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Vermont's Progressive Employment Program: A Preliminary Impact Analysis

Progressive employment is an intervention model piloted at some state vocational rehabilitation agencies across the country. Developed in part to address the limitations of rapid-placement employment models, progressive employment takes a more flexible and gradual approach to employment.

Progressive employment (PE) is a relatively new intervention model for people who face major barriers to employment. It has been adopted by a small group of state vocational rehabilitation (VR) agencies but has not yet been fully tested. As administrators of other state VR agencies consider the PE model, they could benefit from evidence about PE's effects on employment, earnings, and the receipt of disability benefits. If PE can be established as an evidence-based practice, state VR agencies would likely adopt it more quickly.

We used administrative data from Vermont to examine PE's impact on VR customers in the state. Because a randomized controlled trial of PE in Vermont is currently not feasible, we used a quasi-experimental approach to avoid the bias that might otherwise emerge when comparing the outcomes of people selected to receive PE services versus those who were not. We found that PE increases the likelihood of exiting VR with a job by over 20 percentage points, and it may also increase earnings after VR exit. A randomized controlled trial would generate more rigorous evidence about the impacts of PE.

WHAT IS PE?

PE is an intervention model piloted at some state VR agencies across the country. Developed in part to address the limitations of rapid-placement employment models, PE takes a more flexible and gradual approach to employment and is usually targeted at VR customers who, for various reasons, are at high risk of exiting VR without employment. The PE approach also strongly emphasizes serving employers—as well as customers—and is believed by its developers to create stronger, more productive relationships between VR agencies and local employers (Bradshaw et al. 2012).

Vermont's general agency—the Division of Vocational Rehabilitation (DVR)—created PE in 2009 with funding from the American

Recovery and Reinvestment Act, and has refined the model over time (Moore et al. forthcoming in 2018). Four state VR agencies use the model—DVR, the general agencies of Maine and Nebraska, and Oregon's blind agency—and two other agencies are strongly considering PE. DVR started referring some customers for PE services in May 2009, and the expansion of these services in Vermont varied over time and across district offices in the state.

Although the PE model has been adapted to the needs of each participating agency, it has several core components that are part of each agency's model. These components include work-based learning experiences with employers, payments to VR customers to offset training costs, a "dual-customer" design (both the person receiving VR and the employer are considered to

For a more comprehensive description of PE, see Moore et al. (forthcoming in 2018).

We used a variant of difference-in-differences that addressed our inability to fully identify the target population in the data and used the variability of PE receipt across applicant cohorts—holding baseline characteristics constant—to identify the impacts of PE receipt.

For more details on our data and methods, please see the appendix.

be customers), and a team approach that requires regular coordination between VR counselors and stakeholders. In general, the VR agencies target PE services to customers thought to be at high risk of exiting VR without a job, often after having limited success with other VR services, but the nature of such targeting is not well-defined and or uniform. Once customers start PE, the VR agency attempts to rapidly engage them in various workplace activities. The advantages for employers include their ability to screen potential workers before placement and the VR agency assuming the insurance and liability responsibilities for the workers.

DATA AND METHODS

In our analysis, we used administrative data from DVR's records matched to two other sources:

1. Earnings data extracted from state Unemployment Insurance (UI) wage records
2. Supplemental Security Income (SSI) payment records and Social Security Disability Insurance (SSDI) records on benefit receipt from Vermont's public-benefits data system

Our sample included the bulk of all DVR customers who met two criteria: they had applied for services between May 1, 2009, (when DVR first started offering PE to some customers) and December 31, 2014, and they had a signed Individualized Plan for Employment. The sample excluded the relatively small number of customers who had not exited the program by June 30, 2017—when DVR changed its administrative data system—plus the few other customers for whom the data were incomplete. In total, the sample had 15,815 DVR customers, including 2,356 customers (14.9 percent) who had a funding set-aside request for PE services—which we used as the indicator of PE services in our analysis.

Our key outcome variable was the customers' employment status at VR exit after they received services (formally, case closure with employment following receipt of services). We also analyzed customers' receipt of SSI payments and SSDI benefits at exit, but for various data and methodological reasons described in the appendix, we found the results for these outcomes to be less compelling than those for the other outcomes. In addition, we gathered extensive information from the DVR record about customers' sociodemographic characteristics, past work experience

at the time of VR application, earnings in the two quarters before the application quarter, and the county of the DVR office; we used these data to specify a rich set of control variables (hereafter, "baseline characteristics").

