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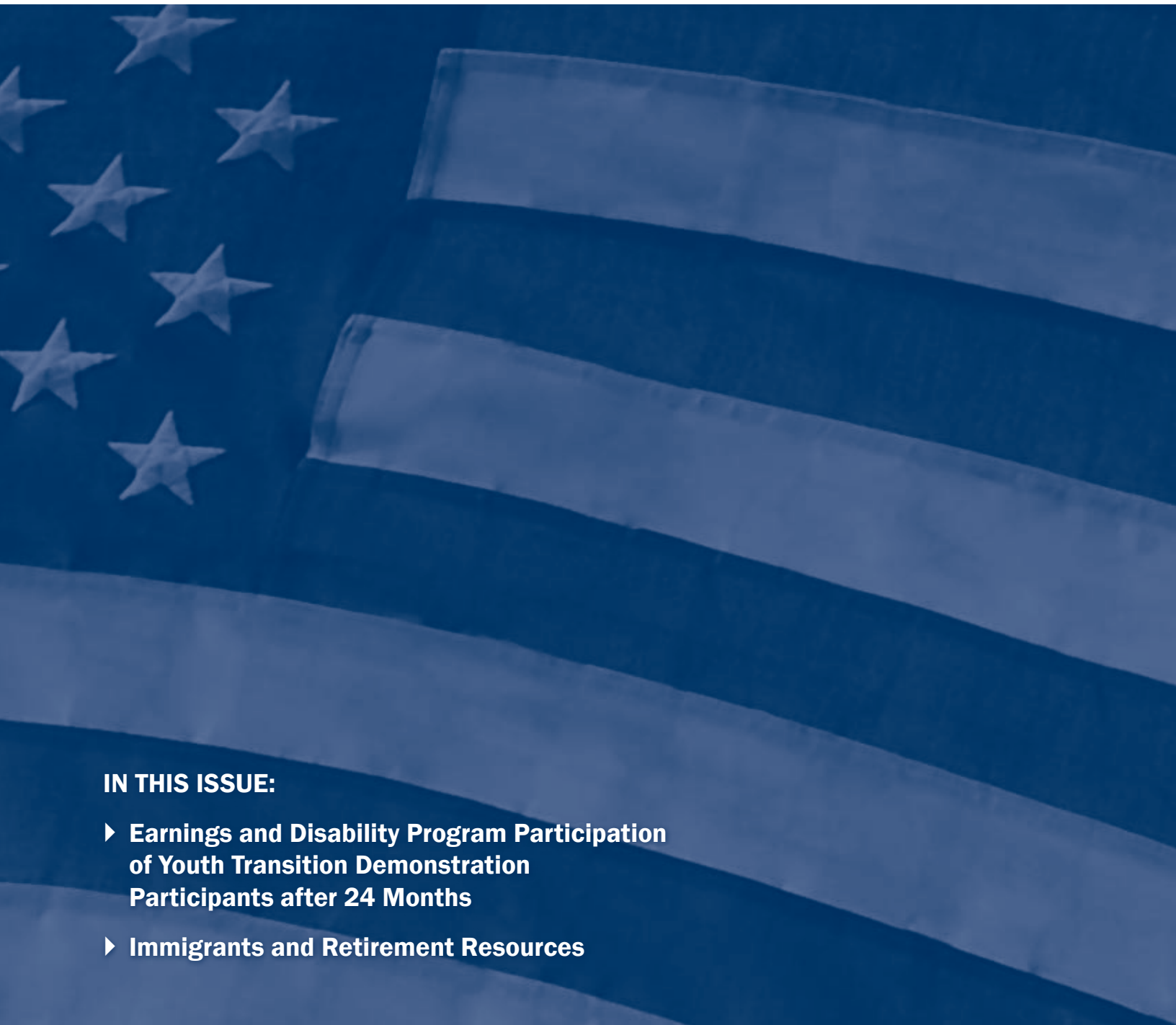

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Articles

Perspectives

Earnings and Disability Program Participation of Youth Transition Demonstration Participants after 24 Months

by Jeffrey Hemmeter*

Using data from Social Security Administration (SSA) program records, this article evaluates employment and SSA disability program payment outcomes for youths participating in SSA's Youth Transition Demonstration (YTD) project. Participants were randomly assigned to treatment or control groups at each of six project sites. Treatment-group youths in the YTD received extra employment-related and other supports in order to improve educational and vocational outcomes during their transition to adulthood. The YTD had a positive impact on the proportion of youths with earnings in the first and second years after random assignment at three sites, with two of those sites having positive impacts in both years. Additionally, the treatment groups in four sites had higher Supplemental Security Income participation rates 24 months after random assignment.

Introduction

Many youths with disabilities need assistance managing their transitions to adulthood. Services available through the education system typically end when youths complete secondary school and are seldom coordinated with adult-based services (Osgood, Foster, and Courtney 2010; GAO 2012). The difficulty of transition to adulthood is compounded for youths receiving means-tested Supplemental Security Income (SSI), who must cope with the added barriers of poverty. Relatedly, many youths who receive SSI drop out of school and encounter the criminal justice system (Loprest and Wittenburg 2007), factors that reduce the likelihood of employment in early adulthood (Hemmeter, Kauff, and Wittenburg 2009). As a result, former child SSI recipients tend to have low labor force participation well into adulthood (Davies, Rupp, and Wittenburg 2009) and remain on the SSI rolls for substantial periods as adults (Rupp and Scott 1995).

To help address these issues, the Social Security Administration (SSA) conducted the Youth Transition Demonstration (YTD) project to identify interventions that improve the educational and vocational outcomes for youths aged 14–25 who receive or potentially qualify for SSI payments or Social Security Disability Insurance (DI) benefits.[1] YTD participants received a variety of employment, education, and other services. To encourage work while they participated in the project, youths were also entitled to waivers of certain SSI and DI rules that would otherwise limit the amount of earnings they could keep while remaining eligible for program payments.

Participants were randomly assigned to treatment and control groups at six YTD project sites. Control group members were subject to standard SSA program

Selected Abbreviations

BHBF	Broadened Horizons, Brighter Futures
CDR	Continuing Disability Review
CPI-W	Consumer Price Index for Urban Wage Earners and Clerical Workers
CTP	Career Transition Program
CUNY	City University of New York
DI	Disability Insurance

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Selected Abbreviations—*Continued*

MPR	Mathematica Policy Research
SGA	substantial gainful activity
SPI	State Partnership Initiative
SSA	Social Security Administration
SSI	Supplemental Security Income
WINS	Work Incentive Network of Supports
YTD	Youth Transition Demonstration

rules and received the employment services ordinarily available in their communities; treatment group members were eligible for the program rule waivers and enhanced employment supports. Participant surveys conducted 1 year after random assignment indicated that treatment groups received higher levels of employment services than control groups at all sites, but experienced no significant reductions in benefit receipt. Although employment increased for participants at three sites, no other statistically significant outcomes emerged (Fraker and others 2011a, 2011b, 2011c, 2012a, 2012b, 2012c). Early results, however, may be misleading. Project impacts may fade as participants no longer receive services. Alternatively, impacts may increase as youths mature and, over time, apply the skills obtained in the project. As part of the long-term research strategy for the YTD, in this article I expand on those early results, using SSA program data to describe the impact of the YTD on earnings and on SSI and DI program participation in the 2 years following random assignment.[2] Future reports will assess these outcomes using both administrative and survey data.

At three of the six sites, the YTD had a positive impact on the proportion of youths with earnings 2 years after random assignment. At two of those sites, and at one site among the other three, the YTD also had positive impacts on earnings in the first year after random assignment. One of those sites guaranteed a summer job in the first year to all treatment group members interested in employment; however, that guarantee was not in place in the second year. Average earnings for the treatment group at that site were lower than those for the control group, consistent with the successful motivation of marginal workers (who would be expected to have lower average earnings) to attempt employment. Additionally, the treatment groups had higher SSI participation rates and payment amounts than control group members in four sites, consistent with the intended effect of the SSI program rule waivers.

YTD Project Overview

SSA started the YTD projects in 2003 to determine whether providing extra employment-related and other supports to youths receiving or potentially qualifying for SSI or DI benefits would improve educational and economic outcomes during their transition to adulthood, thereby eventually reducing dependence on SSA programs. The YTD began with seven sites: one each in California, Colorado, Iowa, Maryland, and Mississippi, and two in New York. The sites provided supports and services to help "youth with disabilities maximize their economic self-sufficiency as they transition from school to work." Each site worked under a cooperative agreement with SSA to "collaborate among federal, state, and local agencies to develop and implement sustainable improvements in the delivery of transition services and supports" and to "test ways to remove other barriers to employment and economic self-sufficiency" (SSA 2003).

Although services and supports differed somewhat across sites, each site provided SSA program benefits counseling, career counseling, personalized planning, family counseling, opportunities for family involvement in client services and activities, and coordination of services (Martinez and others 2010). Participants in each site were also eligible for waivers of SSA program rules that would otherwise restrict their eligibility for payments if earnings exceeded certain thresholds. Also, the termination of SSI or DI eligibility due to a finding of medical improvement in a continuing disability review (CDR) or age-18 redetermination was deferred while youths participated in the YTD. Table 1 describes the standard SSA disability program work incentives and the associated YTD waivers of certain program rules.

After a brief pilot phase, MDRC (a nonprofit contractor) reviewed the seven sites "to determine the feasibility of conducting a national random assignment evaluation of YTD and explore each project's appropriateness for and interest in such an evaluation" (Martinez and others 2010, 4). Based on that review, SSA terminated two sites (Iowa and Maryland) "because of difficulty they had reaching the goals stated in their cooperative agreements" (SSA 2008). Two other sites (California and Mississippi) continued as originally intended because of their overall strong service design; however, they were unable to implement the revisions recommended in MDRC's report, and are not discussed here.[3] The other three projects—Colorado Youth Work Incentive Network of Supports (WINS); Transition WORKS in Erie County, New York; and the

City University of New York (CUNY) YTD Project in Bronx County, New York—continued with a slightly revised YTD design. I refer to these three as the phase 1 sites.

The revised YTD included a stronger evaluation design, a technical evaluation, and greater emphasis on employment services (SSA 2008). Mathematica Policy Research (MPR) oversaw the implementation and evaluation of the revised YTD project; subcontractors provided services at each site. Additionally, TransCen, Inc. provided technical assistance to each of the projects, focusing on employment supports. After a national search, SSA selected three other sites—Career Transition Program (CTP) in Montgomery County, Maryland; Broadened Horizons, Brighter Futures (BHBF) in Miami-Dade County, Florida; and West Virginia Youth Works—to join the YTD project. I refer to these three as the phase 2 sites.

Table 1.
SSI and DI work incentives and the effects of associated YTD program rule waivers

Work incentive	Description	Policy change under YTD waiver
	SSI	
Student Earned Income Exclusion (SEIE)	Enables recipients who are students to exclude a certain amount of earnings from countable income and thus avoid reductions in SSI payments. In 2009 and 2010, SSA excluded the first $1,640 of a student's earnings each month, to a maximum of $6,600 in a year. SEIE eligibility ends when a recipient attains age 22.	Age limit is waived for YTD participants for as long as they attend school regularly.
General Earned Income Exclusion (GEIE)	Enables most SSI recipients to exclude from countable income the first $65 of earnings plus one-half of additional earnings.	YTD participants can exclude from countable income the first $65 of earnings plus three-quarters of additional earnings.
Plan to Achieve Self-Support (PASS)	Enables SSI recipients to exclude from countable income and resources amounts paid for certain expenses, such as the cost of owning a car, pursuing an education, and purchasing assistive technology, to achieve a specific SSA-approved work goal.	YTD participants can also use a PASS to explore career options or pursue additional education.
Individual Development Account (IDA)	Provides a trust-like account for SSI recipients to save for a specific goal, such as purchasing a home, going to school, or starting a business. SSA matches earnings deposited in an IDA, often at $2 for every $1 deposited by the participant. The money accumulated in an IDA is excluded when determining SSI eligibility, and the earnings deposited during a month are excluded when determining the SSI payment amount.	A YTD participant may also use an IDA to save for other approved goals.
	SSI and DI	
Continuing Disability Reviews and Age-18 Redeterminations (Section 301)	Benefits based on disability may continue despite a negative Continuing Disability Review or age-18 medical redetermination if: • the beneficiary is participating in any of certain programs, and • SSA determines that continued participation will increase the likelihood that the individual will remain off the disability rolls permanently once benefits stop. These "likelihood" determinations normally must be made on a case-by-case basis.	If SSA determines that medical disability has stopped and the participant is no longer eligible for assistance, he or she can continue to receive both cash benefits and health care services while participating in the YTD.

SOURCE: SSA (2013) and "YTD Modified SSI Program Rules (Waivers) Descriptions" (http://www.socialsecurity.gov/disabilityresearch/ytdmodifiedssi.html).

Recruitment and Enrollment

The original YTD was designed to serve all eligible youths. The project revision brought about the random assignment of participants into treatment and control groups. This change was difficult for some of the sites, but three of the original sites (the phase 1 sites) successfully modified their projects to accommodate the new recruitment and enrollment design. For example, the Erie County site changed from a classroom-based design to individualized services to allow random assignment into treatment and control groups.

MPR recruited potential participants from lists of SSI recipients and DI beneficiaries aged 14–25 in five of the sites' service areas. MPR randomized those lists and recruited the youths into the YTD project. After the youths voluntarily consented to be part of the project, MPR randomly assigned them to either the treatment group or the control group. If the sibling of a randomized youth was also eligible for the project, the sibling was assigned to the randomized youth's group but was not included in the research sample. MPR also conducted a baseline survey of the youths before randomization.

The new Maryland site followed a slightly different protocol. Its service population included youths considered to be at risk of needing, but not yet receiving, SSI or DI benefits. Site staff recruited youths directly into the project primarily from the local school district. After they consented to be part of the project, youths were processed as described for the other sites.

After randomization, staff at all sites contacted the treatment-group youths and enrolled them into project services. Each of the six sites enrolled between 79 and 89 percent of randomized treatment-group youths into services.

Project Services

Most of the types of services provided at YTD projects were those recommended by the National Collaborative on Workforce Disability for Youth (NCWD 2005), although some were drawn from "best practices" of other interventions for youths with disabilities. The YTD project's core interventions addressed the barriers youths face in their transition from school to work. Chart 1 depicts the barriers and the YTD intervention components, along with the transition environment and key project outcomes. The YTD specifically addressed these six barriers:

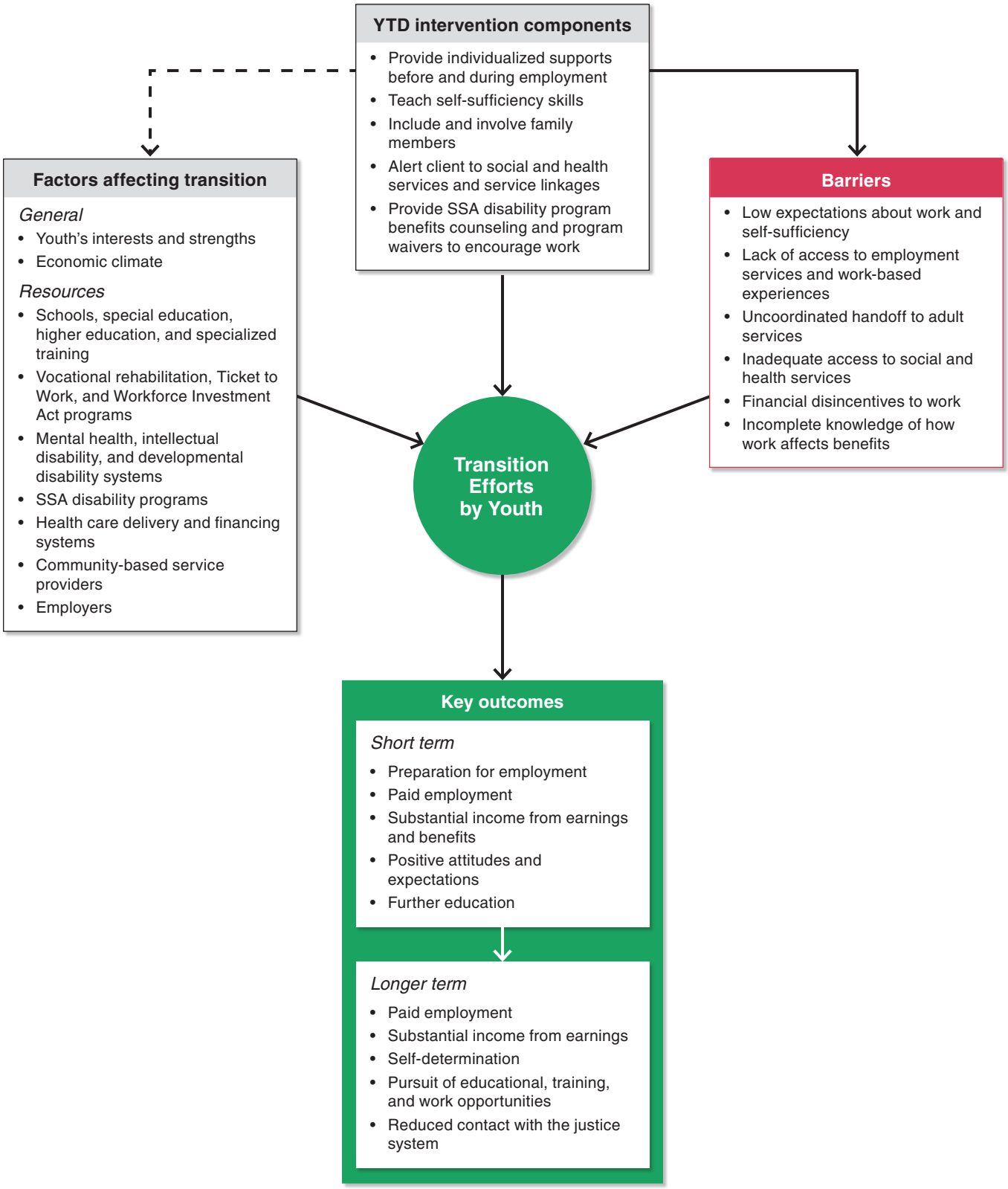
- Youths with disabilities often have low expectations for their economic future.
- Many youths with disabilities lack awareness of or access to employment services or work-based experiences.
- The handoff to general adult services is uncoordinated for many youths.
- Youths typically have inadequate access to social and health services.
- The reduction in SSI payments for recipients with certain thresholds of earned income can be viewed as a financial disincentive to work.
- Many youths and their families believe that working will result in the loss of their SSI payment or Medicaid.

