economic conditions and earnings.

It is important to note that the differences in impacts represent relative trend differences (i.e., factors associated with r_{sy} in Equation 15) across states, not aggregate state differences. It is likely that economic conditions affect all of our outcomes. While our econometric model makes adjustments for any initial differences that exist across states, our ability to control for any within-state changes in policy or economic conditions (beyond controls for the unemployment rate) is limited. We argue that it is these within-state differences that have a stronger influence on earnings and benefits relative to service enrollment.

6. Impacts for Supplemental Benefits and Earnings Outcome Measures Are Generally Insignificant

Our impact estimates for the supplemental measures of employment and benefits—annual employment, annual benefit receipt, and exit from the disability program rolls due to work—all are very small compared to the estimated impacts on the core outcomes discussed above (Exhibits D7 through D9). For most program-age groups, the estimates are not significantly different from zero.

F. OTHER APPROACHES CONSIDERED IN ESTIMATING TTW IMPACTS

We explored three alternatives approaches to estimating impacts based on the general options outlined in Stapleton and Livermore (2002), including:

- ***Participant Comparisons.*** There are several options for estimating participant models that would make contemporaneous comparisons of outcomes for TTW participants to contemporaneous outcomes for selected non-participants in the same state. The advantage of the options is that they would provide an estimate of the effect of the treatment on the treated. In addition, the estimates could be used as a comparison to those from the Ticket-eligible models above to assess the credibility of the results. We provided a full summary of these options in Fraker and Stapleton (2004).

- ***Comparison Models Using Several Historical Cohorts***: A DID model similar to the one used in our analysis in Section D could be estimated with several beneficiary cohorts. It would increase sample size for the estimates and allow for tests of differences in TTW impacts across beneficiary cohorts. For example, rather than including a single cohort back to Ticket, we could theoretically include in our analysis all cohorts from 1996 to 2002

- ***Other Models (Pre-Post–Only or Contemporaneous Comparison–only) Using a Single Cohort***: A final set of options would use a single cohort to focus on only pre-post comparisons or contemporaneous comparisons across states. For example, a within-state pre-post cohort comparison, as originally envisioned in the second approach proposed by Stapleton and Livermore (2002), could be readily estimated by comparing outcomes in, say, Phase 1 states before Ticket rollout to estimates in the same states after rollout. Alternatively, a contemporaneous comparisons model could compare outcomes across states (but not over time).

Below, we briefly provide our rationale for excluding these models by discussing their limitations relative to the fixed effects longitudinal model presented in earlier sections.

1. Low Ticket Participation Rates and Challenges in Identifying TTW Participants Limited the Viability of Matching Models (participant comparisons)

A general participant impact model can be specified directly by defining T as an indicator of whether the individual assigned his or her Ticket to an EN. Specifically, an alternative measure of T could be incorporated into Equation 16 that compares outcomes of participants and non-participants. In such a model, the treatment indicator would identify whether a beneficiary was a Ticket participant while the estimate of λ would represent the effect of the treatment on the treated.

As pointed out by Stapleton and Livermore (2002), the key concern with the general participant impact model is that participants differ from nonparticipants in ways that cannot be adequately observed (for example, in severity of disability, support from family and friends, motivation, availability and accessibility of jobs). The probability of a very low participation rate—likely to be less than one percent in the first rollout year compared to the five percent assumed by Stapleton and Livermore–likely exacerbates the potential problem in identifying differences in participants and non-participants. In fact, Chapter III discusses the challenges in identifying TTW participants based on observable characteristics in administrative data and the importance of characteristics observed in survey data after controlling for those observed in administrative data.