The non-experimental method we used to estimate impacts is a variant of the difference-in-differences (DiD) approach. This approach focuses on changes in outcomes for a target population from before the intervention is introduced to after the intervention is introduced (that is, only those in the target population in the later period received the intervention), after netting out contemporaneous changes in outcomes for a comparison group—subjects not in the target population. The purpose of netting out these changes is to account for any extraneous factors that might have affected the outcomes of both the target and comparison groups. We used a variant of difference-in-differences that addressed our inability to fully identify the target population in the data and used the variability of PE receipt across applicant cohorts—holding baseline characteristics constant—to identify the impacts of PE receipt.

FINDINGS

Descriptive

Baseline characteristics. We found many statistically significant differences in the baseline characteristics of customers receiving PE versus all other customers in the sample (Table 1). Many of these differences indicate that the PE customers, as a group, have much larger barriers to work than other customers—which is expected, given PE's goal of targeting people with such barriers. Specifically, they tend to be younger, have less education and employment experience, have been less likely to work at application or to have had earnings in the previous two quarters, be more likely to receive SSI at application or to be a student, and be more likely to have a developmental disability. There are not statistically significant differences in sex, race or ethnicity across the two groups.

In general, PE recipients tend to be younger than nonrecipients when they apply to DVR; about 35 percent are age 24 or younger (compared with 24 percent of the PE nonrecipients). In terms of the VR offices that customers used, we found a statistically significant difference in the overall

distributions of PE recipients and nonrecipients across offices and years. But somewhat surprisingly, this difference is less than 5 percentage points at all offices but one (Barre). Based on conversations with DVR staff, we had expected greater variation in PE receipt across offices.

PE recipients and nonrecipients also differ in their receipt of SSI benefits at application—but not in their receipt of SSDI benefits. For SSI, 39 percent of eventual PE recipients are SSI recipients, compared with 30 percent of eventual PE nonrecipients. In addition, the two groups differ in the distribution of primary disability types, with PE recipients being 12 percentage points more likely to have a developmental disability.

At application to DVR, eventual PE recipients are 18 percentage points more likely to be unemployed students and 8 percentage points more likely to be unemployed nonstudents. Recipients are also 13 percentage point less likely to have a high school diploma and are less likely to have post-secondary education or degrees.

In the final two full quarters—that is, six months—before application, eventual PE recipients are less likely than nonrecipients to be working or to have sufficient earnings for two quarters of coverage from the Social Security Administration (SSA)¹; only 33 percent of eventual PE recipients have any earnings, versus 47 percent of nonrecipients, and only 14 percent had sufficient earnings for two SSA quarters of coverage, versus 28 percent of nonrecipients.

Differences in sex, race, and ethnicity between the two groups are either statistically insignificant or not large enough to be noteworthy.

Outcomes. Despite presumably greater barriers to employment, PE recipients achieve outcomes that in some respects are on par with those of other customers (Table 2). In fact, PE recipients are 3 percentage points more likely to exit VR with employment after services, and they are about as likely as nonrecipients to have any earnings two and four quarters after VR exit. But PE recipients' average earnings at two and four quarters after VR exit are lower than those of other customers, and by the fourth quarter, they are also less likely to earn at the quarters of coverage level.

For reasons discussed in the appendix, measurement issues diminish our confidence in the accuracy of the findings for SSDI and SSI

receipt. Based on how these outcomes are measured both groups appear to have small increases in SSDI receipt from application to exit, but we saw no change in the difference between the groups. SSI receipt decreases somewhat from application to VR exit for both groups—but by 2 percentage points less for the PE group than for all others.

Impact

For each annual applicant cohort, the impact estimates for all outcome variables appear in Table 3. The weighted average estimates of the impacts, across all cohorts, appear in Table 4.

Employment status at VR exit. Across all applicant cohorts, receipt of PE services increases the probability of exiting VR with employment by an estimated 21.3 percentage points (Figure 1; Tables 3 and 4). That implies an exit with employment rate the 60.4 percent for all PE cases in the sample (Table 2) is more than 50 percent larger than it would have been in the absence of PE. The lower bound of the 95 percent confidence interval for the all-cohort estimate is 20.8 percentage points. The size of the point estimate across application cohorts ranges from 16.9 percentage points (2010) to 28.5 percentage points (2009).