Each of the YTD sites offered services to break down these real or perceived barriers to varying degrees. They offered individualized work-based experiences, including internships, job shadowing, job coaching, and competitive paid employment. They offered empowerment training to help participating youths learn to make their own choices (as opposed to having a parent or guardian choose for them). The sites also addressed the barriers by working with the families to break down misunderstandings about program rules; encouraging the families to participate in planning for the youths' self-sufficiency; working closely with local community services to link the educational and work supports for youths with disabilities, smoothing the transition to needed services; and providing case management to coordinate health and other social services. Based on results from the phase 1 sites showing lower-than-expected levels of employment, the phase 2 sites received enhanced technical assistance focused on finding ways to promote competitive employment experiences.

Youths in the treatment groups were eligible for waivers that allowed them to keep more of their earnings or save their earnings for a work or educational goal without affecting their SSI payment. To accompany those waivers, the sites provided DI and SSI benefits counseling. Virginia Commonwealth University trained the benefits counselors, as it had for an earlier program that provided counseling to DI and SSI beneficiaries under the Ticket to Work Act.[4]

The short-term objectives for the YTD project included encouraging participation in employment-promoting activities, increasing paid employment and income, improving attitudes and expectations, and enabling better educational outcomes. In the long term, the YTD project sought to increase participants'

Chart 1.
YTD design objectives



SOURCE: Adapted from Rangarajan and others (2009).

paid employment and total income, improve their self-determination, increase their general participation in productive activities (education, training, or employment), and reduce their contact with the justice system.

Site Descriptions

Although all the project sites conformed to a general YTD model, their implementations differed. This section provides a broad overview of each site's services and includes a reference to the site's detailed interim report.

Colorado Youth WINS

The Colorado site was run by the WINS Partners at the University of Colorado Health Sciences Center. Colorado Youth WINS served youths aged 14–25 who received SSI payments or DI benefits in Larimer, El Paso, Pueblo, and Boulder counties. A team of staff members provided services in each of the counties' One Stop Workforce Centers.[5] The team included a disability program navigator,[6] a benefits counselor, and one or more career counselors. Four hundred sixty-eight youths were randomized into the Colorado Youth WINS treatment group; 401 enrolled in services (86 percent). The control group comprised 387 youths. Random assignment occurred between August 2006 and March 2008, and services ended in the fall of 2009. Youths were eligible for services for at least 18 months.

To help fill gaps in youths' access to services from existing sources—such as the Division of Vocational Rehabilitation, local school systems, and other providers—Colorado Youth WINS focused primarily on case management, disability program navigation, and benefits counseling. Project staff also developed person-centered plans to help the youths identify educational, employment, and benefit goals and needs. Family members were included in most of the discussions. Career counselors provided vocational assessments and career exploration activities (such as job-site visits). By locating in the One Stop Workforce Centers, Colorado Youth WINS provided easy access to job development and placement services. For more information on the Colorado Youth WINS project, see Fraker and others (2011a).

New York: CUNY YTD Project

The CUNY site worked with youths aged 16–19 who received SSI payments or DI benefits. It was run by CUNY's John F. Kennedy Institute for Worker Education at the Hostos and Lehman campuses in Bronx County. Project staff included benefits counselors, parent advocates, career developers, and students at the partner campuses. An advisory committee of community groups, campus experts, and public agencies provided direction for the program and suggested potential service partners and ways to link the program with community services. Four hundred ninety-two youths were randomized into the CUNY treatment group; 387 enrolled in services (79 percent). The control group comprised 397 youths. Random assignment occurred between July 2006 and November 2008, and services ended in May 2010.

Treatment-group youths received direct services for 1 school year, after which summer employment and limited follow-up services were available. Youths attended Saturday sessions offering recreational and social activities and workshops on self-determination, benefits planning, and career development. Students from the partner campuses who enrolled in a course on working with youths with disabilities led the social and recreational activities; many of those students (and other staff) had disabilities themselves. While youths attended these activities, family members met to discuss benefits and issues related to the youths' self-determination. Youths also developed person-centered plans for identifying and achieving their goals. Parent advocates checked in with families to ensure they participated and met with the people who could address the youths' (or parents') needs. Services culminated with an offer of 7 weeks of summer employment through New York City's Summer Youth Employment Program or in an on-campus job.[7] For more information on the CUNY project, see Fraker and others (2011b).

New York: Erie County Transition WORKS

The Erie County project served youths aged 16–25 who received either SSI payments or DI benefits in Erie County, New York (which includes the city of Buffalo). The Erie 1 Board of Cooperative Educational Services ran the project. They partnered with the Parent Network of Western New York, Neighborhood Legal Services, the Community Employment Office, and other agencies to provide services emphasizing self-empowerment. Youths were eligible for 18 months of services, with some employment supports available after that. Four hundred fifty-nine youths were randomized into the treatment group; 380 enrolled in services (83 percent). There were 384 youths in the control group. Random assignment occurred between January 2007 and March 2008, and services ended in the fall of 2009.

The Erie County service delivery schedule was very structured, with youths attending person-centered planning and self-determination workshops before receiving employment- or education-related services. The youths set short- and long-term goals for themselves (with the help of a counselor) and created a transition plan to meet those goals. Youths were also trained to organize important documents related to their benefits in a binder, to which they could easily refer as needed. Job developers helped conduct formal vocational assessments to learn about the youths' interests, arrange for informal work experiences (such as job shadowing, job-site tours, and mock interviews), and set up paid employment and internships when participants were ready. If youths were interested in continuing their education, transition coordinators helped them explore their options (for example, earning a high school equivalency degree or enrolling in college). For more information on the Erie County project, see Fraker and others (2011c).

Maryland: Montgomery County CTP

St. Luke's House, Inc., a local mental health provider, ran the CTP. This project served youths aged 16–22 who either were in the last 2 years of high school or had left high school in the past 12 months. Unlike the other projects, the CTP served a subset of the overall YTD target: youths with severe emotional disturbances who might be eligible for SSI payments or DI benefits. Roughly 12 percent of participants received either SSI or DI at the time of random assignment. Treatment-group participants were eligible for up to 18 months of services plus 2 years of follow-up services. MPR randomized 422 youths into the treatment group, 374 of whom enrolled in services (89 percent); 383 youths were in the control group. Random assignment occurred between April 2008 and December 2010, and services ended in March 2012.

After enrolling in the CTP, youths received individualized services focused on employment, beginning with an interview with a career transition specialist to determine the areas in which they needed support to accomplish their goals. The career transition specialist would meet with each youth once a week to develop service needs and interim goals. The youths then received a variety of preemployment vocational assessments (such as mock interviews, work trials, or job shadowing) to prepare them for competitive employment. CTP staff worked directly with employers and community employment resources to help place youths in jobs. Concurrent with those employment services, staff provided benefits counseling, case management, referrals to social and health services, and family supports. For more information on the CTP, see Fraker and others (2012a).

Florida: Miami-Dade County BHBF

Abilities, Inc. of Florida (now ServiceSource) ran the BHBF project. They worked with SSI or DI beneficiaries aged 16–22. Youths received up to 18 months of services, plus follow-up services as needed. BHBF enrolled 388 (84 percent) of the 460 youths randomized into the treatment group; 399 youths were in the control group. Random assignment occurred between April 2008 and September 2010, and services ended in March 2012.

As part of a strong benefits-counseling component, BHBF partnered with the Human Services Coalition and the National Disability Institute to provide financial literacy training along with many career preparation and placement services. The program included one-on-one and group services. For example, although youths developed individual plans for reaching their goals, BHBF also hosted job fairs to connect them with employers and provided work-based experiences, enabling youths to explore different jobs. For more information on BHBF, see Fraker and others (2012b).

West Virginia Youth Works

The Human Resources Development Foundation, Inc. ran the Youth Works project in 19 counties, where they provided up to 18 months of services to youths aged 15–25 receiving SSI or DI benefits. The project enrolled 388 (85 percent) of the 455 youths randomized in the treatment group; 397 youths were assigned to the control group. Random assignment occurred between March 2008 and September 2010, and services ended in March 2012.

Youth Works' services began with goal identification, in which youths received one-on-one benefits counseling and their interests were assessed. This culminated in a person-centered plan for future services and goals. Job placement activities followed to help improve youths' job search and "soft" skills. Youths were taught how to search for positions on their own, but were supported by customized employment specialists. Once working, the youths received job coaching and other employment supports. For more information on West Virginia Youth Works, see Fraker and others (2012c).

Data and Methods

The data for this study come from SSA program records matched to the randomization and enrollment dates provided by MPR. SSA's Master Earnings File provides data on all earnings reported on an individual's W-2 tax record (including non-Federal Insurance Contribution Act taxable earnings). The Supplemental Security Record and the Master Beneficiary Record respectively contain the program participation histories for SSI recipients and DI beneficiaries. Dates of death come from the Numerical Identification System (Numident) file, which contains SSA's Death Master List.

The outcomes of interest in this study are earnings and SSA program payments. Earnings include any income reported as wages or self-employment earnings on the Master Earnings File. Wage data are from W-2 records. If an individual worked more than one job in a given year, wage data from each W-2 are totaled to provide a single measure of annual earnings.

Total SSA program payments represent the sum of SSI payments due and DI benefits paid. SSI payments due are those the recipient should have received in a month if he or she reported all earnings and exclusions in a timely manner and if work incentives (including the YTD waivers) were applied correctly. Using this measure smooths the payment stream for each recipient.[8] Smoothing the SSI payment data is especially important given the potential complications arising from YTD waiver implementation. Taking such a step is not as important for DI. There are relatively few DI beneficiaries in this sample, and because they are generally not subject to a monthly earnings test, their monthly benefit amounts are much less likely to vary. Additionally, only one of the five program rule waivers applied to DI beneficiaries, and it affected benefit receipt or nonreceipt, not the payment amount. Thus, actual DI benefits should not fluctuate to the extent that SSI payments do. Results based solely on payments made (for both SSI and DI) are available upon request from the author.

As noted, MPR used random assignment to place individuals into treatment and control groups. Therefore, comparisons of the outcomes between the treatment and control groups are unbiased and measure the average impact we would expect to see in a program in which some individuals choose not to receive services for which they are eligible; that is, the results measure the intent-to-treat impact.[9] I do not use weights and the results do not control for any known differences in the characteristics of the two groups at the time of randomization.[10] All dollar values are adjusted to 2012 dollars using the Consumer Price Index for Urban Wage Earners and Clerical Workers (CPI-W).

The administrative records capture program participation on a monthly basis, so the SSI and DI results include all YTD participants who remain alive through the 24th month after random assignment. Although random assignment occurred irregularly over a 2-year period for each site, administrative earnings data are available only as calendar-year amounts. Therefore, I measure earnings in the first and second calendar years after random assignment. For example, if an individual was randomized in August 2009, the first and second years after random assignment are 2010 and 2011, respectively. For many participants, earnings in the second year represent those in their first year without program services. The few individuals who died over the course of this study are included in the analysis up to the month (for program participation) or year (for earnings) before death.

Results

This section presents the YTD project outcomes as of 24 months (for SSI and DI participation) and 2 years (for earnings) after participants were randomly assigned to treatment or control groups.

Characteristics of YTD Participants

Table 2 shows the characteristics of YTD participants. There were few statistically significant differences between the treatment and control groups, and those occurred only in the phase 1 sites. In CUNY, treatment-group youths were more likely than control-group youths to have mental disorders (other than intellectual disability) as their primary impairment, by 8.4 percentage points. In Erie County, treatment-group members were 7.4 percentage points more likely than control-group members to have nonintellectual disability mental disorders. Treatment-group youths in Colorado were 7.5 percentage points more likely to be male than control-group members; they were also 6.4 percentage points more likely to have an intellectual disability. There were no statistically significant differences in participant characteristics in the phase 2 sites.

Although the characteristics within each site were generally similar, differences between the sites are worth highlighting. Over three-fourths of the Erie County and Colorado sites' youths were aged 18 or

Table 2.
Selected characteristics of YTD participants, by site

Characteristic	Treatment group		Control group		Difference		
	Percent	Standard error	Percent	Standard error	Percentage points	Standard error	*p*-value
Phase 1 sites							
New York: CUNY							
Male	68.1	2.1	66.5	2.4	1.6	3.2	0.62
Disability							
Intellectual disability	25.4	2.0	29.7	2.3	-4.3	3.0	0.15
Other mental disorders	53.5	2.2	45.1	2.5	8.4***	3.4	0.01
Nervous system and other sensory disorders	6.5	1.1	8.6	1.4	-2.1	1.8	0.24
Other disabilities	14.6	1.6	16.6	1.9	-2.0	2.5	0.42
Age at random assignment							
15 or younger	15.9	1.6	19.4	2.0	-3.5	2.6	0.17
16	44.5	2.2	40.6	2.5	4.0	3.3	0.24
17	31.7	2.1	33.2	2.4	-1.5	3.2	0.63
18 or older	7.9	1.2	6.8	1.3	1.1	1.8	0.52
Number	492		397		. . .	. . .	. . .
New York: Erie County							
Male	62.1	2.3	61.7	2.5	0.4	3.4	0.91
Disability							
Intellectual disability	36.4	2.2	38.5	2.5	-2.2	3.3	0.52
Other mental disorders	39.2	2.3	31.8	2.4	7.4**	3.3	0.02
Nervous system and other sensory disorders	8.1	1.3	10.4	1.6	-2.4	2.0	0.24
Other disabilities	16.3	1.7	19.3	2.0	-2.9	2.7	0.27
Age at random assignment							
15 or younger	1.3	0.5	1.0	0.5	0.3	0.7	0.72
16	12.4	1.5	10.2	1.5	2.3	2.2	0.30
17	10.5	1.4	11.7	1.6	-1.3	2.2	0.56
18 or older	75.8	2.0	77.1	2.1	-1.3	2.9	0.67
Number	459		384		. . .	. . .	. . .
Colorado							
Male	60.3	2.3	52.7	2.5	7.5**	3.4	0.03
Disability							
Intellectual disability	33.8	2.2	27.4	2.3	6.4**	3.1	0.04
Other mental disorders	30.3	2.1	34.9	2.4	-4.5	3.2	0.16
Nervous system and other sensory disorders	17.5	1.8	17.1	1.9	0.5	2.6	0.86
Other disabilities	18.4	1.8	20.7	2.1	-2.3	2.7	0.40
Age at random assignment							
15 or younger	10.0	1.4	10.1	1.5	-0.0	2.1	0.99
16	8.5	1.3	7.8	1.4	0.8	1.9	0.67
17	5.3	1.0	5.9	1.2	-0.6	1.6	0.70
18 or older	76.1	2.0	76.2	2.2	-0.2	2.9	0.96
Number	468		387		. . .	. . .	. . .

(Continued)

Table 2.
Selected characteristics of YTD participants, by site—*Continued*

Characteristic	Treatment group		Control group		Difference		
	Percent	Standard error	Percent	Standard error	Percentage points	Standard error	*p*-value
Phase 2 sites							
Maryland							
Male	67.6	2.3	66.5	2.4	1.1	3.3	0.73
Disability							
Intellectual disability	2.9	0.8	3.9	1.0	-1.1	1.3	0.40
Other mental disorders	19.5	1.9	19.9	2.0	-0.4	2.8	0.89
Nervous system and other sensory disorders	1.0	0.5	0.5	0.4	0.4	0.6	0.48
Other disabilities	76.7	2.1	75.7	2.2	1.0	3.0	0.74
Age at random assignment							
15 or younger	0.5	0.3	0.8	0.5	-0.3	0.6	0.58
16	18.3	1.9	19.4	2.0	-1.0	2.8	0.71
17	27.4	2.2	26.2	2.2	1.2	3.1	0.70
18 or older	53.8	2.4	53.7	2.6	0.1	3.5	0.97
Number	420		382		. . .	. . .	. . .
Florida							
Male	58.0	2.3	61.4	2.4	-3.4	3.4	0.32
Disability							
Intellectual disability	32.6	2.2	34.6	2.4	-2.0	3.2	0.54
Other mental disorders	45.7	2.3	43.6	2.5	2.0	3.4	0.55
Nervous system and other sensory disorders	9.1	1.3	7.5	1.3	1.6	1.9	0.40
Other disabilities	12.6	1.5	14.3	1.8	-1.7	2.3	0.47
Age at random assignment							
15 or younger	. . .	. . .	. . .	. . .	. . .	. . .	. . .
16	4.1	0.9	4.0	1.0	0.1	1.4	0.93
17	13.0	1.6	13.3	1.7	-0.2	2.3	0.92
18 or older	82.8	1.8	82.7	1.9	0.1	2.6	0.96
Number	460		399		. . .	. . .	. . .
West Virginia							
Male	58.5	2.3	57.2	2.5	1.3	3.4	0.71
Disability							
Intellectual disability	31.2	2.2	30.0	2.3	1.2	3.2	0.70
Other mental disorders	42.0	2.3	44.8	2.5	-2.9	3.4	0.40
Nervous system and other sensory disorders	10.3	1.4	9.6	1.5	0.8	2.1	0.71
Other disabilities	16.5	1.7	15.6	1.8	0.9	2.5	0.73
Age at random assignment							
15 or younger	0.9	0.4	0.8	0.4	0.1	0.6	0.84
16	5.5	1.1	6.5	1.2	-1.1	1.6	0.52
17	10.1	1.4	11.1	1.6	-1.0	2.1	0.64
18 or older	83.5	1.7	81.6	1.9	1.9	2.6	0.46
Number	455		397		. . .	. . .	. . .

