The Effects of Vocational Rehabilitation for People Who are Blind or Vision-Impaired

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Abstract

We construct a structural model of participation in vocational rehabilitation and labor market outcomes for people with vision impairments. There are multiple services to choose among, and each has different effects on employment and earnings. We estimate negative effects for most service types, leading to surprisingly low rates of return to VR service receipt. The negative returns are strongly affected by large administrative costs.

1 Introduction

The nearly 12% of American adults with blindness or vision impairments[1] often have weak attachments to the labor market.[2] Only 46.2% of working age adults with a vision impairment between are employed, compared to 77.0% of individuals without an impairment, and only 32.4% are employed full-time and full-year (versus 59.1% of individuals without an impairment) (Erickson et al., 2022). Of those who are not working, an estimated 7.7% are actively searching for employment. Compounding these challenges, people with visual impairments who are employed earn less than their non-disabled peers: conditional on being full-time and full-year employed, their median annual earnings are approximately $10,000 less than that of the rest of the population ($40,400 vs. $50,500). Thus, it is not surprising that, while only 10.7% of people without a visual impairment live below the poverty line, 26.1% of individuals who have a vision impairment fall in this category.

To address the employment and economic disparities faced by this vulnerable population, the Federal-State Vocational Rehabilitation (VR) program provides employment-related services to individuals with disabilities to help them acquire and maintain competitive, integrated employment. Each state, the District of Columbia, and all five U.S. territories administer their own VR program with support from Federal grants (Rehabilitation Services Administration - RSA, 2022). In 2020, the federal government budgeted $3.4 billion for the VR program, and states provided another $905 million for rehabilitation services (U.S. Department of Education, 2022). Of the over 800,000 people served by the VR program each year, about 41,000, or 5.17%, are vision-impaired.

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[1] In this paper, we think of blindness as an extreme form of vision impairments. From here on, when we refer to "vision impairment;" it includes "blindness."

[2] Estimates of the number of adult Americans with vision impairment vary between 7.5 million (2.3%) and 32.2 million (12.4%) (Villarroel et al., 2019; Erickson et al., 2022). The first statistic is calculated from the 2019 American Community Survey based on the answers of individuals of all ages to the question, "Is this person blind or does he/she have serious difficulty seeing even when wearing glasses?" The second statistic is based on data from the 2018 National Health Interview Survey which asks questions that allow for a more liberal definition of vision impairment of adults over the age of 18: "Do you have any trouble seeing, even when wearing glasses or contact lenses?" and "Are you blind or unable to see at all?"

Despite the large number of Americans with vision impairments, the large expenditures on the program designed to help them, and the emphasis placed on credible evaluation of the VR program,[3] little is known about the efficacy of public-sector VR programs in improving the labor market outcomes of individuals with visual impairments (Lund and Cmar, 2019). Many papers in the rehabilitation literature consider the effect of VR only on non-labor market outcomes for this population, especially for older individuals (Brody et al., 2002; Eklund et al., 2005; Brody et al., 2006; Christy et al., 2010; Girdler et al., 2010; Kempen et al., 2012; Elshout et al., 2018). Those that analyze labor market outcomes generally focus on the employment rates of VR clients with visual impairments soon after completing services (Capella, 2001; Warren, Giesen, and Cavenaugh, 2004; Estrada-Hernandez, 2008; Bell, 2010; Giesen and Lang, 2018) and/or estimate the "association" between employment and the characteristics and approach of the counselor or structure of agency (Capella-McDonnall, 2005; McDonnall, 2016). In general, this body of literature relies on small sometimes unrepresentative samples and does not examine long-run labor market outcomes (Lund and Cmar, 2019). None of these papers address the problem of endogenous VR service receipt.

To fill this gap, we use panel data from three state VR programs merged with labor market data from unemployment insurance (UI) records to estimate a structural model of the effect of VR services on labor market outcomes for people with vision impairments. Our panel contains information on all people who applied for services from the VR agencies in Virginia, Maryland, and Oklahoma in State Fiscal Year (SFY) 2007. For these individuals, we observe quarterly employment and earnings data as well as VR service data from 2004:q1 to 2012:q2. Observing individual quarterly employment and earnings prior to, during, and after service receipt allows us to estimate both the short- and long-run effects of VR services on employment and earnings. Given that VR programs do not provide a uniform treatment to all individuals (rather, participants work with counselors who tailor the specific services they receive), we estimate the effects of nine different types of services using instrumental variables techniques to account for endogenous provision of each of these services. We estimate the long-run labor market effects of VR programs for individuals with visual impairments. These estimates are used to compute the distribution of net present values of VR services.