Earnings outcomes. There is evidence of positive impacts on employment and earnings two and four quarters after VR exit. For all cohorts combined, we saw a 6.9 percentage-point increase in the share of PE recipients who had any earnings two quarters after exit, along with a 6.3 percentage-point increase in the share of PE recipients who earned more than \$2,600. Similarly, at four full calendar quarters after exit, we saw increases of 9.6 percentage points in any earnings and 5.3 percentage points in earning more than \$5,200.

SSDI and SSI. As noted earlier, problems with measuring benefit-receipt outcomes limited our ability to interpret our findings in this area. Across all cohorts combined, PE receipt increases the receipt of SSDI by an estimated 2.4 percentage points, based on measured SSDI receipt at VR exit. However, this significant result across all cohorts is driven by a significant result for the 2013 cohort only; for all other cohorts, the estimate is not significant at the 5 percent level. In addition, across all applicant cohorts, receiving PE services increases SSI

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¹ In 2017, \$1,300 in earnings was needed to earn one quarter of coverage for Social Security benefits. Up to four SSA quarters of coverage can be earned in a calendar year.

receipt at VR exit on the order of 10 percentage points, as measured (11.6 percentage points by one measure and 9.7 percentage points by another). We saw substantial variation in the point estimates across applicant cohorts, but almost all estimates are statistically significant.

DISCUSSION

Our results suggest that PE holds promise for improving the employment and earnings outcomes of VR customers, both at VR exit and during the first several calendar quarters thereafter. Compared with PE nonrecipients, recipients are more likely to leave the VR program with a job and may have better earnings outcomes during the first four full calendar quarters after VR exit.

The increase in SSDI and SSI receipt may be seen as a drawback, given the goal of reducing reliance on disability benefits, but it's important to note that the SSDI result appears to be driven by an outlier estimate for one applicant cohort. Furthermore, even if PE increases SSI receipt at VR exit, there are plausible reasons to think that there could eventually be reductions in SSI payments after VR exit. First, even if the number of SSI payments remains steady, it may be that the payment amounts are much lower due to program rules that reduce SSI payments by 50 cents for every dollar of earnings above a (low) earnings disregard. Second, the apparently positive impacts of PE on earnings after VR exit may eventually yield reductions in SSI payments that were not evident at VR exit.

This study's findings—and its limitations—suggest that a rigorous prospective test of PE would help establish whether PE is evidence based. The data and methodological limitations of this study mean that we cannot rule out the possibility that the impact estimates are biased. We used statistical methods to address potential bias due to limitations in the measurement of PE receipt, baseline differences between PE and non-PE customers, the effects of the business cycle, and changes in factors external to DVR over the sample period; however, these methods are far from perfect. A randomized controlled trial of PE could overcome these challenges, producing rigorous, unbiased findings on program impacts.

A state VR agency that wants to adopt the PE approach would be well-advised to conduct a randomized controlled trial or other rigorous test of PE before making a commitment to provide this intervention. Although PE appears to be promising, its impacts on key outcomes are uncertain, and a great deal of effort could be wasted by introducing PE without conducting more rigorous tests. To our knowledge, there are no legal or ethical barriers to conducting such a test, and several VR agencies have recently conducted (or are now conducting) randomized controlled trials with support from the Rehabilitation Services Administration.

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Table 1. Baseline descriptive statistics