Even with higher participation rates than those observed, experience from MPR's evaluation of the State Partnership Initiatives (SPI) (Peikes et al. 2005) and anecdotal findings from the process study suggest that it would likely be impossible to control adequately for unobserved factors. In the SPI projects, Peikes et al. estimated impacts by matching participants to non-participants based on propensity scores and then comparing non-experimental estimates to experimental estimates based on differences in outcomes for randomly assigned control and treatment groups. The latter were presumably unbiased, implying that the intervention did not produce a substantial impact on employment outcomes. The researchers found that, although they matched on hundreds of variables, had large pools of beneficiaries for the comparisons, and tested the process several times, the non-experimental methods produced impact estimates that were often statistically significant substantially different from the experimental estimates. As the experimental estimates are not biased, the non-experimental estimates are presumably biased, most likely because of unobserved differences between the treatment and comparison groups.

For these reasons, we did not view participant comparisons as a feasible option for estimating impacts. It is also unlikely that participant comparisons will be a feasible way to estimate future impacts.

2. Historical Cohort Comparison Approaches Are Limited Because of the Business Cycle

An alternative to a fixed effects longitudinal model using a single cohort was to use the same approach to estimate impacts using historical cohorts or repeated cross-sections. A panel data approach would pool data from multiple cohorts (from as early as 1996 given the availability of administrative data). The econometric model for estimating impacts would be

the same as Equation 16, except that it would include additional controls for each cohort to control for cohort effects. A second approach would be to use repeated cross-sections of cohorts using the same model presented in Equation 16, except that it would include individual identifiers (e.g., race, age, impairment) rather than individual fixed effects. Under both approaches, the primary challenge is finding a comparison group of beneficiaries in similar economic and policy conditions.

Of the two possibilities for estimating impacts using historical cohorts, the panel option would provide a stronger approach relative to a repeated cross-section approach, because the latter does not allow use of individual fixed effects to control for many unobservable, but important, individual factors.

In effect, the historical cohort approach would pool the data from the 2001 cohort used to produce the estimates presented here with the earlier cohorts used to produce estimates for the Heckman-Hotz tests, and eventually add later cohorts. The historical cohort estimates would net out a blend of the estimates of the λ_i' from the pre-TTW cohorts from the estimates of the λ_I for the later cohorts. It is apparent from the estimates reported that the estimated impacts on employment services for Phase 1 would change little under this approach, but many of the estimates for earnings and benefits would be zero or significant with a sign opposite that predicted.

The problem with the historical cohort approach is that the factors behind the non-zero estimates of the λ_i' for earnings and benefits likely change over time, reflecting changes in macroeconomic conditions as well as changes in the policy environment other than TTW. We illustrate the effects of macroeconomic conditions by tracking employment rates across cohorts of beneficiaries from 1997 to 2003. As shown in Exhibit D.10, the annual employment rate of disability beneficiaries changed significantly in the periods before TTW rollout, rising to 19.9 percent in 2000 from 18.7 percent in 1997. Conversely, during the 2001 recession and the weak labor market that followed, the employment rate of disability beneficiaries fell to 16.2 percent in 2003, a relative reduction of about 20 percent from the 2000 peak. Consequently, estimates of TTW impacts based on any historical cohort approach would likely be confounded with the effects of changes in the business cycle. Unfortunately, it is difficult to control for the economic and policy changes that would likely be confounded with TTW impacts. Although we could explore the use of various state or even county variables for this purpose, we would still question their adequacy for the task. Our evidence suggests that any TTW impacts are small; therefore, we would need to be confident that we have accurately controlled for changes in the policy and economic environment. Otherwise, we would never know whether most or all of the estimated "effects" of TTW reflect the impacts of TTW rather than some error in the specification of the relevant environmental and policy changes.

3. Other Single Cohort Models Offered Less Flexibility Relative to DID Approach

We considered other single cohort models that use a more simplified estimate of pre-post within-state comparisons and contemporaneous comparisons across states. These models identify a specific source of variation in TTW outcomes across rollout (pre-post) or

across states (contemporaneous comparisons). A pre-post approach to estimating TTW impacts would estimate impacts by comparing variation over time in outcomes for a treatment group of TTW-eligibles in Ticket states to outcomes for a cohort of pre–TTW beneficiaries who would have been eligible for TTW if it had been in effect. For example, the TTW impacts could be calculated by comparing the outcomes of a cohort of beneficiaries in Phase 1 states after the rollout of TTW to the outcomes of a pre–TTW cohort of beneficiaries in the same states before rollout. Alternatively, a contemporaneous comparisons-only approach would compare the outcomes of states where TTW had been implemented (for example, Phase 1 states in 2002) to states where it had not been implemented (for example, Phase 2 and 3 states in 2002).