We make several contributions to the existing literature. First, this is the first paper in the economic literature to consider the impact of VR on vision-impaired people. Of the evaluations of public-sector VR programs in the economics literature, more recent evaluations that take into account selection bias and other important econometric issues include Frölich, Heshmati, and Lechner (2004), Aakvik, Heckman, and Vytlacil (2005), Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019) (these last five papers will be referred to as DPSSS for the remainder of the paper), but none of these studies address questions specific to VR services for people who are vision-impaired.[4] Focusing on this subgroup is important for two reasons: a) the challenges faced by people who are vision-impaired are different than those provided to the general VR population. For instance, while descriptive, Warren-Peace (2009) reports that VR clients who are legally blind have much higher rates of non-competitive closures from VR (29.5%) than VR clients with any other disabilities (1.5%). Köberlein et al. (2013) estimate that productivity losses and absenteeism due to visual impairments in the United States and Canada are approximately $5.3 billion/year, and the cost of reduced labor force participation is approximately $7.4 billion/year. b) States take two different approaches in how they organize their agencies with respect to individuals who have vision impairments. Thirty-four states provide services to clients with all types of disabilities via a single, combined VR agency, while the rest provide VR services for people who are vision-impaired via a separate agency (i.e., Combined versus General and Blind VR Agencies).[5] We improve the generalizability of our results by analyzing data from states with both types of organizational structures. Virginia has a separate Blind VR Agency, while Maryland and Oklahoma have Combined VR Agencies.

Second, while most previous studies treat VR programs as a single intervention,[6] we examine specific types of services rather than just a single treatment indicator. In particular, we modify the service aggregation strategy used in DPSSS, adding two extra service categories of importance to people who are vision-impaired. The VR service types are 1) *diagnosis & evaluation*, 2) *training*, 3) *education*, 4) *restoration*, 5) *maintenance*, 6) *supported employment*, 7) *placement*, 8) *assistive technology*, and 9) *orientation & mobility*.[7] The last two are specific to people in our population of interest.

Third, we address the selection problem that arises if unobserved factors associated with VR service receipt are correlated

[3]U.S. GAO (2005, 2012) emphasizes the need for reliable evaluations of these VR programs. This need is even greater today with the passage of the Workforce Innovation and Opportunity Act of 2014 (WIOA) which requires VR programs to report post-closure employment and earnings. More generally, the Foundations for Evidence-Based Policymaking Act of 2018 requires all Federal agencies to make use of their data and credible statistical analyses when making decisions.

[4]Frölich, Heshmati, and Lechner (2004) use propensity scores but that allow for selection only on observables.

[5]Cavenaugh et al. (2006) and Warren-Peace (2009) find that the correlation between receipt of VR services and earnings at closure of consumers who are legally blind is significantly higher in blindness-specific agencies than in combined agencies. Steinman et al. (2013) identify associations between employment and the structure of the VR agency (e.g., Blind versus Combined Agency). Capella (2001), however, finds that agency type plays no role in future earnings.

[6]Leonard, D'Allura, and Horowitz (1999) and Giesen and Hierholzer (2016) are notable exceptions. For example, Leonard, D'Allura, and Horowitz (1999) find that assistive technology, training, and orientation & mobility improve employment outcomes.

[7]We use a different font throughout to denote services, explanatory variables, and dependent variables.

with labor market outcomes and result in biased estimates of the true effects of VR interventions. This selection problem is inherent in the VR context because of the individualized nature of the VR service determination process (e.g., counselors do not assign services at random; rather, they select services that they think will most benefit their clients) (Clapp et al., 2019). To address this issue, we construct and estimate a structural model of endogenous service provision and labor market outcomes that allows us to identify causal effects. Specifically, we construct a multivariate discrete choice model for service provision choices and augment that with a probit-like employment equation and a log earnings equation. We allow for correlation of errors among all of the equations to ensure consistent and efficient parameter estimates (White, 1982). We use counselor-assignment design instrumental variables that are assumed to impact service receipt but not the pre- and post-service receipt latent labor market outcomes. The instruments influence who receives services. This model of the selection process provides structure and detail on how service receipt and labor market outcomes are correlated.