Variable	Received PE		Did not receive PE		p-value
	Mean	Standard deviation	Mean	Standard deviation	
N	2,356		13,459		
Sex (female=1)	0.4482	(0.4974)	0.4575	(0.4982)	0.4186
Age					0.0000***
14 to 18	0.0361	(0.1865)	0.0204	(0.1412)	
19 to 24	0.3154	(0.4648)	0.2137	(0.4099)	
25 to 34	0.2284	(0.4199)	0.2161	(0.4116)	
35 to 44	0.1337	(0.3404)	0.1626	(0.3691)	
45 to 54	0.1766	(0.3814)	0.1980	(0.3985)	
55 to 64	0.0959	(0.2946)	0.1508	(0.3578)	
65 and older	0.0140	(0.1175)	0.0384	(0.1922)	
Race					0.0326**
Asian	0.0081	(0.0895)	0.0059	(0.0769)	
Black	0.0123	(0.1103)	0.0205	(0.1417)	
Multiracial or other	0.0318	(0.1756)	0.0365	(0.1875)	
Native American	0.0059	(0.0769)	0.0074	(0.0855)	
White	0.9419	(0.2341)	0.9297	(0.2556)	
Hispanic	0.0098	(0.0983)	0.0101	(0.1000)	0.9667
Field Office					0.0000***
Barre	0.0412	(0.1987)	0.1116	(0.3149)	
Bennington	0.0700	(0.2553)	0.0727	(0.2597)	
Brattleboro	0.1019	(0.3025)	0.0955	(0.2939)	
Burlington	0.1880	(0.3908)	0.1641	(0.3703)	
Middlebury	0.0688	(0.2531)	0.0494	(0.2167)	
Morrisville	0.0802	(0.2717)	0.0386	(0.1927)	
Newport	0.1133	(0.3171)	0.0701	(0.2553)	
Rutland	0.1299	(0.3362)	0.1033	(0.3043)	
Springfield	0.0293	(0.1686)	0.0771	(0.2668)	
St. Albans	0.0666	(0.2494)	0.0820	(0.2743)	
St. Johnsbury	0.0722	(0.2588)	0.0556	(0.2291)	
White River Junction	0.0386	(0.1927)	0.0801	(0.2715)	
SSDI at Application	0.2050	(0.4038)	0.1918	(0.3938)	0.1429
SSI at Application	0.3925	(0.4885)	0.2957	(0.4564)	0.0000***
Primary Disability Types					0.0000***
Developmental	0.2445	(0.4299)	0.1277	(0.3338)	
Learning	0.1702	(0.3759)	0.1545	(0.3615)	
Medical systemic	0.0705	(0.2560)	0.1251	(0.3309)	
Mental health	0.3595	(0.4800)	0.3726	(0.4835)	
Neurological	0.0216	(0.1456)	0.0186	(0.1350)	
Substance abuse	0.0446	(0.2064)	0.0731	(0.2603)	

continued

Variable	Received PE		Did not receive PE		p-value
	Mean	Standard deviation	Mean	Standard deviation	
Trauma	0.0645	(0.2457)	0.0852	(0.2792)	
Unknown	0.0246	(0.1550)	0.0431	(0.2031)	
Employment Status at Application					0.0000***
Employment without supports	0.0772	(0.2670)	0.2086	(0.4063)	
Employed, other	0.0293	(0.1686)	0.0412	(0.1987)	
Student	0.1783	(0.3828)	0.0994	(0.2992)	
Not employed	0.7110	(0.4534)	0.6285	(0.4832)	
Education Level at Application					0.0000***
No diploma	0.3183	(0.4659)	0.2352	(0.4242)	
High school diploma	0.4851	(0.4999)	0.4746	(0.4994)	
Some college	0.1392	(0.3462)	0.1881	(0.3908)	
Bachelor's degree	0.0488	(0.2155)	0.0709	(0.2566)	
Graduate degree	0.0068	(0.0821)	0.0284	(0.1661)	
Vocational degree	0.0017	(0.0412)	0.0029	(0.0538)	
Total UI Wages Two Quarters Before Application Quarter	1139.7614	(2750.0704)	2713.3251	(5500.6459)	0.0000***
Any UI Wages Two Quarters Before Application Quarter	0.3318	(0.4709)	0.4745	(0.4994)	0.0000***
Coverage UI Wages Two Quarters Before Application Quarter	0.1418	(0.3489)	0.2770	(0.4475)	0.0000***

Notes: We used chi-squared tests to evaluate differences between categorical variables. We used t-tests to evaluate differences between binary and continuous variables.

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

*** Statistically significant at the 1% level.

SD = Standard deviation.