SOURCE: Author's calculations using SSA program records.

NOTES: Totals do not necessarily equal the sum of rounded components.

. . . = not applicable.

* = statistically significant at the .10 level (two-tailed t-test).

** = statistically significant at the .05 level (two-tailed t-test).

*** = statistically significant at the .01 level (two-tailed t-test).

older at the time of enrollment, and over 80 percent of the Florida and West Virginia participants were 18 or older at enrollment. Conversely, only about 7 percent of the CUNY youths and just over one-half of the Maryland youths were 18 or older. Over 40 percent of CUNY, Florida, and West Virginia participants had mental disorders other than intellectual disability. However, because many of the Maryland youths were not receiving SSI or DI, their disabilities are not identified in the administrative records, for which reason they are grouped in the "other disabilities" category. The different characteristics of the populations served (and the different program models) indicate the risk of comparing outcomes between one site and another.

SSI and DI Program Participation

Because five of the six sites recruited participants from SSA program lists, it is not surprising that almost all youths in those five sites received either SSI payments or DI benefits in the month of random assignment (Table 3). At those sites, between 76.7 percent and 95.1 percent of participants were due an SSI payment, and at all six sites, SSI receipt was more prevalent than DI receipt. The shares of youths receiving DI benefits ranged from 2.6 percent to 25.6 percent, and many were auxiliary beneficiaries eligible for DI benefits based on somebody else's earnings record, typically that of a parent. However, some youths received DI primary (worker) benefits, particularly in the sites with older participants. In Colorado, for example, 10 percent of youths received DI worker benefits (not shown; breakdowns for DI worker and auxiliary benefits are available upon request).

The few youths not receiving any disability benefit in the month of random assignment either (a) were not receiving a check because of temporary overpayment issues or (b) had recently left the program after a CDR or age-18 redetermination. The projects enrolled those youths because of the strong possibility they would return to SSI or DI (although perhaps only after a successful appeal of the CDR or redetermination decision). In CUNY, nonrecipient youths were more prevalent in the control group, accounting for a 5.4 percentage point difference in SSI participation in the month of random assignment. Treatment-group youths were 5.3 percentage points less likely to receive SSI than the control group in Maryland. Recall that that site did not recruit from program records, instead primarily targeting youths deemed "at risk" of needing SSI or DI.

In the CUNY, Erie County, Florida, and West Virginia sites, the treatment group was significantly more likely to receive SSI payments 24 months after random assignment than the control group (Table 4). In CUNY, the difference in SSI participation after 24 months was 10.9 percentage points and is highly significant. This difference is likely due to the use of program rule waivers; for example, the CUNY treatment-group youths were 3 to 10 percentage points more likely to use the various SSA work incentives (and their associated waivers) than control-group youths (Fraker and others 2011b).

Looking only at the 24-month outcomes, however, masks program participation patterns. For example, although the SSI receipt rate fell by 10.2 percentage points 24 months after random assignment for CUNY treatment-group youths, the drop was relatively greater for the control group (15.7 percentage points). Chart 2 illustrates the changes in SSI and DI benefit receipt at random assignment and after 24 months (from Tables 3 and 4). In five of the six sites, the difference between the proportions of treatment- and control-group youths receiving SSI payments is greater at 24 months. Again, this can be explained by the use of program waivers that maintained youths' SSI eligibility (Maryland, the exception, did not draw its participants from the SSI program rolls). For three sites, the difference between the treatment and control groups in the proportion of youths receiving DI benefits decreased over time. In the CUNY, Florida, and West Virginia sites, however, the differences increased. As noted in Fraker (2013) and discussed later in this article, the Florida and West Virginia sites in particular reported large employment impacts, which may have enabled youths there to earn enough quarters of coverage to qualify for DI benefits.[11,12]

Earnings

In the first year after random assignment, only the CUNY, Florida, and West Virginia sites had statistically significant impacts on the prevalence of earners (Table 5).[13] Over 48 percent of CUNY treatment-group youths had earnings, compared with only 23.9 percent of control-group youths. CUNY guaranteed a paid summer job for any treatment-group youth who wanted one, so that finding may reflect a program offering unique to the CUNY site more than the outcome of an intervention. In Florida, 29.8 percent of the treatment-group youths had earnings, compared with 23.6 percent of control-group youths. In West Virginia, 44.0 percent of treatment-group

Table 3.
SSA disability program participation of YTD participants in the month of random assignment, by site

Site and program	Treatment group		Control group		Difference		
	Percent	Standard error	Percent	Standard error	Percentage points	Standard error	*p*-value
New York: CUNY							
SSI	95.1	1.0	89.7	1.5	5.4***	1.8	0.00
DI	3.7	0.8	4.0	1.0	-0.4	1.3	0.77
Either	95.5	0.9	90.2	1.5	5.4***	1.8	0.00
Both	3.3	0.8	3.5	0.9	-0.3	1.2	0.82
New York: Erie County							
SSI	89.3	1.4	87.0	1.7	2.3	2.2	0.29
DI	19.4	1.8	23.2	2.2	-3.8	2.8	0.18
Either	96.7	0.8	94.8	1.1	1.9	1.4	0.16
Both	12.0	1.5	15.4	1.8	-3.4	2.4	0.15
Colorado							
SSI	84.2	1.7	85.8	1.8	-1.6	2.4	0.52
DI	23.5	2.0	25.6	2.2	-2.1	3.0	0.48
Either	95.5	1.0	95.3	1.1	0.2	1.4	0.91
Both	12.2	1.5	16.0	1.9	-3.8	2.4	0.11
Maryland							
SSI	8.8	1.4	14.1	1.8	-5.3**	2.3	0.02
DI	3.8	0.9	2.6	0.8	1.2	1.2	0.34
Either	11.9	1.6	15.7	1.9	-3.8	2.4	0.12
Both	0.7	0.4	1.0	0.5	-0.3	0.7	0.61
Florida							
SSI	76.7	2.0	79.2	2.0	-2.5	2.8	0.39
DI	11.1	1.5	9.8	1.5	1.3	2.1	0.53
Either	81.1	1.8	82.7	1.9	-1.6	2.6	0.54
Both	6.7	1.2	6.3	1.2	0.5	1.7	0.78
West Virginia							
SSI	84.8	1.7	82.9	1.9	2.0	2.5	0.44
DI	22.2	1.9	20.7	2.0	1.5	2.8	0.58
Either	92.7	1.2	90.9	1.4	1.8	1.9	0.33
Both	14.3	1.6	12.6	1.7	1.7	2.3	0.47

SOURCE: Author's calculations using SSA program records.

NOTES: Totals do not necessarily equal the sum of rounded components.

* = statistically significant at the .10 level (two-tailed t-test).

** = statistically significant at the .05 level (two-tailed t-test)

*** = statistically significant at the .01 level (two-tailed t-test).

Table 4.
SSA disability program participation of YTD participants 24 months after random assignment, by site

Site and program	Treatment group		Control group		Difference		
	Percent	Standard error	Percent	Standard error	Percentage points	Standard error	*p*-value
New York: CUNY							
SSI	84.9	1.6	74.0	2.2	10.9***	2.7	0.00
DI	2.2	0.7	3.8	1.0	-1.6	1.2	0.17
Either	85.3	1.6	74.3	2.2	11.0***	2.7	0.00
Both	1.8	0.6	3.6	0.9	-1.7	1.1	0.11
Died	0.2	0.2	1.0	0.5	-0.8	0.5	0.11
New York: Erie County							
SSI	80.5	1.9	74.3	2.3	6.3**	2.9	0.03
DI	20.8	1.9	22.3	2.1	-1.5	2.9	0.60
Either	89.7	1.4	83.8	1.9	5.9*	2.4	0.01
Both	11.6	1.5	12.7	1.7	-1.1	2.3	0.62
Died	0.4	0.3	1.8	0.7	-1.4**	0.7	0.05
Colorado							
SSI	81.0	1.8	78.1	2.1	2.9	2.8	0.30
DI	25.3	2.0	26.6	2.3	-1.3	3.0	0.67
Either	92.7	1.2	90.4	1.5	2.3	1.9	0.23
Both	13.6	1.6	14.3	1.8	-0.7	2.4	0.76
Died	1.1	0.5	0.8	0.4	0.3	0.7	0.66
Maryland							
SSI	17.0	1.8	18.1	2.0	-1.0	2.7	0.70
DI	4.6	1.0	3.9	1.0	0.6	1.4	0.66
Either	20.4	2.0	20.4	2.1	-0.0	2.9	0.99
Both	1.2	0.5	1.6	0.6	-0.4	0.8	0.65
Died	0.7	0.4	0.0	0.0	0.7*	0.4	0.10
Florida							
SSI	72.8	2.1	61.2	2.5	11.7***	3.2	0.00
DI	14.1	1.6	11.4	1.6	2.7	2.3	0.24
Either	77.5	2.0	66.8	2.4	10.7***	3.1	0.00
Both	9.5	1.4	5.8	1.2	3.7**	1.8	0.05
Died	1.5	0.6	1.3	0.6	0.3	0.8	0.74
West Virginia							
SSI	79.4	1.9	70.7	2.3	8.7***	3.0	0.00
DI	27.0	2.1	23.0	2.1	4.0	3.0	0.18
Either	89.4	1.4	80.1	2.0	9.3***	2.5	0.00
Both	17.0	1.8	13.6	1.7	3.4	2.5	0.17
Died	0.7	0.4	0.3	0.3	0.4	0.5	0.39

SOURCE: Author's calculations using SSA program records.

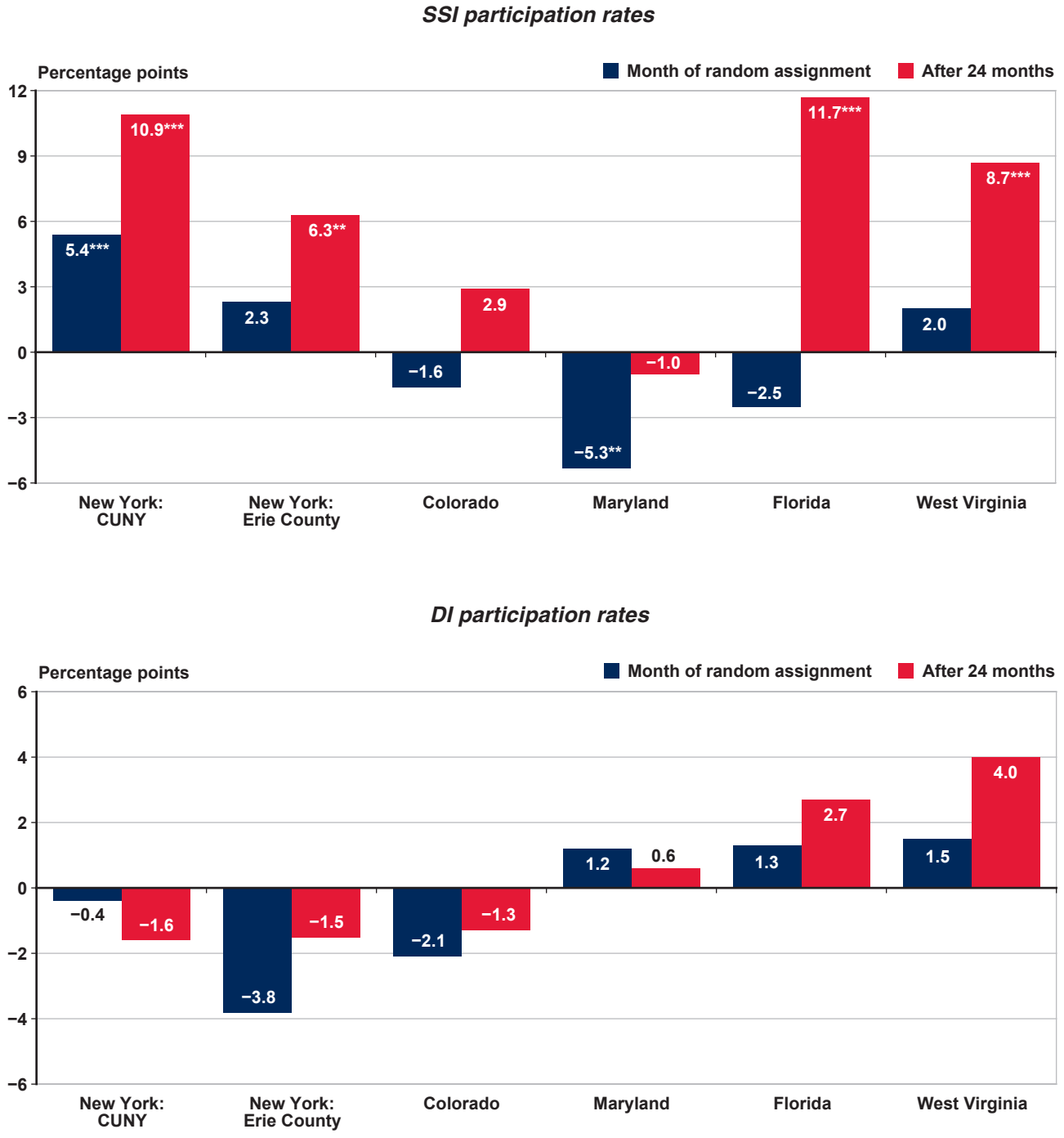
NOTES: Totals do not necessarily equal the sum of rounded components.

* = statistically significant at the .10 level (two-tailed t-test).

** = statistically significant at the .05 level (two-tailed t-test)

*** = statistically significant at the .01 level (two-tailed t-test).

Chart 2.
Percentage point differences in SSI and DI participation rates between YTD treatment groups and control groups in the month of random assignment and after 24 months, by project site



SOURCE: Author's calculations using SSA program records.

NOTES: Each value reflects the treatment-group percentage minus the control-group percentage at the given site and time.

* = statistically significant at the .10 level (two-tailed t-test).

** = statistically significant at the .05 level (two-tailed t-test).

*** = statistically significant at the .01 level (two-tailed t-test).

Table 5.
Earnings of YTD participants 1 year and 2 years after random assignment, by site

Earnings characteristic and site	Treatment group			Control group			Difference			*p*-value
	Percent	Dollars	Standard error	Percent	Dollars	Standard error	Percentage points	Dollars	Standard error	
					1 year after random assignment					
Participants with any earnings (%)										
New York: CUNY	48.4	. . .	2.3	23.9	. . .	2.1	24.4***	. . .	3.1	0.00
New York: Erie County	44.2	. . .	2.3	40.6	. . .	2.5	3.6	. . .	3.4	0.29
Colorado	39.7	. . .	2.3	36.7	. . .	2.4	3.1	. . .	3.3	0.36
Maryland	58.8	. . .	2.4	55.0	. . .	2.5	3.8	. . .	3.5	0.27
Florida	29.8	. . .	2.1	23.6	. . .	2.1	6.2**	. . .	3.0	0.04
West Virginia	44.0	. . .	2.3	28.0	. . .	2.3	16.0***	. . .	3.2	0.00
Earnings among all participants ($)										
New York: CUNY	. . .	714	54	. . .	566	94	. . .	148	104	0.15
New York: Erie County	. . .	1,788	214	. . .	1,684	210	. . .	104	303	0.73
Colorado	. . .	1,738	189	. . .	1,536	190	. . .	202	271	0.46
Maryland	. . .	2,808	274	. . .	2,465	214	. . .	343	352	0.33
Florida	. . .	1,503	179	. . .	1,161	152	. . .	342	239	0.15
West Virginia	. . .	1,776	186	. . .	1,392	187	. . .	384	265	0.15
Earnings among participants with any earnings ($)										
New York: CUNY	. . .	1,477	88	. . .	2,361	331	. . .	-885***	251	0.00
New York: Erie County	. . .	4,044	435	. . .	4,124	447	. . .	-80	632	0.90
Colorado	. . .	4,364	406	. . .	4,187	438	. . .	177	602	0.77
Maryland	. . .	4,764	423	. . .	4,485	329	. . .	279	550	0.61
Florida	. . .	5,026	480	. . .	4,927	470	. . .	98	699	0.89
West Virginia	. . .	4,040	365	. . .	4,978	538	. . .	-937	633	0.14
Participants with earnings exceeding SGA (%)										
New York: CUNY	0.0	. . .	0.0	0.5	. . .	0.4	-0.5	. . .	0.4	0.11
New York: Erie County	3.5	. . .	0.9	3.4	. . .	0.9	0.1	. . .	1.3	0.95
Colorado	3.6	. . .	0.9	3.1	. . .	0.9	0.5	. . .	1.2	0.66
Maryland	5.3	. . .	1.1	4.7	. . .	1.1	0.5	. . .	1.5	0.73
Florida	3.3	. . .	0.8	2.3	. . .	0.7	1.0	. . .	1.1	0.37
West Virginia	2.9	. . .	0.8	2.5	. . .	0.8	0.3	. . .	1.1	0.76

Table 5.
Earnings of YTD participants 1 year and 2 years after random assignment, by site—*Continued*