Fourth, while the analysis in this paper is similar to DPSSS, we extend the models in this line of research in a key way. Due to important differences across VR programs, previous models were estimated separately for each state/agency. However, there are not a sufficient number of individuals with visual impairments receiving VR services in any of the states for which we observe data to estimate the parameters of a model as rich in parameters as those in DPSSS. To address this issue, we reformulate the model to accommodate estimation based on observations pooled from multiple agencies. Despite this, we are still able to allow the effects of service receipt to vary across states. This extended model allows for future analyses of individuals with infrequently observed disabilities (e.g., autism or traumatic brain injury) that previously would have been infeasible.

Finally, our paper also solves a number of the problems in the existing literature (Lund and Cmar, 2019): our sample size is large and based on data from multiple agencies, our data on labor market outcomes are longitudinal, and our methods, building on DPSSS, are rigorous and allow us to make causal inferences. Using our longitudinal data, we provide estimates of the effects of an array of services on VR clients with visual impairments and translate them into distributions of net present values for VR services. We first document substantial heterogeneity in the VR clientele and services across the three states. Then we present the first rigorous, long-run analysis of employment and earnings among VR applicants with visual impairments.

The remainder of the paper proceeds as follows: Section 2 describes the economic model used throughout the paper. We construct a multivariate discrete choice model for service provision choices and augment that with a probit-like employment equation and a log earnings equation. We allow for correlation of errors among all of the equations. Next, we describe the four sources of data used in our analysis in Section 3. Estimation results are presented in Section 4. Next, a rate-of-return analysis is presented in Section 5. Our results imply that VR services for people who are vision-impaired generally are not effective with respect to improving labor market outcomes. For many clients, the rate of return in the labor market is negative. While our focus on labor market outcomes is germane for an evaluation of the VR program, for the population of vision-impaired clients we miss important benefits that the VR program may have on improving independent living skills. To the extent that this is an important benefit, our results understate the overall return to the VR program. Our paper finishes with conclusions.

2 Model

We use a multivariate discrete choice model for service provision choices and augment that with a probit-like employment equation and a log-earnings equation.[8] As in much of the program evaluation literature, we focus on program applicants and do not model the application decision.[9]

Let y^*_{sij} be the value for individual i in state s of participating in VR service j, $j = 1, 2, .., J$, and define $y_{sij} = 1\left(y^*_{ij} > 0\right)$

[8] The model is quite similar to that in DPSSS, and the notation is the same as in Dean et al. (2017) except for a few small deviations.

[9] According to Owsley et al. (2009) (Table 1 and Figure 1), 28% of vision-related rehabilitation services are provided by government agencies. This suggests that an important issue to address is who (in a selection sense) uses government agency services. We do not address this issue here because we do not have data that would allow us to do so. Dean et al. (2017) find a way to address this problem for people with mental illness by constructing a measure of the number of people with mental illness by county using methodology developed in Stern (2014) and Johnson et al. (2017). Dean et al. (2019) do the same for transitioning youth. But there is no equivalent way to do the same for vision-impairment because the data needed for Stern (2014) and Johnson et al. (2017) do not exist for vision-impaired people.

Ipsen and Stern (2020) focus on the application decision and the effect of ruralness on application. They find that ruralness has a significant effect on application. However, this implies selection bias only if ruralness is not included in the set of explanatory variables affecting the labor market outcome variables.

as an indicator for whether i receives service j. Assume that

$$\begin{aligned} y^*_{sij} &= \beta_{sj} + X^y_{si}\beta_j + u^y_{sij} + \varepsilon_{sij}, \\ \varepsilon_{sij} &\sim iidEV, \\ y_{sij} &= 1\left(y^*_{sij} > 0\right) \end{aligned} \tag{1}$$

where X^y_{si} is a vector of exogenous explanatory variables with coefficients β_j that vary across services but do not vary across states, and u^y_{sij} is an error whose structure is specified below. This model is a multivariate discrete choice model with error structure similar to McFadden and Train (2000).