However, both of these approaches were limited to our fixed effects longitudinal model in large part because they incorporated less information. For example, the pre-post–only estimate impacts would be more heavily influenced by national changes that could affect outcomes. As shown in Exhibit D.10, these effects could be quite large depending on the pre-post period chosen. Similarly, contemporaneous-only comparison estimates would be subject to unobserved state differences, which, based on the selection of Ticket states in Phase 1, could be large. For these reasons, we did not consider these models for estimating initial impacts.

Nevertheless, we might consider a pre-post–only model to estimate impacts in future reports if we reached and could identify an economic period that is similar to some or all of the pre–TTW periods for which we have comparable data (1997 through 2001). We might be approaching an economy that is similar to the strong economy of the late 1990s, but it is too early to know whether such an economic period will be achieved soon. Further, long-term changes in the policy environment and long-term structural changes in the economy might still be confounded with the impact of TTW, even if we can precisely control for the effects of the business cycle.

Exhibit D.1. TTW Implementation Schedule Through 2003

Year	Phase 1 States	Phase 2 States	Phase 3 States
2003	Year after Ticket mailing	Year of Ticket mailing	Prior to TTW rollout
2002	Year of Ticket mailing	Prior to TTW rollout	Prior to TTW rollout
2001	Prior to TTW rollout	Prior to TTW rollout	Prior to TTW rollout

Exhibit D.2. Three Year Average (2001-2003) of Outcome Measures for the 2001 DI and SSI Beneficiary Cohort Used in the Impact Analysis

			Mean Values (percentages unless otherwise indicated)									
			Total	DI-only			SSI-only			Concurrent		
Outcome Measure	Definition	Data Source	All Ages	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
CORE OUTCOME MEASURES												
Service enrollment	The beneficiary was an open SVRA case in at least one month of the year or had an actively assigned Ticket sometime during the year.	RSA-911 and TRF	5.3	9.3	5.0	2.4	8.9	3.5	1.8	11.4	5.9	3.4
RSA-only service enrollment	The beneficiary was an open SVRA case in at least one month of the year	RSA-911	4.8	8.4	4.6	2.2	8.1	3.2	1.7	10.5	5.5	3.2
Annual Earnings	Total earnings from FICA-covered employment over the year.	SER	$706	$1,831	$1,017	$570	$634	$311	$157	$934	$438	$259
Benefit amount	The total combined DI and SSI benefit amount over the year. Benefit amounts are fixed to 2001 values unless the beneficiary had some earnings reported in the SER.	TRF	$8,740	$8,677	$10,038	$10,904	$6,729	$6,826	$6,985	$7,638	$7,944	$8,089
SUPPLEMENTAL OUTCOME MEASURES												
Annual employment	Total earnings during the year are greater than $0	SER	16.3	30.3	18.0	11.7	20.8	9.3	4.7	29.2	16.1	10.0
Annual benefit receipt	Total combined DI and SSI benefit amount during the year is greater than $0	TRF	99.8	99.8	99.9	100.0	99.4	99.6	99.7	99.9	100	100
Left cash benefits due to work	Beneficiary is classified by SSA as having left cash benefits due to work and remained off for the entire year	TRF	0.01	0.01	0.00	0.00	0.04	0.02	0.01	0.00	0.00	0.00
Number of beneficiaries (x 1,000)			4,694	413	910	1,096	599	492	386	315	289	193

Source: Tabulations based on linked TRF, RSA-911, and SER longitudinal data files. In accordance with the Internal Revenue Service/SSA data agreement, MPR researchers did not access earnings data with personal identifiers.