Table 2. Outcome descriptive statistics

Variable	N	Received PE		Did not receive PE		p-value
		Mean	Standard deviation	Mean	Standard deviation	
Employed at VR exit	15,815	0.6040	(0.4892)	0.5669	(0.4955)	0.0000***
Receipt of SSDI benefits at VR exit	15,811	0.2258	(0.4182)	0.2069	(0.4051)	0.0418**
Receipt of SSI payments at VR exit	15,811	0.2716	(0.4449)	0.1403	(0.3473)	0.0000***
Total earnings two quarters after VR-exit quarter	15,714	\$2,666.90	(4018.38)	\$3,858.75	(6032.74)	0.0000***
Any earnings two quarters after VR-exit quarter	15,714	0.5702	(0.4952)	0.5680	(0.4954)	0.8610
Coverage earnings two quarters after VR-exit quarter	15,714	\$0.33	(\$0.47)	\$0.40	(\$0.49)	0.0000***
Total earnings four quarters after VR-exit quarter	15,712	\$5,370.85	(8061.60)	\$7,734.68	(11923.26)	0.0000***
Any earnings four quarters after VR-exit quarter	15,712	0.6135	(0.4870)	0.6144	(0.4867)	0.9518
Coverage earnings after VR-exit quarter	15,712	0.3379	(0.4731)	0.4019	(0.4903)	0.0000***

Notes: We used chi-squared tests to evaluate differences between categorical variables. We used t-tests to evaluate differences between binary and continuous variables.

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

*** Statistically significant at the 1% level.

Table 3. Impact estimates for annual cohorts

Outcome	Impact on mean outcome				Confidence interval contains zero
	Year	Point estimate	95% confidence interval		
			Lower bound	Upper bound	
Employed at VR exit	2009	0.2855	0.1575	0.4134	No
	2010	0.1687	0.0693	0.2681	No
	2011	0.2037	0.1052	0.3023	No
	2012	0.1691	0.0608	0.2774	No
	2013	0.2782	0.1637	0.3928	No
SSDI benefit receipt at VR exit	2009	0.0336	-0.0369	0.1041	Yes
	2010	0.0038	-0.0455	0.0531	Yes
	2011	0.0125	-0.0337	0.0586	Yes
	2012	0.0078	-0.0598	0.0442	Yes
	2013	0.0932	0.0408	0.1456	No
SSI payment receipt at VR exit	2009	0.2416	0.1727	0.3105	No
	2010	0.0542	0.0090	0.0995	No
	2011	0.1496	0.1028	0.1965	No
	2012	0.0859	0.0385	0.1333	No
	2013	0.0959	0.0459	0.1459	No

continued

Outcome	Impact on mean outcome				Confidence interval contains zero
	Year	Point estimate	95% confidence interval		
			Lower bound	Upper bound	
Any earnings two quarters after VR exit	2009	-0.1040	-0.2256	0.0176	Yes
	2010	0.0982	0.0019	0.1946	No
	2011	0.0606	-0.0340	0.1551	Yes
	2012	0.0289	-0.0769	0.1347	Yes
	2013	0.1368	0.0246	0.2489	No
Coverage earnings two quarters after VR exit	2009	-0.0101	-0.1207	0.1005	Yes
	2010	0.0753	-0.0168	0.1673	Yes
	2011	0.0478	-0.0437	0.1393	Yes
	2012	0.0530	-0.0487	0.1547	Yes
	2013	0.0707	-0.0401	0.1816	Yes
Any earnings four quarters after VR exit	2009	-0.0785	-0.1974	0.0404	Yes
	2010	0.1179	0.0239	0.2119	No
	2011	0.0817	-0.0110	0.1744	Yes
	2012	0.0992	-0.0038	0.2021	Yes
	2013	0.1634	0.0508	0.2759	No
Coverage earnings four quarters after VR exit	2009	-0.0336	-0.1440	0.0768	Yes
	2010	0.1003	0.0097	0.1909	No
	2011	0.0109	-0.0814	0.1031	Yes
	2012	0.0337	-0.0694	0.1368	Yes
	2013	0.0188	-0.0955	0.1331	Yes

Table 4. Weighted impact estimates for all consumer cohorts from 2009 through 2013

Outcome	Impact on mean outcome			Confidence interval contains zero
	Point estimate	95% confidence interval		
		Lower bound	Upper bound	
Employed at VR exit	0.2130	0.2080	0.2179	No
Receipt of SSDI benefits at VR exit	0.0243	0.0225	0.0260	No
Receipt of SSI payments at VR exit	0.1162	0.1144	0.1180	No
Any earnings two quarters after VR exit	0.0564	0.0519	0.0609	No
Coverage earnings two quarters after VR exit	0.0522	0.0481	0.0563	No
Any earnings four quarters after VR exit	0.0892	0.0853	0.0932	No
Coverage earnings four quarters after VR exit	0.0314	0.0274	0.0354	No

APPENDIX

This section provides additional details about our data and analytic methods.