Next, we introduce two equations associated with the value of working and log-quarterly earnings. Let z^*_{sit} be the value to i from state s of working in quarter t, and define $z_{sit} = 1\left(z^*_{sit} > 0\right)$ as an indicator for whether i works in quarter t. Assume that

$$z^*_{sit} = \gamma_{s0} + X^z_{sit}\gamma + \sum_{k=1}^{K}\sum_{j=1}^{J}\alpha^z_{sjk}d_{sik}y_{sij} + u^z_{sit} + \upsilon^z_{sit} \tag{2}$$

where X^z_{sit} is a vector of (possibly) time-varying, exogenous explanatory variables, d_{sik} is a dummy variable equal to one iff the amount of time between the quarter of application and t is between time nodes τ_k and τ_{k+1}, and u^z_{sit} and υ^z_{sit} are errors whose structure is specified below. The time periods implied by the nodes we use are a) 2 or more quarters prior to application, b) 1 quarter prior to application, c) 1 quarter after application to 10 quarters after application,[10] and d) 11 or more quarters after application.

Let w_{sit} be the log quarterly earnings of i at t, and assume that

$$w_{sit} = \delta_{s0} + X^w_{sit}\delta + \sum_{k=1}^{K}\sum_{j=1}^{J}\alpha^w_{sjk}d_{sik}y_{sij} + u^w_{sit} + \upsilon^w_{sit} \tag{3}$$

where variables are defined analogously to equation (2). Equations (2) and (3) together are similar to Heckman (1974).

Two other features of the model are important to highlight. First, to keep the parameter space manageable, many parameters are restricted to be constant across states. Parameters that vary across states are constants and spline level constants in each of the three equations (β_{s0}, δ_{s0}) along with the effects of service receipt on labor market outcomes (α^z_{sjk}, α^w_{sjk}). We allow the effects of service receipt to vary across states because one of the goals of the research is to measure these differences.

Second, some people become vision-impaired just prior to VR application when they develop the disability. For such people, the time prior to the disability corresponds to quarters when the individual is healthy and, therefore, has better labor market outcomes than in quarters just after the onset of the disability and after VR application. This makes VR service receipt look less productive. We use the approach in the Dean et al. (2018) analysis of VR clients with physical disabilities to model the onset of vision impairment for VR clients who are not congenitally blind. The resulting estimated disability onset curve is provided in Appendix A.7.

Finally, to allow for a rich correlation structure within and across these equations, we assume that

$$\begin{aligned} u^y_{sij} &= \lambda^y_{j1}e_{si1} + \lambda^y_{j2}e_{si2}, \\ u^z_{sit} &= \lambda^z_1 e_{si1} + \lambda^z_2 e_{si2} + \eta^z_{sit}, \\ u^w_{sit} &= \lambda^w_1 e_{si1} + \lambda^w_2 e_{si2} + \eta^w_{sit}, \\ \eta^z_{sit} &= \rho_\eta \eta^z_{sit-1} + \zeta^z_{sit}, \\ \eta^w_{sit} &= \rho_\eta \eta^w_{sit-1} + \zeta^w_{sit}, \\ \begin{pmatrix} \zeta^z_{sit} \\ \zeta^w_{sit} \end{pmatrix} &\sim iidN\left[0, \Omega_\zeta\right], \\ \begin{pmatrix} e_{si1} \\ e_{si2} \end{pmatrix} &\sim iidN\left[0, I\right], \\ \upsilon^z_{sit} &\sim iidN\left[0, 1\right], \text{ and} \\ \upsilon^w_{sit} &\sim iidN\left[0, \sigma^2_w\right]. \end{aligned} \tag{4}$$

[10] DPSSS use an 8 quarter cutoff. We increase the cutoff in this paper because people with visual impairments take longer to finish services. Allowing for the extra time has a significant positive effect on the estimates of quarterly earnings coefficients.

The norm in much of this literature is to specify person-specific unobserved heterogeneity as a one-factor model with a normal density, a one-factor model with a multi-point discrete distribution, or a two-factor model with a mixture of normal random variables for each factor (e.g., Heckman, Stixrud, and Urzúa, 2006; Conti, Heckman, and Urzúa, 2010). We include (e_{si1}, e_{si2}) to allow for two common factors affecting all dependent variables with factor loadings $\left(\lambda^y_{jk}, \lambda^z_k, \lambda^w_k\right)^2_{k=1}$.[11] We also allow for serial correlation and contemporaneous correlation in the labor market errors $(\eta^z_{sit}, \eta^w_{sit})$. The covariance matrix implied by this error structure is presented in Appendix A.1. A similar error structure is used in DPSSS.