Exhibit D.3 Impact Estimates on SVRA-only Service Enrollment for Ticket-Eligible Beneficiaries Age 18 to 57, by Age-Program Group

	DI-only Beneficiaries			SSI-only Beneficiaries			Concurrent Beneficiaries		
Variables	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
				Impact Estimates					
2001 Cohort									
Ticket mailing year (λ1)	-0.10	-0.06	-0.04	-0.03	0.11*	0.02	-0.29*	0.03	-0.10
Ticket one year after mailing (λ2)	NA	NA	NA	NA	NA	NA	NA	NA	NA
Observations (x 1,000)	826	1820	2192	1198	984	772	630	578	386

Source: Tabulations of econometric estimates are based on linked TRF and RSA-911 longitudinal data files.

Notes: The dependent variable equaled one if the beneficiary was an open SVRA case in at least one month of the year; otherwise, it equaled zero. All models include an intercept and controls for the county unemployment rate, individual fixed effects, state fixed effects, and year fixed effects. The impact estimates are regression coefficients (x 100) from separate econometric analyses for each age-program group. The sensitivity tests were not applied here because most impact estimates were statistically insignificant.

*Significant at the 1% level.

Exhibit D.4 Impact Estimates and Sensitivity Tests for Total (SVRA and EN) Service Enrollment Outcomes For Ticket-Eligible Beneficiaries Age 18 to 57, by Age-Program Group (Upper Bound)

	DI-only Beneficiaries			SSI-only Beneficiaries			Concurrent Beneficiaries		
Variables	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
Impact Estimates (percentage)									
2001 Cohort									
Ticket mailing year (λ1)	0.62*	0.38*	0.16*	0.53*	0.41*	0.15*	0.45*	0.44*	0.13*
Ticket one year after mailing (λ2)	NA	NA	NA	NA	NA	NA	NA	NA	NA
Observations (x 1,000)	826	1820	2192	1198	984	772	630	578	386
Sensitivity Tests Using the Same Model for Earlier Cohorts									
1999 Cohort									
λ1′	-0.04	-0.10*	-0.07*	-0.21*	-0.03	-0.06*	-0.12	-0.12*	-0.06
λ2′	NA	NA	NA	NA	NA	NA			
Observations (x 1,000)	818	1679	1973	1231	893	700	657	532	357
1998 Cohort									
λ1′	-0.10	-0.09*	-0.04*	-0.14*	0.07	0.01	-0.17*	-0.07	0.09
λ2′	NA	NA	NA	NA	NA	NA			
Observations (x 1,000)	827	1608	1831	1263	847	655	665	505	339

Source: Tabulations of econometric estimates are based on linked TRF and RSA-911 longitudinal data files.

Notes: The dependent variable equaled one if the beneficiary was an open SVRA case in at least one month of the year or had an actively assigned Ticket at some time during the year; otherwise, it equaled zero. All models include an intercept and controls for the county unemployment rate, individual fixed effects, state fixed effects, and year fixed effects. The impact estimates are regression coefficients (x 100) from separate econometric analyses for each age-program group. λ1 represents the impact estimate in the year of Ticket mailing. The sensitivity test results include estimates from earlier cohorts using the same econometric model from the impact analysis. λ1′ represents the estimate of λ1 applied to an earlier cohort. Summaries of the coefficient estimates appear in Exhibit XIII.4.