Earnings characteristic and site	Treatment group			Control group			Difference			*p*-value
	Percent	Dollars	Standard error	Percent	Dollars	Standard error	Percentage points	Dollars	Standard error	
					2 years after random assignment					
Participants with any earnings (%)										
New York: CUNY	35.4	. . .	2.2	29.0	. . .	2.3	6.4**	. . .	3.1	0.04
New York: Erie County	38.1	. . .	2.3	36.7	. . .	2.5	1.4	. . .	3.3	0.67
Colorado	44.2	. . .	2.3	34.9	. . .	2.4	9.3*	. . .	3.3	0.01
Maryland	37.4	. . .	2.4	36.9	. . .	2.5	0.5	. . .	3.4	0.89
Florida	27.8	. . .	2.1	23.3	. . .	2.1	4.5	. . .	3.0	0.13
West Virginia	22.9	. . .	2.0	14.9	. . .	1.8	8.0***	. . .	2.7	0.00
Earnings among all participants ($)										
New York: CUNY	. . .	974	120	. . .	1,003	145	. . .	-29	187	0.88
New York: Erie County	. . .	2,177	254	. . .	1,817	224	. . .	361	346	0.30
Colorado	. . .	2,047	225	. . .	1,643	208	. . .	404	312	0.20
Maryland	. . .	3,729	366	. . .	3,761	394	. . .	-32	537	0.95
Florida	. . .	2,050	242	. . .	1,580	201	. . .	470	320	0.14
West Virginia	. . .	1,858	241	. . .	1,789	384	. . .	69	441	0.88
Earnings among participants with any earnings ($)										
New York: CUNY	. . .	2,748	295	. . .	3,427	418	. . .	-679	497	0.17
New York: Erie County	. . .	5,686	573	. . .	4,883	507	. . .	803	784	0.31
Colorado	. . .	4,599	447	. . .	4,698	500	. . .	-100	685	0.88
Maryland	. . .	5,985	510	. . .	6,081	553	. . .	-96	751	0.90
Florida	. . .	6,006	566	. . .	5,538	510	. . .	468	793	0.56
West Virginia	. . .	4,805	505	. . .	6,943	1,278	. . .	-2,138*	1,172	0.07
Participants with earnings exceeding SGA (%)										
New York: CUNY	2.2	. . .	0.7	2.0	. . .	0.7	0.2	. . .	1.0	0.83
New York: Erie County	4.6	. . .	1.0	4.7	. . .	1.1	-0.2	. . .	1.5	0.92
Colorado	4.7	. . .	1.0	4.4	. . .	1.0	0.3	. . .	1.4	0.82
Maryland	9.9	. . .	1.9	7.5	. . .	1.7	2.5	. . .	2.6	0.34
Florida	5.9	. . .	1.2	4.0	. . .	1.1	1.9	. . .	1.6	0.25
West Virginia	2.6	. . .	1.0	4.8	. . .	1.4	-2.2	. . .	1.7	0.19

SOURCE: Author's calculations using SSA program records.

NOTES: Earnings are shown in 2012 dollars.

Totals do not necessarily equal the sum of rounded components.

SGA = substantial gainful activity; . . . = not applicable.

* = statistically significant at the .10 level (two-tailed t-test).

** = statistically significant at the .05 level (two-tailed t-test)

youths had earnings, compared with 28.0 percent of control-group youths. In Maryland, although the result is not statistically significant, 58.8 percent of treatment-group youths had earnings, compared with 55.0 percent of the control group. Recall that the Maryland project served fewer youths receiving SSI payments than the other sites; that site was also located in a service-rich area. Those factors likely contribute to the absence of a statistically significant result. All of these results are similar to those given in the 12-month interim reports.

Two years after random assignment, both CUNY and West Virginia treatment-group youths continued to be more likely to have earnings than their control-group peers; in addition, Colorado treatment-group youths increased their labor force participation in the second year and were also more likely to have earnings than their control-group peers. The sustained impact of CUNY is somewhat surprising because the first-year impacts were thought to be partly due to the guaranteed employment experience. In all sites except Colorado, fewer treatment-group youths had earnings 2 years after random assignment than did after 1 year.

There was no significant difference in mean earnings or in the percentage of participants with earnings that exceeded the level identified by SSA as representing substantial gainful activity (SGA) in either the first or second year after random assignment for any site.[14] For differences in mean earnings, when looking only at participants with earnings, two results are statistically significant: CUNY after 1 year and West Virginia after 2 years. Surprisingly, both of those impacts are negative, indicating that the projects reduced the average earnings of the treatment groups. This outcome suggests that the projects may draw more "marginal" workers into the labor force; those workers would be expected to have low earnings, thus lowering the average for all those with earnings.[15]

To explore this hypothesis further, I looked at the earnings distributions of YTD youths in five earnings categories: $0; $1–$250; $251–$1,000; $1,001–$5,000; and over $5,000. Those results, available upon request, show that CUNY and West Virginia appear to have shifted workers out of the $0 group and primarily into the $1,001–$5,000 group in both years after random assignment. Similarly, Colorado (which had a significant impact on any earnings but an insignificant negative impact on average earnings of those with earnings in the second year) also appears to have shifted youths into the higher earnings categories in the second year. The other sites did not have statistically significant differences in the earnings distributions of the treatment and control groups.

SSA Program Payments

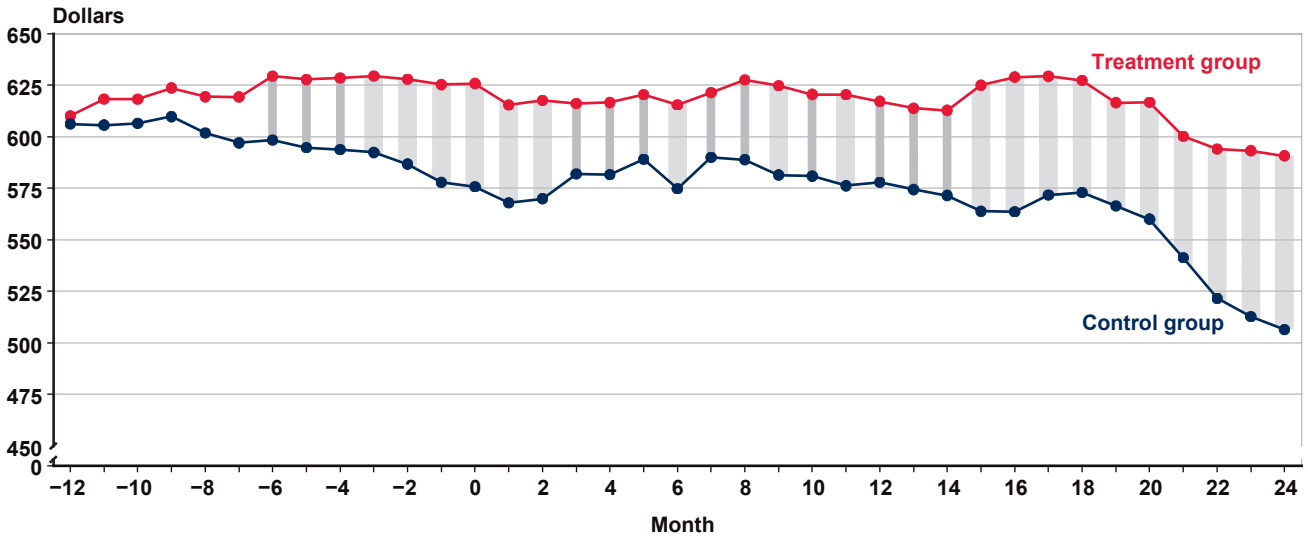
As Tables 3 and 4 show, fewer treatment-group youths left the SSI program than did control-group youths. Treatment-group youths may also have retained higher SSA program payments because they were eligible for special waivers that allowed them to keep more of their earnings and stay on a disability program longer than normal program rules allow. Specifically, the waivers exempted treatment-group youths from program payment reductions that were due to earnings—reductions to which control-group youths, under the normal program rules, remained subject.

Charts 3–8 show the average SSA program payments for the treatment and control groups for the 12 months before and the 24 months after random assignment, as well as for the month of random assignment itself (which is designated as month 0).[16] The vertical bars indicate months in which the difference is statistically significant.

The differences between the average SSA program payments for the treatment and control groups varied across sites. For CUNY, the difference was statistically significant for all months beginning in the sixth month before random assignment. By contrast, for Colorado, the difference was only statistically significant in two instances (months 10 and 17 after random assignment). Treatment-group payments tended to be significantly greater than those for the control group, especially in the second year after random assignment. For example, in Florida, the treatment group had statistically higher payments in all months after month 16; and in West Virginia, the treatment group had statistically higher payments in all months after month 9.

Only in Maryland, where most youths were not receiving any SSA program payments at random assignment, were the control group's payments consistently higher than the treatment group's payments over most of the period (although the difference is generally not statistically significant). Overall, the sites with significant increases in the percentage of youths with earnings in the first year (CUNY, Florida, and West Virginia; see Table 5) also had statistically significant higher SSA program payments at 12 months (differences of $39, $25, and $38, respectively; see Charts 3, 7, and 8). Those differences grew to $84, $61, and $61, respectively, at month 24.

Chart 3.
CUNY participants: Average amount of SSA program payments in 12 months preceding and 24 months following random assignment (in dollars)



SOURCE: Author's calculations using SSA program records.

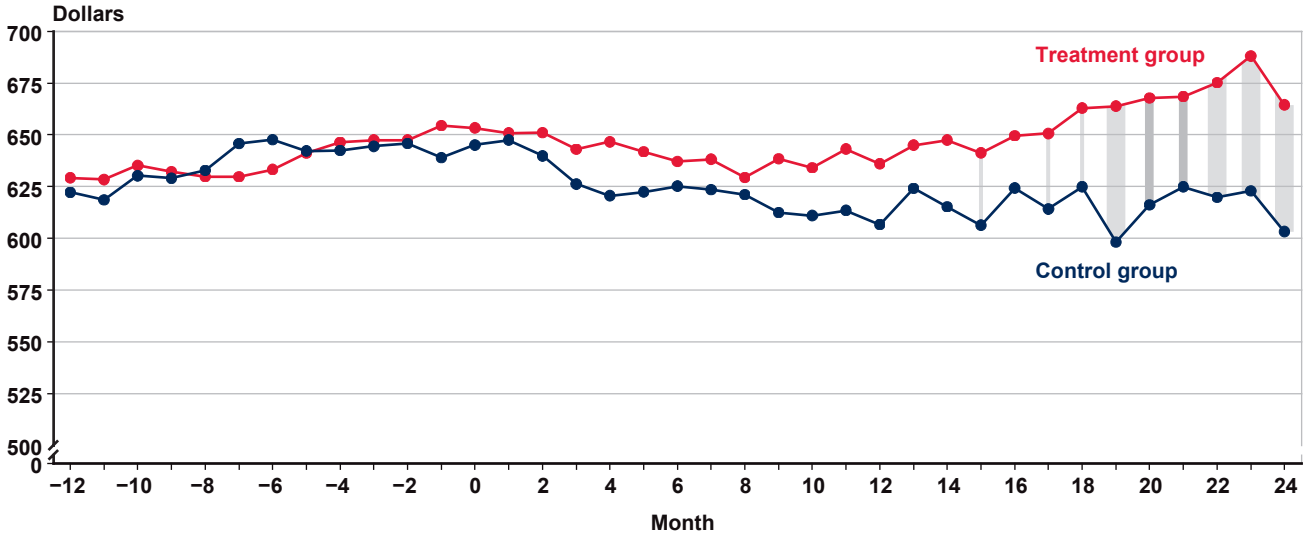
NOTES: Payments are adjusted to 2012 dollars using the CPI-W.

= difference is statistically significant at the .10 level (two-tailed t-test).

= difference is statistically significant at the .05 level (two-tailed t-test: months -6, -5, -4, 3, 4, 5, 7, 8, 10, 12, 13, and 14).

= difference is statistically significant at the .01 level (two-tailed t-test: months -3, -2, -1, 0, 1, 2, 6, 9, 11, 15, 16, 17, 18, 19, 20, 21, 22, 23, and 24).

Chart 4.
Erie County participants: Average amount of SSA payments in 12 months preceding and 24 months following random assignment (in dollars)



SOURCE: Author's calculations using SSA program records.

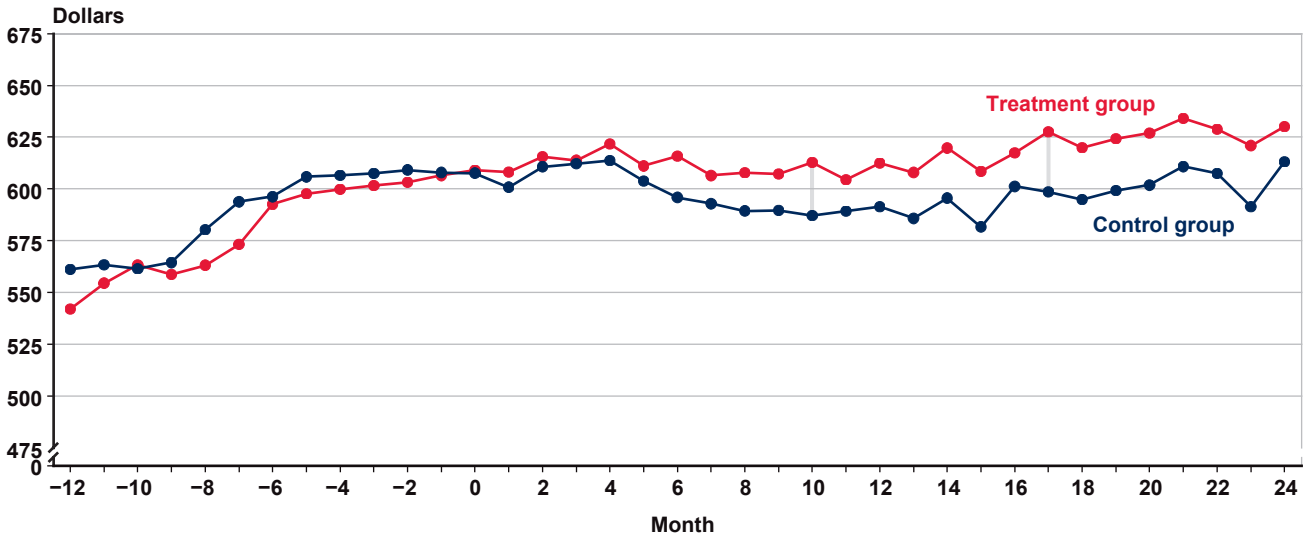
NOTES: Payments are adjusted to 2012 dollars using the CPI-W.

= difference is statistically significant at the .10 level (two-tailed t-test: months 15, 17, and 18).

= difference is statistically significant at the .05 level (two-tailed t-test: months 20 and 21).

= difference is statistically significant at the .01 level (two-tailed t-test: months 19, 22, 23, and 24).

Chart 5.
Colorado participants: Average amount of SSA program payments in 12 months preceding and 24 months following random assignment (in dollars)



SOURCE: Author's calculations using SSA program records.

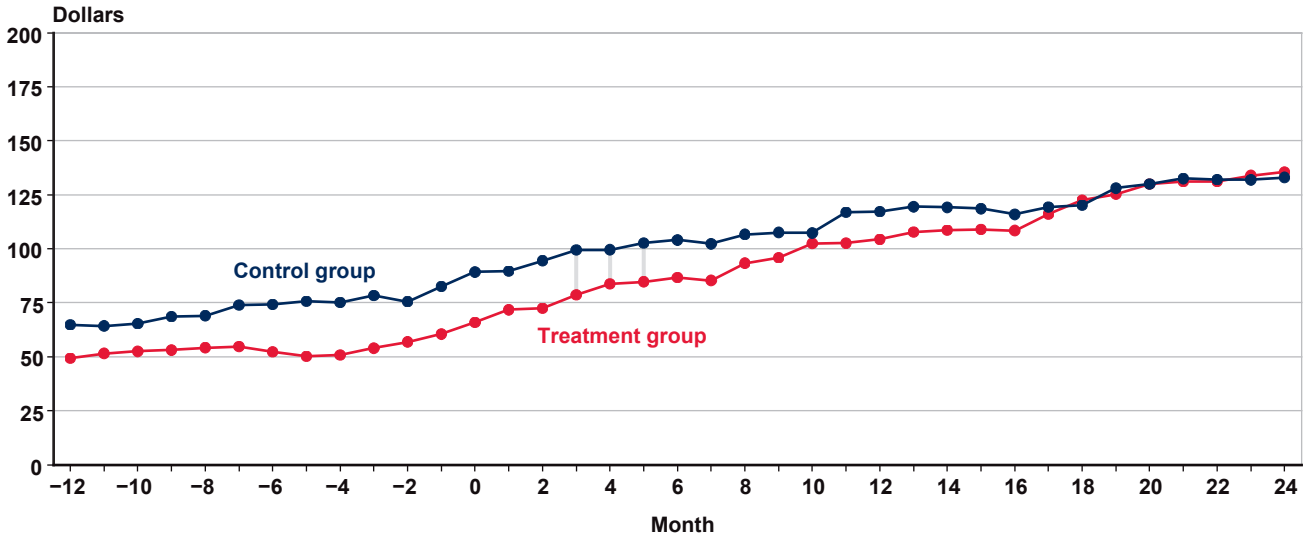
NOTES: Payments are adjusted to 2012 dollars using the CPI-W.

= difference is statistically significant at the .10 level (two-tailed t-test: months 10 and 17).

= difference is statistically significant at the .05 level (two-tailed t-test).

= difference is statistically significant at the .01 level (two-tailed t-test).

Chart 6.
Maryland participants: Average amount of SSA program payments in 12 months preceding and 24 months following random assignment (in dollars)



SOURCE: Author's calculations using SSA program records.