2.1 Identification

Complementing our structural model with common factors (e_{si1}, e_{si2}), two approaches are used to address the selection problem that service receipt and labor market outcome variables are likely to be endogenous. First, we control for pre-treatment labor market differences between those who do and do not receive services. If the differences in unobserved factors that confound inference in equations (2) and (3), u_{sit}, are fixed over time, then this controls for the observed pre-treatment labor market differences. This method of controlling for selection, which is the central idea of the difference-in-difference design, is used extensively in the literature (e.g., Meyer, 1995; Heckman et al., 1999).

Second, we include two instruments in equation (1) that are excluded from equations (2) and (3). As described in Section 3.1.3 and Appendix A.4, our choice of instruments for service j in Virginia and Maryland is the propensity of an individual's counselor to assign other clients to service j. For Oklahoma, we develop and use a different instrument with a similar motivation because of limitations in the Oklahoma data. Similar instruments are applied in Kling (2006), Doyle (2007; 2008), Maestas, Mullen, and Strand (2013), and DPSSS. Excluding these instruments from the labor market equations requires that counselor assignment only affects labor market outcomes through the services that the client receives and is sensible. As illustrated in Sections 3.1.3 and 4.2, these variables are strongly correlated with service receipt. However, it must also be the case that these variables are exogenous or unrelated to the structural errors. While one can never be certain this holds, there are good reasons to think it is a reasonable assumption especially given that we include in the analysis the client's observed limitations, county-level employment rates, and pre-service labor market outcomes. Most notably, VR clients have limited ability to select their counselor; conditional on observed limitations and geography, counselors are randomly assigned.[12] So, unless clients relocate to take advantage of the practices of particular field offices, the assignment to counselors is effectively random conditional on the observed limitations of clients. A threat to the validity of these instruments may arise if variation in the availability of jobs where training (or other VR services) is productive might jointly affect labor market outcomes and the average behavior of counselors. Including measures of local labor market conditions directly in labor market outcome equations (2) and (3) should ameliorate this problem. Another concern arises if there is unobserved variation in the ability of counselors to match clients with jobs that affects both his/her decisions about what types service to offer clients and later success in the labor market. We assume that this type of confounding effect is not important. Finally, one might worry that variation in counselor behaviors impacts the decision to apply for services and thus result in endogenous application decisions. Dean et al. (2017, 2019) test for such effects and both accept the null hypothesis of no bias.

Our approach for addressing the endogenous selection of services represents a substantial advance over the existing literature where the past research generally relies on limited controls for pre-program earnings and assumes service participation is otherwise exogenous. Along with Aakvik, Heckman, and Vytlacil (2005) and DPSSS, this is one of the first studies to identify the impact of VR services on labor market outcomes using both a history of pre-program earnings and plausibly exogenous instrumental variables. It is the very first to use such methods applied to VR clients who are vision-impaired.

2.2 Estimation

We estimate the parameters of the model using maximum simulated likelihood (MSL). It is straightforward to simulate the likelihood function using well-known methods described in Stern (1997). The functional form of the conditional likelihood contribution associated with observed program choices follows from the assumption in equation (1) that the idiosyncratic errors are iidEV. The functional form of the conditional likelihood contribution for labor market outcomes follow from the

[11] Heckman, Stixrud, and Urzúa (2006) and Conti, Heckman, and Urzúa (2010) are more general than our specification, but they rely on the existence of other information about the latent factors to identify the mixture. The models with one factor and a multi-point discrete distribution are more general than our specification (given the asymptotic approximation described in Heckman and Singer, 1984) in the number of parameters included in the unobserved heterogeneity density but less general in terms of the number of independent factors to use.

[12] Counselors are assigned by office policy that does not involve client choice. For example, some field offices assign counselors to balance caseload across counselors and some assign counselors by client locale.

normality assumption for $(\upsilon^z_{sit}, \upsilon^w_{sit})$ and the bivariate normality assumption for $(\zeta^z_{sit}, \zeta^w_{sit})$ in equation (4). Additional details are presented in Appendix A.2.

3 Data

We use two main data sources for each state: a) the administrative records for the SFY 2007 applicant cohort and b) the quarterly administrative records on labor market activity of the UI agency in the state from 2004:q1 to 2012:q2 for those people in the VR agency data. We also merge these files with data from the Bureau of Economic Analysis (BEA) on county-specific employment patterns. Each of these is discussed in turn below.