*Significant at the 1% level

Exhibit D.5 Impact Estimates and Sensitivity Tests for Annual Earnings For Ticket-Eligible Beneficiaries Age 18 to 57, by Age-Program Group

	DI-only Beneficiaries			SSI-only Beneficiaries			Concurrent Beneficiaries		
Variables	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
Impact Estimates									
2001 Cohort									
Ticket mailing year (λ1)	-1.97	0.41	-0.69	-6.19	2.21	-0.28	-6.16	-7.81	-6.09
Ticket one year after mailing (λ2)	37.00*	35.91*	13.19*	25.27*	28.02*	11.00*	29.73*	12.98	-10.36
Observations (x 1,000)	1240	2730	3289	1797	1475	1158	945	868	580
Sensitivity Tests Using the Same Model for Earlier Cohorts									
1999 Cohort									
λ1′	2.06	-9.69*	3.20	-12.57*	-3.50	-1.49	-24.20*	-22.39*	-10.43*
λ2′	54.14*	41.60*	19.93*	39.25*	32.86*	8.90*	33.47*	9.29	6.16
Observations (x 1,000)	1226	2518	2815	1846	1339	1004	985	798	514
1998 Cohort									
λ1*	6.76	-6.39	2.55	1.79	-4.47	-3.65	-26.56*	-19.57*	1.30
λ2*	94.49*	31.39*	14.25*	70.47*	26.80*	10.06*	24.79*	2.11	24.37*
Observations (x 1,000)	1240	2411	2616	1895	1271	938	998	757	487
1997 Cohort									
λ1*	18.54*	7.27	4.52	20.02*	7.67*	-2.92	17.91*	3.74	-6.64
λ2*	123.27*	61.88*	22.64*	81.43*	41.23*	19.80*	61.61*	31.66*	4.47
Observations (x 1,000)	1269	2319	2401	1936	1201	873	1013	721	461
1996 Cohort									
λ1′	18.14*	2.21	-0.61	16.96*	7.74*	6.37*	-1.27	0.08	-13.84*
λ2′	120.79*	40.81*	19.31*	72.67*	52.75*	19.22*	64.73*	40.90*	-7.78
Observations (x 1,000)	1295	2229	2182	1978	1157	812	1036	687	434

Source: Tabulations of econometric estimates are based on linked TRF files and SER longitudinal data files. In accordance with the Internal Revenue Service/SSA data agreement, MPR researchers did not access earnings data with personal identifiers.

Notes: The dependent variable equaled the total Social Security earnings from employment during the year. All models include an intercept and controls for the county unemployment rate, individual fixed effects, state fixed effects, and year fixed effects. The impact estimates are regression coefficients from separate econometric analyses for each age-program group. λ1 and λ2 represent the impact estimates in the year of Ticket mailing and the year after Ticket mailing, respectively. The sensitivity test results include estimates from earlier cohorts using the same econometric model from the impact analysis. λ1′ and λ2′ represent the estimates of λ1 and λ2 applied to an earlier cohort.

*Significant at the 1% level.

Exhibit D.6 Impact Estimates and Sensitivity Tests for Adjusted Federal Benefit Amount For Ticket-Eligible Beneficiaries Age 18 to 57, by Age-Program Group

	DI-only Beneficiaries			SSI-only Beneficiaries			Concurrent Beneficiaries		
Variables	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
Impact Estimates									
2001 Cohort									
Ticket mailing year (λ1)	-10.69*	-3.76*	-4.43	-10.93*	-4.97	-1.59	-18.90*	-13.51*	-15.89*
Ticket one year after mailing (λ2)	-41.48*	-21.54*	-12.92	-22.56*	-19.12*	-8.13*	-60.25*	-30.29*	-37.14*
Observations (x 1,000)	1240	2730	3289	1797	1475	1158	945	868	580
Sensitivity Tests Using the Same Model for Earlier Cohorts									
1999 Cohort									
λ1′	-3.23	-2.30	-2.92*	-12.79*	0.71	1.47	-10.60*	3.16	-2.82
λ2′	-18.87*	-23.54*	-13.78*	-31.92*	-6.64*	-0.71	-9.42	-0.12	1.36
Observations (x 1,000)	1226	2518	2815	1846	1339	1004	985	798	514
1998 Cohort									
λ1′	-10.06*	-7.74*	-6.35*	-18.41*	-0.81	4.07*	-2.49	-1.14	-9.81*
λ2′	-41.91*	-28.98*	-18.61*	-33.45*	-8.87*	0.86	-6.54	-5.15	-12.99*
Observations (x 1,000)	1240	2411	2616	1895	1271	938	998	757	487
1997 Cohort									
λ1′	-9.71*	-9.22*	-5.47*	-13.10*	-3.21*	0.15	-3.95	-1.60	-0.92
λ2′	-49.85*	-29.01*	-18.89*	-30.57*	-9.44*	-3.98*	-13.42*	-12.56*	-6.60
Observations (x 1,000)	1269	2319	2401	1936	1201	873	1013	721	461
1996 Cohort									
λ1′	-12.86*	-5.14*	-2.71*	-6.93*	-3.44*	-0.09	-0.32	-4.52	0.16
λ2′	-50.22*	-20.78*	-15.18*	-22.51*	-13.69*	-5.77*	-27.23*	-18.26*	-5.57
Observations (x 1,000)	1295	2229	2182	1978	1157	812	1036	687	434