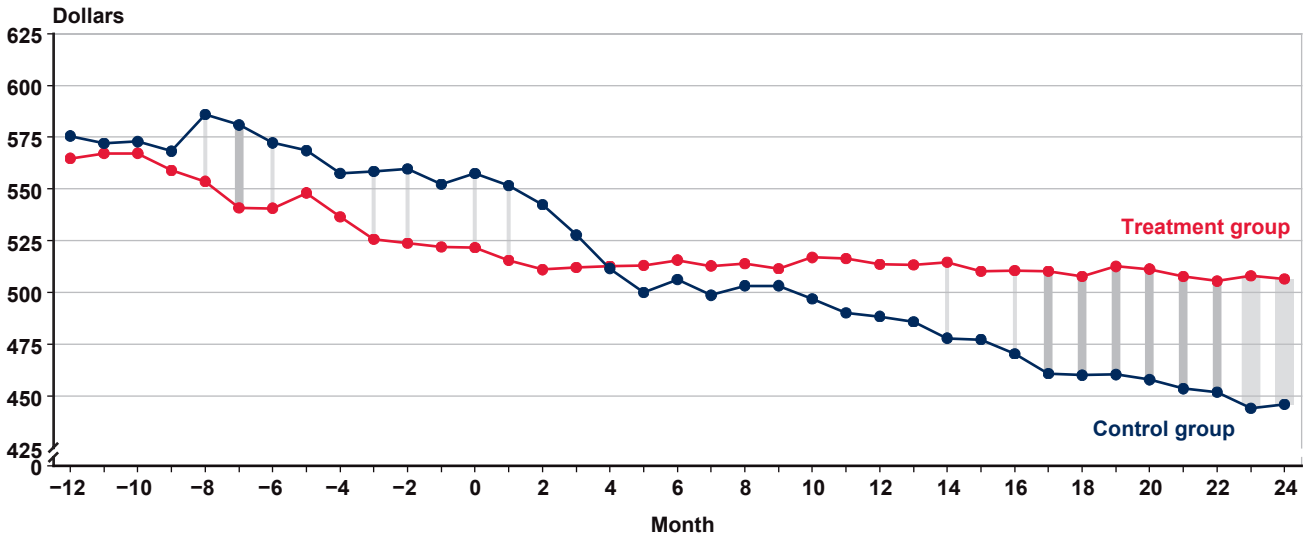
NOTES: Payments are adjusted to 2012 dollars using the CPI-W.

= difference is statistically significant at the .10 level (two-tailed t-test: months 3, 4, and 5).

= difference is statistically significant at the .05 level (two-tailed t-test).

= difference is statistically significant at the .01 level (two-tailed t-test).

Chart 7.
Florida participants: Average amount of SSA program payments in 12 months preceding and 24 months following random assignment (in dollars)



SOURCE: Author's calculations using SSA program records.

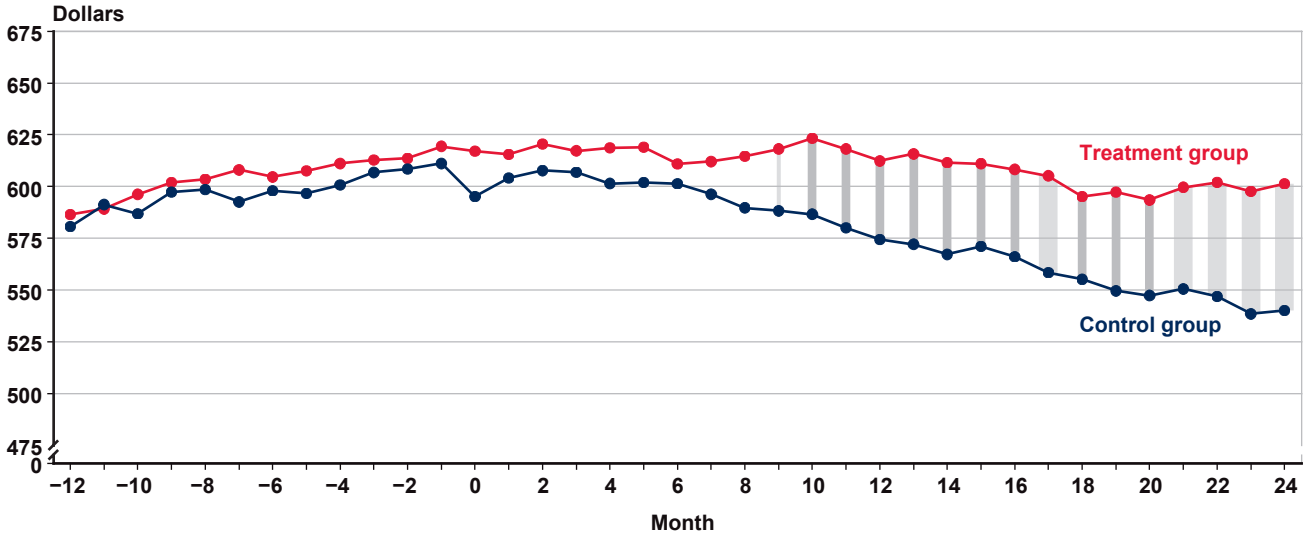
NOTES: Payments are adjusted to 2012 dollars using the CPI-W.

= difference is statistically significant at the .10 level (two-tailed t-test: months -8, -6, -3, -2, 0, 1, 14, and 16).

= difference is statistically significant at the .05 level (two-tailed t-test: months -7, 17, 18, 19, 20, 21, and 22).

= difference is statistically significant at the .01 level (two-tailed t-test: months 23 and 24).

Chart 8.
West Virginia participants: Average amount of SSA program payments in 12 months preceding and 24 months following random assignment (in dollars)



SOURCE: Author's calculations using SSA program records.

NOTES: Payments are adjusted to 2012 dollars using the CPI-W.

= difference is statistically significant at the .10 level (two-tailed t-test: month 9).

= difference is statistically significant at the .05 level (two-tailed t-test: months 10, 11, 12, 13, 14, 15, 16, 18, 19, and 20).

= difference is statistically significant at the .01 level (two-tailed t-test: months 17, 21, 22, 23, and 24).

Regression-Adjusted Results

Although youths were randomly assigned into the treatment and control groups, the two groups differed significantly at the baseline in many of the demographic characteristics reported in the 12-month interim reports (a few instances are identified in Table 2). Additionally, SSI payment or concurrent SSI/DI benefit receipt differed in the month of random assignment for CUNY and Maryland participants. Researchers often use multivariate regression analysis to improve the precision and efficiency of their estimates when there are known differences (Orr 1999).

The baseline surveys revealed differences between the treatment and control groups in several characteristics. Although the differences varied across sites, those characteristics included age; sex; race; language; living arrangement; self-reported health status; need for assistance in hearing, walking, or other functions; ability to perform certain independent activities; volunteer work, job training, and disability program payments received in the past year; paid work in the past month; expectations about future work; and parent's education and employment. Differences were generally small and within the number of differences one would expect to be due to chance. The Maryland site had the highest number of statistically different characteristics, with 6 (of 31 measured characteristics). However, those differences may substantially affect the results. The estimates presented in the 12-month interim reports were regression-adjusted, meaning these variables were included in a regression of the outcome on the treatment dummy. The coefficient on the treatment dummy is the regression-adjusted intent-to-treat impact.[17]

To test if known differences alter the results, I compared the raw-difference impacts with the impacts when controlling for sex, disability, age at random assignment, and the value of the outcome variable in the month of random assignment (results available upon request). In only two cases did an insignificant raw impact become significant with the regression adjustment (for Erie County, the receipt of DI worker benefits; and for Colorado, the receipt of SSI payments); and although the magnitude of those two impacts changed, in neither case did it change direction.[18] In one case, a significant raw impact became insignificant with the regression adjustment (for West Virginia, the receipt of auxiliary DI benefits). None of these changes substantially alters the general thrust of the raw-difference results.

Discussion

This article examines how YTD projects affect earnings and SSA disability program participation for youths 24 months after random assignment into treatment or control groups. The results are consistent with the objectives of the YTD project as a whole. Given the young age and minimal work experience of most YTD participants, the small change in earnings is unsurprising. However, the results suggest that some sites succeeded in moving marginal workers into the labor force, increasing the prevalence of earners but reducing average earnings amounts.

Fraker (2013) showed that, at least in the first year after random assignment, employment impacts were positively correlated with average intensity of employment services. Interestingly, for most sites, the share of the treatment group that had earnings dropped in the second year after random assignment, coinciding with the end of service delivery for most youths. For example, the share of youths with earnings in the CUNY treatment group dropped 13.0 percentage points, and in West Virginia, that share dropped 21.1 percentage points. Although the control groups generally mimicked that second-year drop in employment, these two sites sustained significant impacts on employment. Additionally, the significant impacts on the prevalence of earners only in the second year after random assignment in the Colorado site—after program services ended for most youths—suggest potential delayed project impacts. Whether the earnings impacts will rebound in future years or fade out completely remains an open question.

The differences between treatment and control groups in SSI participation increased over 24 months in all sites except Maryland. The higher shares of treatment-group youths receiving SSI payments and the generally higher payment amounts are consistent with the intent of the YTD project for SSI participants. The program waivers allow the treatment-group youths to keep more of their income and remain in the program longer than the control-group youths. Combined with the earnings results, the waivers may indicate better employment outcomes for treatment-group youths. Although not statistically significant, the negative SSI participation impact in Maryland is also consistent with the YTD's intent of reducing the need for SSI among youths not already receiving it. Future research will determine if project services and waivers improve longer-term employment outcomes.

It is too early to determine the overall success of the YTD project, but the results provide evidence of increased earnings and employment in some sites. Although this is an important outcome, the results presented here do not consider other sources of income, as would be necessary to more fully assess progress toward self-sufficiency. The 12-month interim reports took, and the final report will take, advantage of survey information on work experiences, living arrangements, and nondisability program transfer payments to provide greater insight on participant self-sufficiency.

The sample sizes for each YTD project site were chosen to enable the detection of impacts in the range of 7–12 percent (Rangarajan and others 2009). Thus, although the projects may have been well implemented, smaller impacts cannot be detected by design. For example, the differences between the treatment and control groups in the shares with earnings in the second year after random assignment range from 0.5 to 9.3 percent; only the estimates close to the lower bound of minimum detectable impacts (or the upper bound of the observed range of outcomes) are significant.

Prior studies on transition-age SSI recipients have generally reported observational outcomes (for example, Loprest and Wittenburg 2007). The YTD is one of the first initiatives to establish a causal relationship between services and employment for that population.[19] An earlier large-scale study, the State Partnership Initiative (SPI), included some projects that used an experimental design; interestingly, the few projects with those services were found to increase employment but not earnings, at least in the short term (Peikes and others 2005).[20] This outcome may have resulted from individuals limiting their earnings to maintain benefit eligibility (Wittenburg, Mann, and Thompkins 2013). Although the SPI tested versions of some of the program waivers included in the YTD, such testing took place at only one of the SPI sites that used an experimental design. For the SPI population, the effects of waiver availability (separate from services received) on earnings and program participation, along with any longer-term impacts, are unclear.

For two reasons, the YTD may yield more positive results as time passes. First, many project participants are or recently were still in school and thus may not yet have been ready or able to have substantial earnings. Although the phase 2 project sites focused more on competitive employment, all YTD sites encouraged work experiences, including unpaid internships and temporary employment. Benefits from those experiences may not be realized until much later, and thus may not yet be captured in SSA's program records. Second, the YTD's sustained impacts on SSI participation after services ended may indicate increased use of the SSI rule waivers that encourage work. The surveys informing the interim and final reports will provide greater detail on the experiences of treatment- and control-group youths up to 3 years after random assignment.

More extensive and focused analyses could help policymakers better understand how well YTD-type services work among those who use them. Planned future reports will use the baseline surveys to estimate 3-year project impacts, which may improve the estimates. YTD services were meant to enable youths to permanently change the path they would otherwise have followed. As such, any project impacts may last well into the future, and further examination of this population may yield important findings for similar future interventions.

Notes

Acknowledgments: The author thanks Chris Silanskis, Joyanne Cobb, Susan Kalasunas, Thomas Fraker, Arif Mamun, Paul Davies, Deborah Cortwright, and Chris Tamborini for their helpful comments on this article and an earlier paper.

[1] SSA previously funded similar projects such as the Transitional Employment Training Demonstration, Project NetWork, and the State Partnership Initiative, all of which served broader populations than that served by the YTD. The Department of Labor's Structured Training and Employment Transitional Services demonstration served a similar age range, but was limited to youths with low intelligence test scores who did not necessarily receive SSI or DI benefits. Those projects had varying impacts, but generally improved employment outcomes for participants. Wittenburg, Mann, and Thompkins (2013) summarize those and other employment programs for people with disabilities.

[2] A paper presenting preliminary results was based on partial data for three of the sites (Hemmeter 2012); this article fully updates that paper.

[3] See Bucks Camacho and Hemmeter (2013) for a review of the California and Mississippi projects.

[4] See SSA's Work Incentives Planning and Assistance website for more information on that program and the training its participants received (http://www.socialsecurity.gov/work/WIPA.html).

[5] One Stop Workforce Centers, sponsored by the Department of Labor's Employment & Training Administration, provide various job placement and related support services for both job seekers and employers.

[6] The position is named for the Department of Labor initiative under which it was established (for more information, see http://www.doleta.gov/disability/new_dpn_grants.cfm).

[7] The Summer Youth Employment Program is a subsidized employment program available on a lottery basis to all New York City youths.

[8] SSA generally pays amounts due when underpayments are detected but does not necessarily recoup all overpayments.

[9] For a review of this methodology, see Orr (1999) or Duflo, Glennerster, and Kremer (2006).

[10] The 12-month interim reports cited in the site descriptions assess project impacts based primarily on survey data using regression adjustments to control for several statistically significant differences between the treatment and control groups. Although MPR correctly implemented randomization, some differences between the treatment and control groups remained. Unfortunately, many of the differing characteristics are not available in the administrative records upon which the current analysis is based. Regression-adjusted results using the available administrative data and the differences in characteristics between the treatment and control groups are discussed later.

[11] Although DI eligibility typically requires 20 quarters of coverage (with 10 occurring in the past 5 years), that requirement is relaxed for younger individuals. Until age 22, youths need only 6 quarters of coverage to become eligible for DI.

[12] See Gertler and others (2011) for a discussion of the "difference-in-difference" estimate exemplified by this difference between the differences at the baseline and at month 24. This estimate controls for time-invariant heterogeneity in the sample by differencing out any underlying trends. In calculating this estimate, I find that YTD participation increased SSI payment receipt by 4 to 14 percentage points over 24 months; the impact on DI benefit receipt was much smaller, and sometimes negative.

[13] In the year of random assignment, the difference in the percentages of the treatment and control groups with earnings was significant in the Florida and West Virginia sites (5 and 7 percentage points, respectively; not shown). For the statistics included in Table 5, no other differences in employment-related outcomes in the year of random assignment were significant. Whether those differences reflect early program results cannot be determined, given the yearly measure of earnings.

[14] SGA is a monthly measure of the upper limit of work activity that precludes an individual from being determined disabled for SSI and DI eligibility. In 2012, the SGA amount was $1,010 for nonblind beneficiaries and $1,690 for blind beneficiaries. For DI beneficiaries, earnings above the SGA level can result in a suspension of benefits after a trial work period. For SSI recipients, earnings above the SGA level will not necessarily result in suspension of payments; rather, payments are generally reduced by $1 for every $2 earned above $65 until the payment reaches $0. For the purposes of this analysis, I annualize SGA by multiplying by 12.

[15] Consider the following simplified example: Given four people, two of whom work earning $5 and two of whom do not work, the average earnings of the workers equal $5 ([$5+$5]/2). If one nonworker starts to work, but earns only $2, then even though more people are working, average earnings drop to $4 ([$5+$5+$2]/3).

[16] Results in this article differ from those in the 12-month interim reports for several reasons. First, although all the interim reports used SSI payments due, those data may have changed as SSA became aware of new earnings information. Second, this article's use of actual DI benefit payments rather than DI amounts due may lead to some minor differences. Third, the authors of the interim reports adjusted payments for inflation to 2008 dollars, and this article adjusts payments to 2012 dollars using the CPI-W.

[17] Note that a regression without any other covariates would yield the same coefficient on the treatment dummy as the raw difference between the treatment and control groups.

[18] Similarly, the interim reports include a comparison of the raw and regression-adjusted estimates and find few instances in which the direction and significance of the results differed.

[19] See Luecking (2009) for a review of the evidence on the role of employment services and experiences for youths in the transition from school to work.

[20] SPI projects typically served SSI recipients or DI beneficiaries aged 18–65, so the comparability of some results with those of the YTD may be limited. Furthermore, participation at some SPI sites was restricted to persons with mental disorders, those receiving vocational rehabilitation services, or other groups (Peikes and others 2005).

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Immigrants and Retirement Resources

by Purvi Sevak and Lucie Schmidt*

The extensive literature documenting differences in wages between immigrant and native-born workers suggests that immigrants may enter retirement at a significant financial disadvantage relative to workers born in the United States. However, little work has examined differences in retirement resources and retirement security between immigrants and natives. In this article, we use data from the Health and Retirement Study linked with restricted data from the Social Security Administration to compare retirement resources of immigrants and natives. Our results suggest that while immigrants have lower levels of Social Security benefits than natives, when holding demographic characteristics constant, immigrants have higher levels of net worth. The estimated immigrant differentials vary a great deal by number of years in the United States, with the most recent immigrants being the least prepared for retirement.