3.1 VR Administrative Data

3.1.1 VR Sample Frame

Our starting point is the administrative records of the three state VR agencies for individuals who applied for VR services in SFY 2007 (July 1, 2006 - June 30, 2007). Our analysis focuses on $1,964$ state VR clients who have vision impairments: 598 in Maryland, 953 in Oklahoma, and 413 in Virginia.[13]

3.1.2 VR Agency Data for Service Provision

For VR clients, the counselor and individual develop an individualized plan for employment (IPE) which specifies the array of services to be provided. There are between 48 to 81 separate services provided by state VR agencies (varying by state). Starting with Dean et al. (2002) and then adjusting to take into account changes in the frequency of different services since 2000 and services of particular interest for individuals who are vision-impaired, we aggregate these services into the nine service types listed in Table 1.

The set of aggregated services are:

- *Diagnosis & evaluation*: services for assessing eligibility and developing an IPE as well as medical diagnostics;
- *Training*: career and technical training including vocational, job readiness, on-the-job training, GED, business and vocational schools;
- *Education*: college and university;
- *Restoration*: medical/health care service;
- *Maintenance*: extra living expenses (such as shelter, food, clothing, incidentals) needed for an individual to participate in VR, as well as VR-related transportation and vehicle/home modifications;
- *Job placement*: job search and placement assistance including resume preparation, developing interview skills, identifying job opportunities, referral to a specific job;
- *Job supports*: job coaching, supported employment (but not sheltered workshops);
- *Assistive technology* (also called rehabilitation technology): devices that aid in living and working with blindness or low vision, including low-vision devices, computer hardware and software, and worksite modifications; and
- *Orientation & mobility*: included in the more general category of disability-related, augmentative-skills training. Although the largest portion, this broader category also includes training for travel, home, and navigating rehabilitation.

The state VR administrative records provide information on types of purchased services.[14] The numbers in Table 1 show wide variation in the prevalence of different services and the source of services across states. The three most commonly used purchased services in Maryland are *maintenance* (0.468), *diagnosis & evaluation* (0.319), and *assistive technology* (0.273). In Oklahoma, they are *diagnosis & evaluation* (0.563), *restoration* (0.562), and *maintenance* (0.382), while, in Virginia, they are *maintenance* (0.513), *restoration* (0.332), and *diagnosis & evaluation* (0.293). There are some notable differences across states: in Oklahoma, 56.2% of clients receive *restoration*, while, in Maryland, only 11.4% receive *restoration*; in Virginia, 12.6% receive *placement*, while, in Oklahoma, only 4.8% receive *placement*; in Maryland, 27.3% receive *assistive technology*, while, in Virginia, only 2.9% receive *assistive technology*.

[13] A more thorough description of the data is available in Clapp et al. (2020). Some of the data tables in this paper also appear in Clapp et al. (2020).

[14] In addition to purchased services, some VR services can be provided in-house. However, we exclude in-house services from the analysis because of the very low proportions for receipt in our data. It may be that in-house services are just not recorded well by counselors in the data system as the three agencies included in our data and other agencies not included in our analysis report high levels of in-house services, especially for *assistive technology* and *orientation & mobility*.

Table 1: Means of Service Receipt and Explanatory Variables

Variable	Mean in Maryland	Mean in Oklahoma	Mean in Virginia
# Observations	598	767	413
		Purchased Service Receipt Propotions	
Diagnosis & Evaluation	0.319	0.563	0.293
Training	0.199	0.137	0.194
Education	0.079	0.108	0.102
Restoration	0.114	0.562	0.332
Maintenance	0.468	0.382	0.513
Placement	0.070	0.048	0.126
Supported Employment	0.064	0.033	0.058
Assistive Technology	0.273	0.176	0.029
Orientation & Mobility	0.049	0.034	0.029
		Explanatory Variables	
Male	0.480	0.463	0.554
White	0.428	0.803	0.608
Native American	0.015	0.126	0.002
HS Diploma	0.343	0.360	0.298
Some College	0.249	0.274	0.177
College Degree	0.159	0.061	0.182
Education Missing		0.012	0.036
Age/100	0.418	0.468	0.374
Cognitive Impairment	0.037	0.009	0.012
Hearing Impairment	0.075	0.060	0.044
Physical Impairment	0.518	0.459	0.235
Learning Disability	0.054	0.021	0.005
Mental Illness	0.161	0.104	0.019
Substance Abuse	0.070	0.021	0.002
Disability Significant	0.084	0.260	0.942
Disability Most Significant	0.779	0.588	0.039
Government Assistance	0.503	0.265	0.487
Veteran	0.030	0.066	0.005
Congenital Blindness	0.227	0.165	0.196
Fed Govt Employment Adjustment	-0.209	-0.174	-0.185
Commuting Adjustment	-0.164	-0.168	-0.161
Young Age Dummy	0.030	0.054	0.143
Prior VR Spell Dummy	0.095	0.140	