Data

The number of PE referrals increased across the annual applicant cohorts from 2009 until 2011 (252 in 2009, 450 in 2010, 518 in 2011); it then declined modestly through 2014 (435 in 2012, 391 in 2013, 310 in 2014). This pattern is likely the result of a variety of factors: the inclusion of only eight months in 2009; substantial growth followed by a decline in applications to DVR after the Great Recession; changes in DVR's capacity to deliver PE services; changes in the mix of DVR customers; and changes in the number of job openings in Vermont. Our analysis also excluded sample cases that were open after June 30, 2017, which may have contributed to the decline in PE cases in 2012 through 2014 relative to previous years.

The DVR data used for our analysis consists of demographic information; status indicators at application for educational attainment and enrollment, employment, SSI payment receipt and SSDI benefit receipt; DVR case data such as counselor identifier, application date, and VR exit date; information at VR exit such as employment status, SSI payment status, and receipt of SSDI benefits; and PE receipt information. The PE status data indicate when a set-aside request was made by a DVR counselor to use funding devoted exclusively to PE services. One important limitation of this variable is that it does not identify all customers who received PE services; we describe how our methods address that limitation below. The DVR administrative data are linked to state UI earnings records, providing quarterly measures of customer earnings both before and after applying to DVR. The information on receipt of Social Security disability benefits from Vermont's public-benefits data system is also linked to the DVR administrative data.

Unfortunately, the benefit receipt data from Vermont's data system are unreliable for SSDI benefits and incomplete for SSI payments. One reason is that they rely on reports from VR customers. If customers do not provide the necessary information at VR exit, the counselor may fill in the blanks with the most recent information available—possibly from the initial application for services. Another reason is that SSA often adjusts benefits retroactively, especially when earnings change, because of slow reporting by beneficiaries or extensive processing delays. Hence, even if a VR customer accurately reports receipt or nonreceipt of a check during the month of VR exit, that report may misrepresent the customer's status. Although we report estimates for disability benefit receipt measures for the sake of completeness, we do not have confidence in their interpretation.

Methods

There are several important issues to consider for any analysis of PE in Vermont. For example, DVR administrative data cannot identify all PE recipients—only recipients for whom there was a PE set-aside funding request. Though most customers who

received PE services received them through such a request, this is not true for all PE recipients. Hence, any analysis that relies solely on the PE status measures from the DVR administrative data will incorrectly assign some PE recipients to the no receipt group.

Variation over time also complicates any PE impact analysis. PE was deployed at the end of the Great Recession, and its use increased substantively during the subsequent economic recovery. Throughout this dynamic period, the changing landscape of the labor market likely affected the outcomes of all DVR customers, including PE recipients. DVR-specific factors may have also affected PE receipt over time. For example, PE referral patterns at DVR likely changed as counselors observed which types of PE referrals were most likely to result in successful employment outcomes.

We used a quasi-experimental approach to identify the causal effect of PE on customer employment, earnings, and receipt of SSI and SSDI benefits. After controlling for factors that may be correlated with PE referrals—including customer characteristics, DVR factors such as field office and counselor, and year—we estimated PE's annual impacts for 2009 through 2013. We examined three sets of outcomes: employment at VR exit, four earnings measures (assessed in 2017 dollars), and receipt of SSI and SSDI benefits at VR exit. For earnings, we examined two outcome types: (1) whether a customer had any earnings during a certain period and (2) whether he or she earned enough to obtain the maximum quarters of coverage for Social Security benefits during the period. Because the state's public-benefits data system has unreliable data on SSDI benefits and incomplete data on SSI payments, we relied mostly on DVR administrative data to examine benefit receipt at VR exit; we only used SSI data from the state's data system to conduct a sensitivity analysis of our main results.