Introduction

An extensive literature in economics documents that immigrants receive lower wages than native-born workers with similar characteristics.[1] Those gaps in wages imply that immigrants may enter retirement at a significant financial disadvantage relative to US natives. However, much less work has examined differences in retirement resources and retirement security between immigrants and natives. This topic is important because immigration has often been suggested as a way to improve, at least temporarily, the finances of a pay-as-you-go Social Security program (see, for example, Lee and Miller (2000), Storesletten (2000), and Board of Trustees (2010)). This approach to improving the financial stability of Social Security can be particularly effective in a system with many illegal immigrants who may pay Social Security taxes but never claim benefits (Goss and others 2013). Understanding how immigrants as a whole fare when they reach retirement is an important consideration when evaluating the long-term costs and benefits of any changes in immigration policy. Although Social Security program rules are neutral in that they do not insure one group differentially from another, it is important to understand the extent to which old-age outcomes might differ for a large immigrant population.

In this article, we use the Health and Retirement Study (HRS) to compare retirement resources of immigrants with those of the native born. Most research on the wealth of immigrants nearing retirement ages does not examine the potential role of Social Security. This is an important omission because Social Security benefits are the most important source of income for most retired Americans. We use HRS data linked with restricted-access data on earnings histories from Social Security administrative records to estimate future Social Security benefits for respondents who have not yet reached retirement age. Then, we supplement those estimated benefits with

Selected Abbreviations

HRS	Health and Retirement Study
PIA	primary insurance amount

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self-reported data on actual Social Security benefits for respondents aged 65 or older, as well as data on pension coverage, housing, and total net worth. We document differences in retirement resources between immigrants and natives and then explore the role of economic and demographic characteristics in explaining those differentials. Finally, we look at differences in retirement resources of immigrants based on the number of years they have spent in the United States.

We find that working-age immigrants have lower predicted Social Security benefits than natives and that immigrants over the age of 65 have lower self-reported actual Social Security benefits. These differentials are statistically significant and remain so even after controlling for a number of demographic and socioeconomic characteristics such as education, self-reported health, census division, and race and ethnicity. However, there is wide variation in duration of residence in the United States among HRS respondents, and we find that the immigrant differentials in expected resources from Social Security depend greatly on the number of years they have spent in the country. Although immigrants in the HRS who have been in the United States for the median number of years (about 36) or less have substantially lower expected resources from Social Security than natives, that gap decreases with additional years of US residency. Furthermore, we find that the gap is due to fewer quarters of work in Social Security–covered employment, rather than lower earnings during covered quarters.

The differences in Social Security income may not lead to lower retirement security if immigrants compensate for them with higher private wealth accumulation. We find that average net worth is substantially lower for immigrants. However, when we hold education, race, ethnicity, and other demographic characteristics constant, the foreign born have substantially *higher* net worth than native-born respondents with the same characteristics, suggesting that this wealth gap is due to differences in characteristics and not immigrant status per se. Our back-of-the-envelope calculations suggest that at the sample median years of residence in the United States, after controlling for individual characteristics, immigrants in our sample have amassed private wealth to offset about 50 percent of their lower predicted Social Security benefits.[2] These findings add to a growing literature that documents a great deal of variation in economic well-being within the immigrant population. They also identify a particularly vulnerable group with respect to retirement security—recent immigrants nearing retirement.

Why Might Immigrants Have Lower Retirement Resources?

Families rely on three main types of resources during retirement: (1) Social Security income, (2) pensions, and (3) private savings and wealth. Traditionally, this has been referred to as the "three-legged stool" of retirement security (Cutler 1996). There are a number of reasons why each of these resources might be expected to differ between immigrants and natives. Earnings are a primary determinant of both Social Security benefits and private wealth. Previous research documents significantly lower earnings for immigrants than for natives. A large fraction of this differential can be explained by differences in observable socioeconomic characteristics (Borjas 1999). In addition, country of origin has a large effect on immigrant/native earnings differentials (Duleep and Dowhan 2008; Abramitzky, Boustan, and Eriksson 2012). Finally, evidence suggests that entry earnings of more recent immigrants have been declining across cohorts. Some researchers have interpreted that decline as a decrease in immigrant quality (Borjas 1985, 1987, 1992), while others have argued that it reflects changes in the transferability of skills from the host country to the United States (Duleep and Regets 2002).

Under current Social Security rules, workers who have immigrated to the United States are likely to receive lower benefits than natives. Because Social Security requires 40 quarters of covered earnings before an individual is eligible to receive any benefits, many immigrants may not meet eligibility requirements. Those workers who are eligible for Social Security may have lower benefits either because they have earned fewer quarters of coverage than natives or because they have worked "off the books." Indeed, Cohen and Iams (2007) find that immigrants in the Survey of Income and Program Participation (SIPP) are less likely to receive Social Security benefits, and Favreault and Nichols (2011) find that about 20 percent of male immigrants have made contributions to the system but are not eligible for benefits.[3] In addition, because benefits are based on average earnings over the 35 years of highest earnings, even immigrants and natives with identical earnings at retirement may have large differences in Social Security benefits, if immigrants are more likely to have years without Social Security–covered earnings.

However, the redistributive nature of Social Security may mean that many immigrants realize a higher rate of return on payroll tax contributions than US natives because immigrants have fewer years of

covered earnings (Gustman and Steinmeier 2000). This is confirmed by Favreault and Nichols (2011), who find that immigrants who receive benefits are likely to receive higher replacement rates. Furthermore, recent work by Borjas (2011) suggests that immigrants who arrive in the United States at older ages may have higher employment rates than same-age, native-born workers, in part to accumulate the necessary work credits for Social Security.

Despite the fact that immigrants may have fewer quarters of Social Security–covered earnings and therefore lower Social Security benefits than natives, their retirement resources may still be adequate if they compensate for those differences with greater private wealth accumulation. Previous research notes that Social Security and private savings could be substitutes for each other (see Feldstein (1974) and CBO (1998) for a review of this literature). In addition, conditional on earnings, private wealth accumulation could vary between immigrants and natives because of differences in savings rates (resulting from differences in preferences for savings or differential consumption and expenditure patterns) or because of differences in rates of return.

However, evidence suggests that immigrants have lower savings rates than natives (Carroll, Rhee, and Rhee 1994). In addition, there may be measurement issues associated with comparing savings rates of immigrants with those of natives. For example, Hispanic immigrants are more than twice as likely as natives to have provided financial assistance to family members (both in and out of the United States), and they are more likely to expect their retirement years to be financed by income of other family members (Kamasaki and Arce 2000). Although these intergenerational transfers may be undocumented in standard data sets, for many immigrants these transfers may be a major component of retirement saving and planning.[4]

Furthermore, immigrants exhibit substantially different portfolio allocations than do natives, in ways that we would also expect to lead to differences in net worth. Previous research finds that immigrants are less likely to own a broad array of financial assets (including the simplest forms of assets—such as savings and checking accounts) than the native born (Osili and Paulson 2007), and that they hold a much higher proportion of their net worth in automobiles than in financial or housing assets (Cobb-Clark and Hildebrand 2006). Evidence on Hispanic immigrants suggests that they tend to save more for short-term goals such as a home purchase, and that they are extremely risk averse, placing greater importance on safety than on the rate of return on investments (Kamasaki and Arce 2000). These differences in savings behavior and portfolio allocation across immigrant groups contribute to a great deal of observed variation in net worth and retirement well-being (see, for example, Cobb-Clark and Hildebrand (2006) and Favreault and Nichols (2011)).

Our research adds to the literature on immigrants and wealth with a focus on retirement security. We use data from the HRS linked to restricted-access Social Security administrative data, which allows us to examine a broader set of resources available to immigrants at retirement. We examine immigrant/native differentials in Social Security, looking at both *expected* benefits or primary insurance amounts (PIAs) for immigrant workers aged 51–61 and *actual* self-reported Social Security income for those aged 65 or older. The PIA is the benefit a person would receive if he or she chose to begin receiving Social Security benefits at his or her normal retirement age.[5] We also examine measures of private wealth accumulation, including pension coverage, housing, and net worth. We then explore whether the differentials in those measures can be explained by a number of demographic and socioeconomic factors and whether they vary in magnitude by the number of years spent in the United States.

Data and Methodology

To examine immigrant differences in retirement resources and retirement timing, we use data from the HRS. In 1992, the HRS interviewed individuals born from 1931 through 1941 (aged 51–61) and their spouses or partners, and it has reinterviewed those respondents every 2 years since. In 1998 and every 6 years after that, additional birth cohorts were added to the HRS. Also in 1998, respondents in the Asset and Health Dynamics Among the Oldest Old (AHEAD) study were added to the HRS, making it a representative sample of US residents aged 51 or older. For most of our analysis, we use samples of respondents interviewed in 1998, 2000, 2002, or 2004 because the combination of HRS respondents and the newly added AHEAD respondents gives us a representative sample of people older than age 65. We restrict our sample differently when examining expected Social Security benefits because Social Security earnings histories were collected only for HRS respondents who were aged 51–61 when they were first interviewed in 1992 or 1998.

The HRS has a number of advantages for conducting this type of analysis relative to other data sets. In every wave, the survey asks about income from a

variety of sources, labor supply, and levels of different types of assets and financial accounts. In many surveys, respondents find questions on asset holdings difficult to answer, leading to significant problems with nonresponse and measurement error (Smith 1995). Respondents may believe that the surveyor wants an exact measure of their wealth, and they provide a precise but inaccurate estimate. Respondents may also find questions asking for a precise measure of their wealth too intrusive. As a result, the wealth data in many surveys are viewed with skepticism. As described in detail in Smith (1995), the HRS survey design specifically tried to minimize such biases by using unfolding brackets to obtain ranges of asset values when individuals refused to report exact values or said they did not know the exact value. Equally important is the fact that the HRS can be merged to respondents' actual Social Security earnings histories through restricted access, making it possible to estimate future Social Security benefits for respondents who have not yet started collecting them.

Our primary focus is on the financial resources that individuals will have access to in their retirement. Our unit of analysis in this article is the individual, and we stratify our analysis by both sex and marital status. This allows us to compare the financial resources of immigrants with those of natives for four distinct subgroups—married men, married women, unmarried men, and unmarried women. Because most of the married men in our sample are coupled with married women in the sample, in some sense we are double counting those families. However, by looking separately at the married men and the married women, we can estimate the relationships between wealth, immigrant status, and distinguishing traits by using individual characteristics of either the husband or the wife, and we can therefore examine differences in these relationships by sex.

Most control variables—including race, ethnicity, education, and self-reported health status—are for the individual in question. However, the HRS, like most data sets, measures wealth at the family level. As a result, our wealth measures for married individuals are at the family level rather than at the individual level. In addition, our unmarried subsamples include the never married, divorced, and widowed. In interpreting our results, it is important to keep in mind that the composition of these groups is affected by immigrant/native differences in mortality, marriage, and divorce. Sevak and Schmidt (2008) document lower age-specific mortality rates for immigrants than for natives. Immigrants are less likely than natives to have never been married, are more likely to be married, and are less likely to divorce (Grieco and others 2012).

We examine three major sources of retirement income—Social Security benefits, pension coverage, and private wealth. To calculate future eligibility and expected benefits, we merge HRS data with Social Security administrative records on covered earnings. The records, which are available for roughly 75 percent of the sample, report annual earnings (up to a yearly maximum) in sectors covered by Social Security from 1951 through 1991 for respondents born in the 1931–1941 period and from 1951 through 1999 for respondents born in the 1942–1947 period.

We use self-reported data in the HRS for earnings beyond those years, and we then impute earnings into the future for individuals who have not reached age 62 by 2004 (that is, individuals born in the 1943–1947 period). To do so, we assume that the individual's labor force status remains the same as that in 2004; for those who were working, we use a flat inflation-adjusted earnings profile until age 62.[6] One concern is that the self-reported earnings may have measurement error that is lacking from the administrative data. However, as discussed earlier, the HRS was designed and updated with a great emphasis on accurate measurement of financial variables. Gustman and Steinmeier (1999) find that PIAs calculated from self-reported earnings in the HRS overstate those calculated from the restricted Social Security administrative earnings records by 5.8 percent on average. However, this varies largely by sex. PIAs for men calculated from self-reported earnings are only 1.4 percent overstated relative to those calculated from the administrative data, while for women they are overstated by 13.5 percent.

We apply the rules used by Social Security to calculate eligibility and the PIA formula. In reality, the actual benefits are a function of the PIA but will vary based on the exact year and age of entitlement, as well as on marital status. Because we want to compare potential benefits across individuals of different ages, holding constant birth year, marital status, any changes in Social Security eligibility age, and actual retirement age, we use the PIA itself rather than projected benefits and apply Social Security rules for individuals reaching age 62 in a fixed year (2006) to calculate the PIA. This allows us to isolate differences in PIA that are due to work history.

We calculate the PIA at the individual level, based on an individual's earnings history, even for married

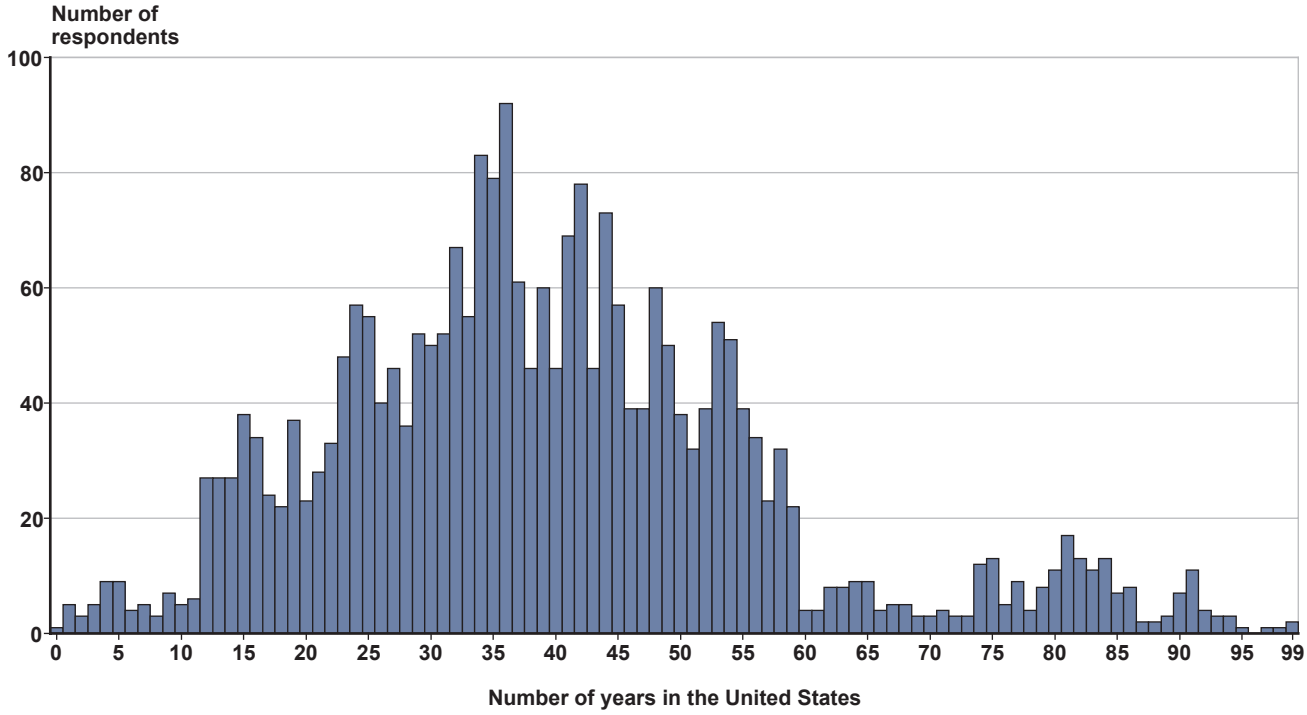
respondents. As a result, our PIA measures are not confounded by the fact that some individuals may receive Social Security retirement benefits as dependent benefits from their spouses, rather than benefits based on their own earnings histories. For respondents older than age 65, we examine current self-reported Social Security income. Although the PIA is based solely on an individual's earnings history, actual self-reported Social Security benefits include dependent benefits received based on a spouse's earnings history as well.

We then examine differences in pension coverage as reported by respondents in the HRS. Lastly, we look at measures of private wealth, examining an indicator for home ownership, measures of home equity, and total net worth. Net worth includes home equity, other real estate, stocks, bonds, individual retirement accounts (IRAs), businesses, farms, balances in checking and savings accounts, certificates of deposit (CDs), automobiles, trusts, and other assets—net of debts. It is worth nothing that our data do not include information on the earnings or expected pension benefits of immigrants in their countries of origin, and as such, we might understate their total available retirement resources.

Approximately 2,900 age-representative HRS respondents (those born in 1947 or earlier), or roughly 10 percent of the sample, are foreign born. We plot the distribution of immigrants by the number of years they have been in the United States (Chart 1). The median number of years in the United States is 36. However, there is substantial variation across the sample. Some respondents immigrated as children, others in their working years, and others as seniors.

Table 1 provides a snapshot of how retirement resources vary between the US-born and foreign-born respondents in our HRS analysis sample. The first seven variables measure the various forms of retirement income or wealth that we focus on in this article: (1) the PIA for respondents who have not yet retired, (2) self-reported Social Security benefits for those older than age 65, (3) pension coverage rates, (4) family net worth, (5) home ownership, (6) home equity, and (7) nonhousing family net worth. However, because economic well-being at retirement is not strictly limited to those variables, we look at a number of additional factors. We compare family income to see whether and to what extent immigrants and natives differ in terms of the contributions of family members. We also compare the current labor force activity of immigrants with that of natives by examining own earnings and indicators for whether the respondent is retired or working. The sample for each of those comparisons differs depending on the variable of

Chart 1.
Distribution of immigrants in the HRS, by number of years in the United States, 2004



SOURCE: Authors' tabulations using the Health and Retirement Study.

interest. For most variables, the unit of observation is an HRS respondent older than age 51 in 1998, 2000, 2002, and 2004. For the PIA, the sample includes only respondents who were aged 51–61 in 1992 or 1998. Self-reported Social Security benefits are only calculated for respondents older than age 65, while pension coverage is only reported for those workers younger than age 65.