3.1.3 VR Agency Explanatory Variables

Table 1 also provides the sample means for the explanatory variables from each of the state agency data sources. Standard demographic variables include race, gender, age, and level of education. Note the large proportion of Native Americans in Oklahoma which allows us to estimate the effect of being Native American on labor market outcomes in Appendix A.5. Education variables include *no high school diploma* (reference group), *HS diploma*, *some college*, *college degree*, and *education missing*. The last education-related variable is a dummy for observations missing education information used to avoid losing a significant number of observations in Virginia and Oklahoma. Oklahoma has many fewer people with a college education than Maryland and Virginia. There is a *government assistance* variable measuring the monthly amount of assistance the applicant received from TANF, SSI, SSDI, or other government programs. On average, applicants received $397 monthly in the application quarter (including zeroes).[15] There are also two adjustment variables to control for employment by the federal government and cross-state employment. Both of these variables are discussed in more detail in Section 3.3.

There are also a number of dummy variables relating to disability. One set identifies disabilities coexisting with vision impairment. A second set provides a measure of the significance of the disability: not significant (reference group), significant, and most significant. The significance variables are used to ration services in periods of limited funding (known as order of selection quarters) with the most significant receiving the highest priority. The numbers for both sets of variables are notably different and somewhat suspect in Virginia. Fewer individuals are diagnosed with coexisting disabilities, and very few are identified as having a most significant disability. The blind agency in Virginia was never under order of selection,[16] and, consequently, identifying someone as most significantly disabled had little relevance to counselors. Additionally, Virginia's blind agency is a stand-alone agency and may not have kept track of other disabilities very well. There is a *congenital blindness or vision impairment* dummy variable, a *young age* dummy for individuals under the age of 18 during service, and a *prior VR spell* dummy to capture the selection effects associated with those clients who were returning after a prior service spell. For some explanatory variables, there is significant variation across states in means. These include *White* (0.803 in Oklahoma to 0.428 in Maryland),[17] *Native American* (0.126 in Oklahoma and very small in the other two states),[18] *physical impairment* (0.518 in Maryland to 0.235 in Virginia), *mental illness* (0.161 in Maryland to 0.019 in Virginia), *veteran* (0.066 in Oklahoma to 0.005 in Virginia), and *young age dummy* (0.143 in Virginia to 0.030 in Maryland).

Finally, to identify the impact of services on labor market outcomes, we use two instrumental variables that are correlated with the treatment assignment but not included in the labor market equations (2) and (3). For Virginia and Maryland, these instruments are the proportion of other clients in our cohort for the individual's counselor receiving a particular service. These variables are transformed as is described in Appendix A.4. Two features of these instrumental variables are worth highlighting. First, the number of clients varies across counselors. In Virginia, the mean number of clients per counselor is 5.2, and the standard deviation is 9.3. In Maryland, the mean is 21.7, and the standard deviation is 8.9. About 10% of applicants have missing counselor information or have a counselor with one or two clients. For such cases, we include a variable indicating the counselor information is missing. Second, the proportion of clients receiving each service varies by counselors. For example, Table 2 shows descriptive statistics for counselor-based instruments. For *diagnosis & evaluation* in Virginia, the mean proportion of the each client's counselor's assignment of *diagnosis & evaluation* across counselors is 0.807, and the standard deviation is 0.264. Standard deviations are large relative to means, and the range of values is $0.0 - 1.0$ for most services. Maryland has similar descriptive statistics. Overall, there is strong evidence of meaningful variation in behavior across counselors. Using a likelihood ratio test, we reject the null hypothesis that the joint density of services is the same across counselors. The fact that there is significant variation in the provision of services across counselors makes our instrument viable.

Counselor is not a meaningful concept in Oklahoma. While counselors exist and provide services, the information on counselors is not informative. Thus, instead of using counselor, we use a weighted average of service receipt where the weights are inversely proportional to the distance between each individual and other individuals. Distance is used because it is more likely that two individuals living close to each other are likely to use the same counselor than two individuals far apart. See Appendix A.4 for further detail.