We examined constructed outcome variables that account for differences in outcomes over time and for the uncertainty surrounding who received PE services. We derived parameters from ordinary least squares (OLS) regressions that were estimated based on customers who applied for VR in 2014. Using those parameters, we predicted what the outcomes and PE-receipt probabilities would have been for applicants in each cohort from 2009 through 2013 if these customers had applied in 2014 instead.

In addition, for each annual cohort, we used parameters from a separate OLS regression to predict the probability that someone who applied for VR in that year received PE services. To identify the causal effect of receiving PE services for each year, we regressed the difference between the 2014 cohort-predicted outcome and the applicant's observed outcome on the difference between (1) the 2014 cohort-predicted PE probability for an applicant with the same characteristics and (2) the predicted PE probability for an applicant with the same characteristics in the applicant's own annual cohort. The regression's estimated parameter is PE's impact on the outcome, as we explain below. The regressions controlled for the variables described in Table 1 as well as for DVR counselor and county of residence. We used

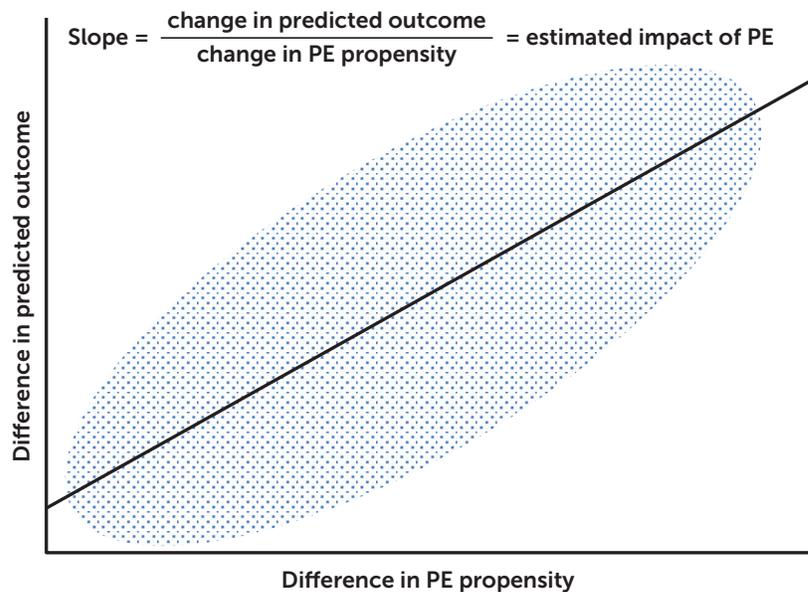
bootstrapping techniques to construct confidence intervals for each impact estimate (Efron and Tibshirani 1986).²

To understand the intuition behind this impact estimate, consider the estimate for employment at VR exit based on data for 2009 and 2014 applicants. Holding the observed applicant characteristics constant, the likelihood of PE is presumably at least as high for 2014 applicants as for 2009 applicants. As illustrated in Figure 1, differences in PE propensities will be larger for applicants with some characteristics than for others—namely, for applicants with characteristics that predict a relatively high likelihood of PE in 2014. If PE increases an applicant’s employment at VR exit, we would expect the difference between expected 2014 employment and actual 2009 employment to grow with the predicted difference between the PE participation propensity based on 2014 applicant data and the predicted difference based on 2009 applicant data. The method posits that the cause of this positive slope in Figure 1 is the expansion of PE.

Our method controls for the fact that 2009 and 2014 are at very different points in the business cycle, provided that the business cycle’s effect on expected outcomes (such as employment at VR exit) does not vary with observed applicant characteristics. We assessed the validity of this assumption by estimating impacts separately for applicants in each year from 2009 to 2013, using 2014 applicants as the comparison group in each case. If business-cycle effects vary substantially with the likelihood that an applicant with given characteristics would receive PE, we would expect to see major variation in the impact estimates for employment across the five years.

We also created a multiyear average estimate of impacts for each outcome, weighting each year’s contribution to the estimate in proportion to the number of applicants for that year who received PE. If the business-cycle assumption described previously is correct—and there are no other systematic reasons for variation in impact estimates across years—the multiyear impact estimate we constructed represents the best single estimate of PE’s impact during the five-year period.

Figure 1. Illustration of intuition behind the impact estimation method



² Confidence intervals were not adjusted for multiple comparisons.