For almost all indicators of financial well-being, immigrants are worse off than the native born. Among the married male subsample, immigrants aged 51–61 have a forecasted monthly PIA that is $316 lower than that of native-born respondents. Similarly, married male immigrants aged 65 or older have realized annual Social Security benefits that are $3,069 lower than those of similar natives. Married male immigrants are 11 percentage points less likely to have pension coverage and 14 percentage points less likely to be homeowners. The net worth of immigrant families is almost $100,000 less than that of native families. The one exception to those patterns is in the area of home equity; conditional on home ownership, mean home

Table 1.
Comparison of US natives with immigrant HRS respondents aged 51 or older, by selected characteristics and years—1998, 2000, 2002, and 2004 (except where noted)

	Men		Women	
Selected characteristic	US native	Immigrant	US native	Immigrant
		Married		
Social Security PIA ($) [a]	1,504	1,188 **	733	582 **
Actual annual (family) Social Security benefits ($) [b]	15,142	12,073 **	16,175	13,596 **
Pension coverage (%) [c]	60	49 **	56	47 **
Family net worth ($)	375,335	276,744 **	376,682	282,752 **
Homeowner (%)	88	74 **	88	75 **
Family home equity ($) [d]	137,679	143,204 *	138,923	146,674 **
Nonhousing family net worth ($)	331,106	218,217 **	323,714	241,440 **
Family income ($)	69,322	58,030 **	66,471	55,788 **
Own earnings ($)	24,947	22,525 *	10,330	8,458 **
Retired (%)	58	47 **	36	25 **
Working (%)	45	46	36	32 **
		Unmarried		
Social Security PIA ($) [a]	1,227	955 **	855	580 **
Actual annual (family) Social Security benefits ($) [b]	9,960	8,503 **	8,861	7,164 **
Pension coverage [c] (%)	49	33 **	55	42 **
Family net worth ($)	208,500	158,852 **	167,751	139,904 **
Homeowner (%)	57	41 **	59	44 **
Family home equity ($) [d]	111,844	139,204 **	104,880	136,576 **
Nonhousing family net worth ($)	206,296	158,569	113,130	70,079 **
Family income ($)	41,427	30,507 **	24,324	20,105 **
Own earnings ($)	16,964	13,627	7,260	6,749
Retired (%)	59	58	49	39 **
Working (%)	33	32	26	23 **

SOURCE: Authors' tabulations using the Health and Retirement Study matched to Social Security administrative records.

NOTES: Financial variables are in 2006 dollars.

* Means/medians are significantly different from one another at the 10 percent level.

** Means/medians are significantly different from one another at the 5 percent level.

*** Means/medians are significantly different from one another at the 1 percent level.

a. Among respondents aged 51–61 in 1992 or 1998.

b. Among respondents aged 65 or older (includes spousal benefits if applicable).

c. Among respondents younger than age 65 in 1998 and 2000.

d. Among homeowners.

equity of immigrants is about $5,500 higher than that of nonimmigrants. Both family income and own earnings are lower for married male immigrants than for their native-born counterparts. Interestingly, married male immigrants are less likely to report being retired, but are not more likely to be currently working.[7] In most of these cases, the noted immigrant differences are statistically significant at the 5 percent level or higher.

The distinction between our measure of PIA (calculated at the individual level) and actual Social Security benefits (reported at the family level) shows up clearly when comparing patterns between married men and married women (Table 1). The PIAs for married women are roughly half those for married men, but actual Social Security benefits are higher for married women. This asymmetric treatment should be kept in mind when interpreting results for these variables.

Patterns for PIA, Social Security benefits, pension coverage, and private wealth are similar for the other subsamples (married women, unmarried men, and unmarried women). The major differences that emerge when stratifying by both sex and marital status pertain to employment. Both married and unmarried female immigrants are less likely to report retirement and less likely to be currently working than native-born women of the same marital status.[8] Unmarried male immigrants have similar rates of retirement and labor force participation to those of their native-born counterparts, and they also have similar levels of own earnings. Table 2 provides summary statistics for our sample across other variables used in this analysis. Immigrants are more likely to be Hispanic, to have fewer years of education, more children, and worse self-reported health[9] than natives.

Table 2.
Summary statistics for HRS respondents aged 51 or older, by selected characteristics and years—1998, 2000, 2002, and 2004

Selected characteristic	Immigrants	US natives
Number of years in the United States	38.04	. . .
	(16.93)	
Female	0.59	0.57
Age	67.97 **	68.31
	(10.78)	(10.33)
Black	0.09 **	0.14
Hispanic	0.45 **	0.04
Education (years)	9.88 **	12.27
	(4.96)	(3.10)
Number of children	3.49 **	3.20
	(2.45)	(2.18)
Self-reported health	3.14 **	2.87
	(1.15)	(1.15)
Family income (in 2006 $)	44,587 **	53,731
	(117,878)	(85,494)
Currently working	0.34	0.36
Retired	0.39 **	0.49
Age at retirement (censored)	61.60 **	59.81
Number of observations	7,058	68,731

SOURCE: Authors' tabulations using the Health and Retirement Study.

NOTES: Selected standard errors are in parentheses.

. . . = not applicable.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

In one sense, the raw immigrant/native differentials presented in Table 1 tell much of the story. In the United States, immigrants have substantially lower levels of retirement resources. This has important implications for public policy. However, we know from previous research that there are large differences in wealth by sex (Schmidt and Sevak 2006; Edlund and Kopczuk 2009), race (Blau and Graham 1990; Barsky and others 2002), education (Behrman and others 2010), and self-reported health (Smith 1999; Attanasio and Hoynes 2000). Appropriate policies designed to improve immigrants' preparedness for retirement might depend on whether the raw wealth gaps are driven by differences in these characteristics or immigrant status per se. As a result, it is important to know to what extent the immigrant/native differences highlighted in Table 1 can be explained by differences in selected characteristics between the immigrants and natives in our sample.

To this end, we estimate a series of multivariate regressions for the different measures of retirement resources separately for each of the four sex/marital-status groups. We first estimate the following:

$$Outcome_i = \alpha + \beta_1 Immigrant_i + X_i \gamma + \varepsilon_i ,$$

where we regress our outcome measures on an indicator of whether the individual is an immigrant. The X vector controls for a number of additional variables correlated with both wealth and immigrant status. Those variables include a quadratic in age, number of children, and self-reported health status.[10] We also include a control for years of education as a proxy for permanent income, which should matter for savings decisions. We control for census division to account for spatial clustering of immigrants in particular areas of the country. We also control for race and Hispanic ethnicity. These characteristics are closely associated with country of origin among immigrants, and previous work has shown a significant amount of heterogeneity in immigrant outcomes depending on country of origin (for example, Cobb-Clark and Hildebrand (2006); Duleep and Dowhan (2008); Favreault and Nichols (2011); Abramitzky, Boustan, and Eriksson (2012)). However, we are unable to control directly for country of origin in our analysis.[11] The regressions also include survey year fixed effects. For most regressions, we calculate Huber-White robust standard errors that are clustered at the respondent level; this accounts for the fact that we have multiple observations for respondents within our sample in those regressions, and therefore our errors are likely to be correlated for a given individual across survey waves.[12]

Finally, we also exploit the fact that the HRS notes the year of immigration to test for differential effects for those immigrants who have been in the United States for longer periods. We reestimate our specification adding a quadratic in years in the United States.[13] There is some debate in the earnings literature on how to interpret the estimated effect of number of years in the United States. Some have interpreted the effect as evidence of assimilation, but in a cross-sectional analysis, it may be driven by changes in the characteristics or skills of successive cohorts of immigrants, or changes in the relationship between skills and US economic outcomes (see Borjas (1999) for a detailed discussion). Examining repeated cross-sectional data allows a researcher to differentiate between assimilation effects and cohort-of-arrival effects, but the availability of those data is restricted by the limited number of cohorts currently included in the HRS.[14] Because wealth is a function of earnings, consumption, and savings over all prior years, the estimated difference by number of years in the United States may reflect the effects of assimilation on earnings, consumption, and savings in each successive year the immigrant has been in the country. However, it could also reflect differences in the characteristics of immigrant cohorts. To the extent that those cohort differences are captured in differences in health status or education, we can control for them in our empirical work, but we cannot rule out the possibility that these estimates are driven by unobserved differences in the characteristics of immigrant cohorts over time.

As Borjas (2011) notes, it can be difficult to interpret the coefficients on a quadratic in number of years in the United States. To facilitate interpretation of our results, we evaluate the wealth gap implied by these coefficients at three specific points in the distribution of years in the United States—25th percentile, median, and 75th percentile. At each of those points, we perform a Wald test to determine whether our model predicts significant differences in a given outcome measure between natives and immigrants for the given number of years.

Results

Because the primary source of retirement income for most individuals in the United States is Social Security, we first look at differences in Social Security benefits between immigrants and natives and then examine pension coverage and wealth among immigrant and native workers, encompassing what has traditionally been referred to as the three-legged stool.

Differences in Social Security Benefits

Table 1 shows that, in each subsample, immigrants have substantially lower monthly PIAs, and therefore they have lower expected Social Security benefits than do natives. Table 3 looks at those differences in a regression framework, with controls for age, education, self-rated health, number of children, census division, and race and ethnicity. Estimated coefficients on those control variables are in the expected direction.[15] For all subgroups, each additional year of education is associated with an increase in the PIA. Self-rated health, which ranges from 1 for "excellent health" to 5 for "poor health," is also correlated with the PIA, such that respondents in worse self-reported health have lower expected benefits, consistent with the well-documented relationship between health and earnings (for example, Smith (1999)). For married men, when all covariates are included, the estimated expected monthly benefit is $231 lower for immigrants than for natives (compared with a raw gap of $316 without covariates, as shown in Table 1). The magnitude of this differential remains large, given mean expected monthly Social Security benefits of approximately $1,500 for native-born married men.

Similar patterns are evident for the other subgroups. For married women, the raw PIA gap shown in Table 1 is $151 and falls to $77 after including all control variables. For unmarried men and women, the raw gaps

Table 3.
Estimated monthly Social Security benefit for HRS respondents aged 51–61 in 1992 or 2008, overall and by percentile number of years in the United States (in dollars)

Characteristic	Married		Unmarried	
	Men	Women	Men	Women
	Without controlling for quarters of covered earnings			
US native (reference category)	1,505	734	1,227	854
Model-predicted immigrant difference	-231 **	-77 **	-169 **	-158 **
	(30)	(32)	(69)	(44)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	-298 **	-121 **	-473 **	-235 **
	(36)	(40)	(87)	(54)
50th	-97 **	-21	-133	-116 **
	(36)	(37)	(82)	(55)
75th	16	12	29	-61
	(35)	(38)	(80)	(53)
	Controlling for quarters of covered earnings			
US native (reference variable)	1,505	734	1,227	854
Model-predicted immigrant difference	136 **	93 **	159 **	89 **
	(17)	(15)	(37)	(24)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	187 **	114 **	168 **	118 **
	(21)	(19)	(49)	(30)
50th	199 **	111 **	167 **	114 **
	(21)	(18)	(44)	(29)
75th	158 **	94 **	164 **	86 **
	(20)	(18)	(43)	(28)

SOURCE: Authors' tabulations using the Health and Retirement Study matched to Social Security administrative records.

NOTES: The model predicts differences in PIA between US natives and immigrants. It includes a measure for number of years since immigration and its quadratic—controlling for age, race, education, census division, year of immigration, number of children, and health. Standard errors are in parentheses.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

in PIAs are similar in magnitude, at roughly $273. Adding controls in each case reduces the monthly PIA gap by over $100—$169 for the men and $158 for the women.

In Table 3, we also examine how the immigrant/native estimated differential in the PIA varies by number of years in the United States—evaluated at the 25th, 50th, and 75th percentiles of years spent in the country. These estimates suggest that for married men, the immigrant differential in the PIA is negative and significant throughout a good part of the distribution of years spent in the United States. Immigrants in the United States at the 25th percentile number of years have a PIA that is $298 lower than that of natives. At the median, the immigrant gap in the PIA is much smaller, at $97, but still statistically significant at the 5 percent level (throughout the remainder of the text, references to statistical significance signify the 5 percent level). At the 75th percentile, the gap actually turns positive, but is not statistically different from zero. These results suggest that while there are large and statistically significant differences in PIAs between married male immigrants and natives, there is a great deal of heterogeneity across immigrants depending on how long they have been in the United States. As noted earlier, this could be due to assimilation or to changes in the characteristics of immigrant cohorts over time. Results are qualitatively similar for the other subgroups. Years spent in the United States appear to be most important for unmarried men, where the PIA for immigrant men at the 25th percentile of years in the United States is 61 percent of the mean benefit for the native born.

Our PIA results suggest that immigrants nearing retirement are likely to have lower future Social Security benefits than natives, even after controlling for a wide array of socioeconomic characteristics. One explanation for our findings is that because of their later arrival in the United States, immigrants simply have fewer quarters of Social Security–covered earnings. Another possibility is that immigrants have the same number of quarters of covered earnings, but that those earnings are lower. Taking advantage of our restricted data, we reestimate our PIA regressions controlling for the number of covered quarters. The bottom panel of Table 3 shows that controlling for quarters of covered earnings turns the overall immigrant coefficient for all groups (after adjusting for all control variables) *positive* and significant, suggesting that the differences in PIAs between immigrants and natives discussed earlier can be entirely explained by quarters of covered earnings. After controlling for quarters of covered earnings, there are no longer significant differences in PIAs by number of years spent in the United States. This suggests that the larger gap in PIA of more recent immigrants observed in the top panel of Table 3 is due to a shorter history of Social Security–covered employment rather than differences in earnings between different immigrant cohorts.

In Table 4, we conduct the same exercise for actual self-reported annual Social Security benefits for respondents aged 65 or older. The immigrant patterns are very similar to those for expected Social Security benefits presented in Table 3. For married men, the raw immigrant/native differential without control variables as presented in Table 1 is $3,069. Adding all controls reduces the differential by half, to $1,495; however, the remaining gap is still statistically significant and large in magnitude. When we evaluate the immigrant effect by years spent in the United States, again, differences in average Social Security benefit levels emerge. The gap is negative and significant at the 25th percentile and not statistically different from zero at the median, but turns positive and statistically significant at the 75th percentile. The patterns found for married men are extremely similar to those for the other three subgroups, with negative and significant gaps at lower number of years in the United States, combined with positive and significant gaps at higher number of years in the country.

A major difference between Tables 3 and 4 is that actual, self-reported Social Security benefits for immigrants at the 75th percentile of years spent in the United States are about $800 to $1,400 higher than those for natives, although this is not the case for PIAs among the younger HRS respondents. We think there are two main potential explanations for that difference. Differences between cohorts—either in age or in year of immigration—is the simplest explanation. The subsample with actual, self-reported Social Security benefits consists of respondents aged 65 or older, while the PIA sample only includes respondents aged 51–61. An alternative explanation for the difference could be that immigrants may work longer than natives and retire later (there is some evidence of this for married men in Table 1), which would, conditional on covered earnings history, lead to higher Social Security benefits relative to their US-born counterparts. If this is the case, we would not see the difference for the younger cohort in projected PIAs because they have not yet retired.

Table 4.
Actual self-reported annual Social Security benefit for HRS respondents aged 65 or older, overall and by percentile number of years in the United States, 1998–2004 (in dollars)

Characteristic	Married		Unmarried	
	Men	Women	Men	Women
US native (reference category)	15,148	16,187	9,996	8,851
Model-predicted immigrant difference	-1,495 **	-1,085 **	-680	-661 **
	(346)	(381)	(460)	(232)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	-2,583 **	-1,917 **	-1,705 **	-1,346 **
	(352)	(416)	(486)	(282)
50th	-398	-119	85	27
	(379)	(452)	(497)	(300)
75th	821 **	805 *	1,410 **	566 *
	(385)	(473)	(505)	(303)

SOURCE: Authors' tabulations using the Health and Retirement Study.

NOTES: The model predicts differences in benefits between US natives and immigrants—controlling for age, race, education, census division, year of immigration, number of children, and health. Standard errors are in parentheses.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

Differences in Pension Coverage and Wealth

We next examine pension coverage among immigrant and native workers. Those regressions are run on the sample of HRS respondents younger than age 65 who were currently working. As shown in Table 1, married male immigrants have an 11 percentage point lower probability than their native-born counterparts of reporting that they have a pension. Again, those differentials are large in magnitude, given a mean probability of pension coverage among the married male native born of 60 percent. Results in the "Married" section of Table 5 show that controlling for age, health, education, census division, and race and ethnicity reduces the difference in the probability of pension coverage by 7 percentage points. Furthermore, additional years spent in the United States reduce the immigrant/native gap in pension coverage among married men. Immigrants in the United States for the 25th percentile number of years show a 10 percentage point lower probability of reporting pension coverage, but as time spent in the country increases to the median number of years, that gap in coverage decreases by half (5 percentage points). Immigrants in the United States for the 75th percentile number of years no longer exhibit a statistically significant gap in pension coverage. For married women, the significant raw gap in pension coverage shown in Table 1 is completely eliminated once controls are included. For unmarried men and women, the pattern of results is more similar to that of married men—a significant raw differential in pension coverage is reduced in magnitude once controls are included, but remains large and economically significant. However, these significant differentials are entirely driven by immigrants with the fewest number of years in the United States.