Source: Tabulations of econometric estimates are based on longitudinal TRF data files.

Notes: The dependent variable is the total combined DI and SSI benefit amount over the year. All models include an intercept and controls for the county unemployment rate, individual fixed effects, state fixed effects, and year fixed effects. In estimating impacts, we allow benefits to vary from their base year only when a person reports employment. We make this restriction to avoid fluctuations in benefit amounts that might not be related to Ticket, such as overpayments. The impact estimates are regression coefficients from separate econometric analyses for each age-program group. λ1 and λ2 represent the impact estimates in the year of Ticket mailing and the year after Ticket mailing, respectively. The sensitivity test results include estimates from earlier cohorts using the same econometric model from the impact analysis. λ1′ and λ2′ represent the estimates of λ1 and λ2 applied to an earlier cohort.

*Significant at the 1% level.

Exhibit D.7 Impact Estimates for Annual Employment For Ticket-Eligible Beneficiaries Age 18 to 57, by Age-Program Group

	DI-only Beneficiaries			SSI-only Beneficiaries			Concurrent Beneficiaries		
Variables	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
Ticket mailing year (λ1)	-0.00	0.00	-0.02	-0.28*	0.02	0.04	-0.14	-0.16	-0.04
Ticket one year after mailing (λ2)	-0.04	-0.16	-0.29*	-0.50*	0.13	0.09	0.12	-0.26	-0.19
Observations (x 1,000)	1240	2730	3289	1797	1475	1158	945	868	580

Source: Tabulations of econometric estimates are based on linked TRF files and SER longitudinal data files. In accordance with the Internal Revenue Service/SSA data agreement, MPR researchers did not access earnings data with personal identifiers.

Notes: The dependent variable equaled one if the beneficiary total earnings during the year were greater than $0; otherwise, it equaled zero. All models include an intercept and controls for the county unemployment rate, individual fixed effects, state fixed effects, and year fixed effects. The impact estimates are regression coefficients (x 100) from separate econometric analyses for each age-program group. The sensitivity tests were not applied here because most impact estimates were statistically insignificant.

*Significant at the 1% level.

Exhibit D.8 Impact Estimates for Any Positive Benefit Amounts For Ticket-Eligible Beneficiaries Age 18 to 57, by Age-Program Group

	DI-only Beneficiaries			SSI-only Beneficiaries			Concurrent Beneficiaries		
Variables	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
Ticket mailing year (λ1)	0.01	0.00	0.01	-0.08*	-0.02	0.01	-0.05*	-0.03*	0.01
Ticket one year after mailing (λ2)	-0.05	-0.04*	-0.02	-0.18*	-0.05	-0.01	0.00	-0.03	0.01
Observations (x 1,000)	1240	2730	3289	1797	1475	1158	945	868	580

Source: Tabulations of econometric estimates are based on longitudinal TRF data files.