We now turn to measures of private wealth and examine immigrant/native differentials in total net worth, measured at the family level (Table 6). As shown in Table 1, large raw wealth differentials exist between immigrants and natives in all subgroups. For married men, married women, and unmarried men, the gaps are between 24 and 26 percent of the average level of net worth for natives in those subgroups. For unmarried women, the gap is smaller—roughly 17 percent. However, adding controls for age, education, self-rated health, number of children, census division, and race and ethnicity completely eliminates the estimated immigrant/native gap in net worth for all subgroups. In fact, for three of the four subgroups, the adjusted differentials are significant and *positive*. Among married men, immigrants (after controlling for those variables) have total net worth

Table 5.
Estimated pension coverage of HRS respondents younger than age 65 and working, overall and by percentile number of years in the United States, 1998–2004

Characteristic	Married		Unmarried	
	Men	Women	Men	Women
US native (reference category)	0.60	0.56	0.49	0.56
Model-predicted immigrant difference	-0.07 **	-0.03	-0.12 **	-0.08 **
	(0.02)	(0.03)	(0.06)	(0.03)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	-0.10 **	-0.03	-0.15 **	-0.12 **
	(0.03)	(0.03)	(0.07)	(0.04)
50th	-0.05 **	0.00	-0.00	-0.04
	(0.02)	(0.03)	(0.07)	(0.04)
75th	-0.03	0.00	-0.05	-0.02
	(0.03)	(0.03)	(0.07)	(0.04)

SOURCE: Authors' tabulations using the Health and Retirement Study.

NOTES: The linear probability model predicts differences in pension coverage between US-native and immigrant workers—controlling for age, race, education, census division, year of immigration, number of children, and health. Standard errors are in parentheses.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

Table 6.
Estimated net worth of HRS respondents aged 51 or older, overall and by percentile number of years in the United States, 1998–2004 (in dollars)

Characteristic	Married		Unmarried	
	Men	Women	Men	Women
US native (reference category)	376,875	378,255	210,396	168,339
Model-predicted immigrant difference	27,005 **	52,694 **	22,217	37,861 **
	(10,516)	(10,599)	(15,583)	(7,307)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	7,660	51,541 **	41,598 **	41,049 **
	(12,666)	(12,862)	(19,101)	(9,217)
50th	55,241 **	70,943 **	56,748 **	50,585 **
	(12,451)	(12,287)	(19,345)	(9,024)
75th	78,481 **	76,285 **	51,774 **	48,677 **
	(13,288)	(13,117)	(21,167)	(9,539)

SOURCE: Authors' tabulations using the Health and Retirement Study.

NOTES: The model predicts differences in net worth (measured at the family level) between US natives and immigrants—controlling for age, race, education, census division, year of immigration, number of children, and health. Standard errors are in parentheses.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

that is $27,000 *higher* than that of the native born. The positive differentials are even larger for women (both married and unmarried)—$52,694 and $37,861, respectively. Together, these estimates suggest that the raw mean immigrant/native difference in wealth shown in Table 1 is due to underlying differences in demographics, education, and family structure as well as race and ethnicity. Within groups defined by those characteristics, immigrants have more wealth than the native born. Regression results again show that differences between immigrants and natives vary substantially by length of time spent in the United States. Among married men, US immigrants at the 25th percentile number of years have net worth that is not statistically different from that of their US-native counterparts. At the 50th and 75th percentile number of years in the country, the wealth premium among immigrant married men is large and statistically significant. For the other three subgroups, these positive, sizable, and statistically significant differentials exist even at the 25th percentile number of years in the country.

We move on to examine the prevalence of home ownership among immigrants versus natives (Table 7). As shown in Table 1, immigrants in all subgroups are less likely to report home ownership than natives. All four groups have rates of home ownership between 13 and 16 percentage points lower than those of natives (although the rates of home ownership are substantially higher for married couples than for individuals, meaning that immigrant gaps in home ownership are smaller in percentage terms among the married). Table 7 presents home ownership gaps adjusted for our set of control variables, and controlling for all covariates reduces that differential by 7–8 percentage points. As in the previous regressions, we again see evidence of assimilation effects. For married men, the probability of home ownership for immigrants is 14 percentage points lower at the 25th percentile of years in the United States, compared with 6 percentage points lower at the median. There are no statistically significant differences in home ownership between immigrant and native married men at the 75th percentile. Results for other subgroups present a largely similar pattern. These findings (lower home ownership rates among immigrants, but significant assimilation) are consistent with findings in Borjas (2002).

Table 8 shows levels of home equity for respondents who are homeowners. As Table 1 shows, average home equity conditional on home ownership is higher among immigrants than natives for all subgroups. Estimates in Table 8 show that the differential grows in magnitude with both the inclusion

Table 7.
Estimated prevalence of home ownership among HRS respondents aged 51 or older, overall and by percentile number of years in the United States, 1998–2004

	Married		Unmarried	
Characteristic	Men	Women	Men	Women
US native (reference category)	0.89	0.88	0.57	0.60
Model-predicted immigrant difference	-0.08 **	-0.07 **	-0.08 **	-0.07 **
	(0.01)	(0.01)	(0.02)	(0.01)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	-0.14 **	-0.11 **	-0.09 **	-0.13 **
	(0.01)	(0.01)	(0.03)	(0.02)
50th	-0.06 **	-0.03 **	-0.04	-0.05 **
	(0.01)	(0.01)	(0.03)	(0.02)
75th	0.00	0.01	-0.01	0.00
	(0.01)	(0.01)	(0.03)	(0.02)

SOURCE: Authors' tabulations using the Health and Retirement Study.

NOTES: The linear probability model predicts differences in home ownership between US natives and immigrants—controlling for age, race, education, census division, year of immigration, number of children, and health. Standard errors are in parentheses.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

of covariates as well as number of years in the United States, particularly for married respondents. These results are consistent with two additional studies on immigrants and home ownership. Drew (2002), using data from the American Housing Survey, found that the median value of first-time home purchases among the foreign born was 50 percent higher than that of the native born, and as a result, immigrants were making larger down payments. Chatterjee and Zahirovic-Herbert (2011) find evidence of higher home equity among immigrants, conditional on home ownership, in the National Longitudinal Survey of Youth (NLSY). In both studies, much of this effect was due to the spatial clustering of immigrants in high housing-cost areas like California and New York. Our results include controls for census division of residence, so they account for this clustering. As an additional robustness test, we reestimate those regressions excluding the Mid-Atlantic census region (which includes New York) and the Pacific census region (which excludes California). Our coefficients are slightly smaller in magnitude, but the main results all still hold. One possible explanation for why immigrant homeowners may have greater home equity is that they may be more risk averse, investing a greater share of their wealth in their homes relative to assets like stocks, which they might find riskier. Alternatively, Chatterjee and Zahirovic-Herbert (2011) suggest that immigrants may have credit constraints and lack information about the formal banking sector, and as a result make higher down payments when purchasing a home.

Given the higher levels of home equity for immigrants relative to natives (conditional on home ownership), it is also possible that the higher levels of net worth adjusted for the control variables, as shown in Table 6, might be entirely driven by housing equity. This could be exacerbated by the fact that our sample period includes a number of years of rising housing prices in most parts of the country. In Table 9, we report estimates using only nonhousing wealth (total net worth minus home equity). For all subgroups, throughout the distribution of number of years in the United States, immigrants have higher levels of nonhousing wealth than do natives, and the differential averages out to roughly half that of their total net worth. This suggests that the immigrant advantage in private wealth (conditional on a number of socioeconomic factors as well as race and ethnicity) is driven by differences in both housing and nonhousing wealth.

Table 8.
Estimated levels of home equity among HRS respondents aged 51 or older, overall and by percentile number of years in the United States, 1998–2004 (in dollars)

	Married		Unmarried	
Characteristic	Men	Women	Men	Women
US native (reference category)	137,662	138,921	111,696	105,048
Model-predicted immigrant difference	16,683 **	23,340 **	38,338 **	34,062 **
	(3,158)	(3,168)	(7,403)	(3,603)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	18,555 **	19,462 **	54,466 **	38,129 **
	(3,763)	(3,844)	(8,990)	(4,419)
50th	27,369 **	26,366 **	41,705 **	36,710 **
	(3,755)	(3,681)	(9,030)	(4,366)
75th	28,164 **	29,801 **	22,702 **	34,662 **
	(4,066)	(3,986)	(10,840)	(4,688)

SOURCE: Authors' tabulations using the Health and Retirement Study.

NOTES: The model predicts differences in home equity between US-native and immigrant homeowners—controlling for age, race, education, census division, year of immigration, number of children, and health. Standard errors are in parentheses.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

Table 9.
Estimated nonhousing net wealth of HRS respondents aged 51 or older, overall and by percentile number of years in the United States, 1998–2004 (in dollars)

Characteristic	Married		Unmarried	
	Men	Women	Men	Women
US native (reference category)	241,764	242,730	135,495	96,836
Model-predicted immigrant difference	18,168 **	37,354 **	4,780	18,980 **
	(8,822)	(8,876)	(12,803)	(14,625)
Model-predicted immigrant difference at given percentile number of years in the United States:				
25th	6,980	39,435 **	14,407	25,815 **
	(10,603)	(10,780)	(15,718)	(7,353)
50th	35,133 **	50,707 **	30,878 *	26,163 **
	(30,441)	(10,301)	(15,872)	(7,206)
75th	49,039 **	53,108 **	30,749 *	19,672 *
	(11,516)	(10,996)	(17,417)	(7,618)

SOURCE: Authors' tabulations using the Health and Retirement Study.

NOTES: The model predicts differences in nonhousing net worth between US natives and immigrants—controlling for age, race, education, census division, year of immigration, number of children, and health. Standard errors are in parentheses.

* denotes significant differences between immigrants and natives at the 10 percent level.

** denotes significant differences between immigrants and natives at the 5 percent level.

*** denotes significant differences between immigrants and natives at the 1 percent level.

Discussion and Conclusion

An extensive literature in labor economics has focused on wage differentials between immigrants and natives, but much less attention has been paid to differences in retirement resources and retirement security. In this article, we examine differences in the retirement resources of immigrants versus those of the native born. Our results suggest that preretirement immigrants have lower expected Social Security benefits than natives, and that retired immigrants have lower actual Social Security benefits. These lower benefits reflect fewer years of Social Security–covered employment rather than lower average contributions in those years. Our findings present an alternative focus on immigrant differences in Social Security to those of Gustman and Steinmeier (2000) and Favreault and Nichols (2011) who highlight the higher relative replacement rates among immigrants.

In addition, we find that working immigrants are substantially less likely to have pension coverage and that immigrants on average have lower private wealth than do natives. However, after controlling for various demographic differences—education, age, self-reported health, census division, and race and ethnicity—immigrants have substantially *higher* net worth than their similarly situated native-born counterparts.

A logical question is whether the higher private wealth exhibited by immigrants is sufficient to offset the lower levels of Social Security benefits in terms of aggregate retirement security. To assess this, we perform back-of-the-envelope postestimation calculations for our subsample of married men to compare the net present value of future Social Security benefits with net worth for both immigrants and natives. Given the differences in immigrant effects based on the number of years immigrants have lived in the United States, we make that comparison at the 25th, 50th, and 75th percentiles of years spent in the country.

Among the sample of married male respondents aged 51–61, our analysis using the restricted Social Security earnings data suggests that the net present value of the lower Social Security benefits of immigrants in the United States for the median number of years (26) is about $25,000. The estimated immigrant premium in net worth evaluated at the same median number of years is about $14,000—suggesting that these immigrants have amassed private wealth sufficient to offset just over half of their relatively lower Social Security benefits.[16] Married male immigrants in the United States for fewer years have not accumulated enough private wealth relative to natives to offset their lower Social Security benefits, although those in the

United States for the 75th percentile number of years have amassed private wealth substantially greater than that of natives.

Among respondents aged 65 or older, we perform a similar comparison by looking at reported Social Security benefits. For that group, at the 25th percentile of years (30) in the United States, the greater relative private wealth amassed by immigrants (roughly $30,000) offsets 75 percent of the net present value of their lower Social Security benefits. Immigrants in the country for more than 30 years have net worth that sufficiently offsets their lower Social Security benefits relative to natives. In addition, immigrants in the United States for more than 40 years have both higher Social Security benefits and net worth relative to natives.[17]

These results are subject to a number of caveats. First, in interpreting the effects of number of years in the United States, it is impossible for us to disentangle true assimilation effects from cohort differences in either immigrant quality or transferability of skills. We are unable to identify differences that are due to country of origin, which the existing literature suggests is quantitatively important. In addition, any patterns found in the HRS cohort may not be representative of differences in retirement security among future generations approaching retirement.

Furthermore, the mobility of immigrants brings additional complications to an analysis of retirement resources not found with the native born. Our data provide no information on earnings or retirement benefit eligibility for immigrants in their countries of origin, so we could be systematically understating the resources available.

In addition, our analysis implicitly assumes no permanent return to the country of origin among US immigrants, which is clearly unrealistic. Estimates of emigration among immigrants range from 15 to 30 percent (Borjas and Bratsberg 1996; Mayr and Peri 2008), and those rates vary by age at immigration, number of years spent in the United States, conditions in the country of origin, and eligibility for Social Security benefits. Remigration rates would be expected to decrease with number of years in the United States and increase with age at immigration (Duleep 1994). That pattern of return migration may mitigate some of the potential hardship among recent US immigrants. We find that those immigrants have lower levels of predicted and actual Social Security benefits, but returning to a home country with additional resources and lower cost-of-living expenses might make those benefits go farther.

Our interpretations of the results also do not account for totalization agreements, which are bilateral agreements between the United States and other countries that allow individuals' eligibility for Social Security benefits to be based on a combination of their work under the United States Social Security system and their work under the system of their home country (Barrick and Kestenbaum 2013). Currently, 24 countries have those agreements with the United States. Because most of the countries with totalization agreements are industrialized countries in Europe and Asia (including Japan and Korea), we would expect that the immigrants affected by those agreements have higher levels of human capital and income, relative to immigrants from countries without such agreements. Therefore, we might be underestimating the retirement resources at the upper end of the immigrant wealth distribution relative to the lower end.

That being said, our results suggest that the truth about immigrants' retirement security is, at a minimum, much more nuanced than the conventional wisdom regarding their preparation for retirement. Our results are consistent with a growing literature on immigrant effects on wages that highlights differences across immigrant groups. These findings suggest that immigrants might be more prepared for retirement than previously indicated in the literature, compensating for lower Social Security benefits with higher private savings. However, as with the distribution of retirement security among the native born, a sizable tail of the distribution is less well-prepared for retirement. That tail is primarily made up of recent immigrants, who, given the age restrictions in the HRS, must be those respondents who migrated to the United States at older ages. Further research is necessary to understand fully this segment of the population and to inform appropriate policies.

Notes

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[1] See Borjas (1999), Blau and others (2003), and Duleep and Dowhan (2008) for a review of this literature.

[2] However, these calculations ignore the annuity value of Social Security in protecting individuals from outliving their savings.

[3] An estimated 11 million undocumented immigrants reside in the United States (Passel and Cohn 2010), and many of them participate in the labor force and contribute to Social Security. The Social Security Administration

estimated that taxes paid into Social Security by undocumented immigrants exceeded benefits paid out by $12 billion in 2007 (Goss and others 2013).

[4] Amuedo-Dorantes and Pozo (2002) also note intergenerational transfers with respect to precautionary savings.

[5] The PIA is calculated as the sum of three separate percentages of portions of the worker's average indexed monthly earnings. See http://www.socialsecurity.gov/oact/cola/piaformula.html for the calculation formula and other details.

[6] The HRS sample contains individuals at ages where the earnings profile is often thought to be flat or declining (see, for example, Lillard and Willis (1978), Hanoch and Honig (1985), Murphy and Welch (1990), and Johnson and Neumark (1996)). However, recent evidence suggests that the earnings profile continues to increase at older ages as long as individuals continue to work full time (Casanova 2012). Given the mixed evidence, we assume a flat earnings profile when performing these imputations. The Social Security Administration follows a similar approach when projecting individual retirement benefits in its Social Security statements, but with a flat nominal earnings profile.

[7] Calculations of average retirement age must deal with censoring, as some fraction of the sample will not yet have retired. However, regardless of sample used, immigrants appear to be retiring 1 year later on average than natives. Among respondents aged 75 or older in the 2004 HRS, the average retirement age was 64.18 for immigrants and 63.24 for natives.

[8] Working and reporting being retired are not mutually exclusive or exhaustive categories.

[9] Self-rated health is reported on a scale of 1 to 5, where 1 represents excellent health and 5 represents poor health.

[10] We do not include a control for income because it is clearly endogenously determined. However, adding controls for log income does not qualitatively change our results.

[11] Country of origin is available in the HRS as restricted data, but it is prohibited to link country of origin with Social Security's restricted earnings history data.

[12] The PIA is only estimated once for each respondent because it is based on one's earnings history.

[13] We have also estimated regressions where we control for number of years in the United States in a linear specification and where we allow for a nonlinear spline specification. Results are qualitatively similar and are available from the authors upon request.

[14] However, repeated cross-sectional analyses are also biased by differential return migration (Duleep and Dowhan 2002; Lubotsky 2007). Our analysis is not subject to that bias.

[15] The full set of estimates is available from the authors upon request.

[16] To simplify our calculations, we assume all couples claim benefits at the normal retirement age and that both partners live for 18 years after that.

[17] However, these calculations do not take into account the way in which the annuity provided by Social Security insures against risks associated with longevity, including outliving one's savings. Mitchell and others (1999) note that the standard life-cycle model implies that consumers should be willing to give up a sizable share of their total net worth (30 to 38 percent) to purchase an actuarially fair annuity at age 65. That could be particularly important for immigrants because they experience lower age-specific mortality than the native born (Sevak and Schmidt 2008). A full analysis would take into account differential longevity risks and a measure of annuity-equivalent wealth (see, for example, Gentry and Rothschild (2010)). This, however, is beyond the scope of the current article.

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