Notes: The dependent variable equaled one if the combined total DI and SS benefit amount over the years was greater than $0; otherwise it equal zero. All models include an intercept and controls for the county unemployment rate, individual fixed effects, state fixed effects, and year fixed effects. In estimating impacts, we allow benefits to vary from their base year only when a person reports employment. We make this restriction to avoid fluctuations in benefit amounts, such as overpayments, that might not be related to the Ticket. The dependent variable equaled one if the beneficiary's Total combined DI and SSI benefit amount during the year is greater than $0; otherwise, it equaled zero. The impact estimates are regression coefficients (x 100) from separate econometric analyses for each age-program group. The sensitivity tests were not applied here because most impact estimates were statistically insignificant.

*Significant at the 1% level.

Exhibit D.9. Impact Estimates for Left Cash Benefits Due to Work For Ticket-Eligible Beneficiaries Age 18 to 57, by Age-Program Group

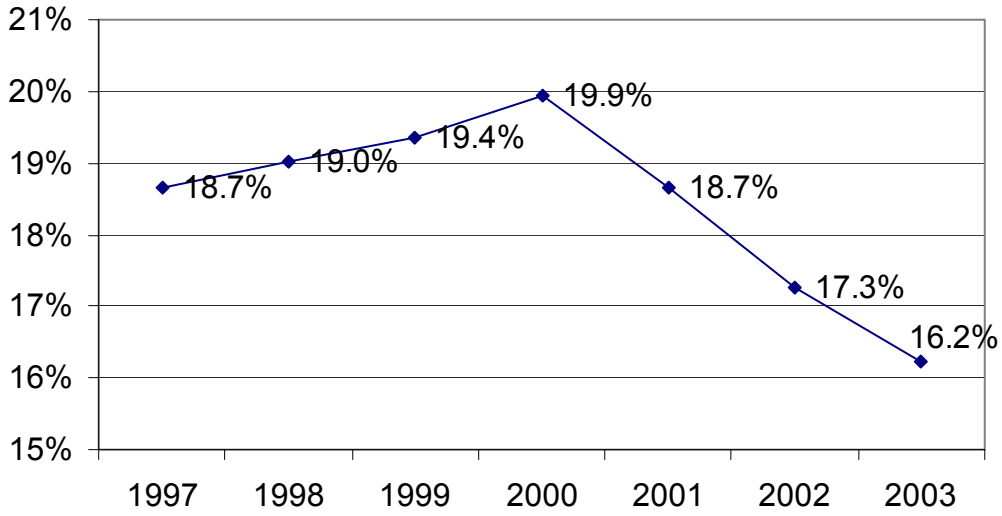
	DI-only Beneficiaries			SSI-only Beneficiaries			Concurrent Beneficiaries		
Variables	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57	Age 18-39	Age 40-49	Age 50-57
Ticket mailing year (λ1)	0.00	-0.00	-0.00	-0.00*	0.00	0.00	0.00	0.00	-0.00
Ticket one year after mailing (λ2)	0.00	-0.00*	-0.00*	-0.00*	0.00	0.00*	0.00	0.00	-0.00
Observations (x 1,000)	1240	2730	3289	1797	1475	1158	945	868	580

Source: Tabulations of econometric estimates are based on longitudinal TRF data files.

Notes: The dependent variable equaled one if SSA classified the beneficiary as having left cash benefits due to work and remained off for the entire year; otherwise it equaled zero. All models include an intercept and controls for the county unemployment rate, individual fixed effects, state fixed effects, and year fixed effects. The impact estimates are regression coefficients (x 100) from separate econometric analyses for each age-program group. The sensitivity tests were not applied here because most impact estimates were statistically insignificant.

*Significant at the 1% level.

Exhibit D.10. Annual Employment Rate of Disability Beneficiaries Age 18 to 57, 1997 - 2003



Source: Tabulations are based on linked TRF and SER data files. In accordance with the Internal Revenue Service/SSA data agreement, SSA staff produced these tabulations; MPR researchers did not access earnings data with personal identifiers. The sample includes cross-sections of SSI and DI beneficiaries who received 12 months of benefits in each year. A person was employed if s/he had any earnings in the SER during the calendar year.