[15] The variable measuring government financial assistance may be endogenous. However, for this population, the income thresholds at which government benefits are reduced or eliminated is relatively high.

[16] Order of selection is a situation where the state's VR agency or agencies are limited in the number of people that can receive services usually because of state fiscal problems. When order of selection is in place, the VR agency is required to choose clients for service receipt based on their severity measure (not significant disability, significant disability, and most significant disability).

[17] According to US Census Bureau (2016), the proportions of the population that were white in 2010 were 54.7% in Maryland, 68.7% in Oklahoma, and 64.8% in Virginia.

[18] The number of Native Americans in Oklahoma in the 2010 Census is $482,760$ (Norris, Vines, and Hoeffel, 2012), and the number of people in Oklahoma in the 2010 Census is $3,751,351$ (US Census Bureau, 2018). Thus, the proportion of the population that is Native American in 2010 is 0.129.

Table 2: Descriptive Statistics for Counselor-Based Service Instruments

	Maryland		Virginia		Oklahoma	
Service	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Diagnosis & Evaluation	0.711	0.208	0.807	0.264	0.238	0.184
Training	0.324	0.270	0.321	0.354	-1.081	0.167
Education	0.113	0.132	0.043	0.112	-1.250	0.244
Restoration	0.177	0.168	0.303	0.365	0.141	0.143
Maintenance	0.707	0.219	0.497	0.305	-0.277	0.179
Placement	0.128	0.172	0.236	0.299	-1.644	0.356
Supported Employment	0.074	0.125	0.187	0.244	-1.913	0.292
Assistive Technology	0.617	0.290	0.196	0.324	-0.764	0.243
Orientation & Mobility	0.115	0.102	0.044	0.081	-2.044	0.423

Note: The instrument for Maryland and Virginia is different than for Oklahoma.

Table 3: Employment and Conditional log Quarterly Earnings Before and After Service Receipt

	Employment			log Quarterly Earnings		
	Maryland	Oklahoma	Virginia	Maryland	Oklahoma	Virginia
Before						
# Obs	7366	11723	5104	2372	3362	1881
Mean	0.322	0.287	0.369	7.998	7.894	8.166
Std Dev	0.467	0.452	0.482	1.363	1.185	1.171
After						
# Obs	12966	20679	8938	3131	5425	2451
Mean	0.241	0.262	0.274	7.950	7.894	8.119
Std Dev	0.428	0.440	0.446	1.401	1.185	1.360

3.2 State Unemployment Insurance Data

In addition to the state VR administrative data, we also have data on individual quarterly earnings prior to, during, and after service receipt. In particular, this study uses quarterly employment records provided by employers to the state agency collecting and storing UI data for purposes of determining eligibility for unemployment insurance benefits. Details on how the UI data are matched to the state VR administrative data are provided in Dean et al. (2017).

In our analysis, we try to explain two labor market outcome variables: *employment* and conditional *log quarterly earnings*.[19] *Employment* is a binary measure of working in a particular quarter in the labor market and is modeled in equation (2). We model *log quarterly earnings* in equation (3). While it would be valuable to decompose quarterly earnings into wage level and hours, this is not possible in the state UI data. Table 3 provides information on sample sizes and on the moments of employment data and earnings data disaggregated between quarters before and after initial service provision. Both employment rates and log quarterly earnings decline after service provision. For example, the mean employment rates are approximately 30% prior to service and 25% after service (as compared to about 80% for the general population).

An interesting feature of the earnings data is that there is a large drop in the density of earnings at the substantial gainful activity (SGA) rate of $4,200 per quarter. In particular, only 15.3% of employed VR clients earn more than the SGA prior to service receipt, only 12.6% earn more than the SGA in the short run after service receipt, and only 18.5% earn more than the SGA in the long run after service receipt. A vision-impaired person with SSDI loses their benefits (SSDI payment and Medicaid eligibility) beyond the SGA. Thus, people receiving SSDI have a strong incentive not to earn more than the SGA in quarterly earnings. This incentive might explain some of the small effects of VR services on log quarterly earnings discussed in Section 4.1. But, it has no obvious impact on the estimated small effects of VR services on employment.

Figure 1 shows how employment rates change from 8 quarters prior to VR service receipt to 20 quarters after service

[19] Employers report aggregate earnings in a given quarter to the state UI agency. Recall that equations (2) and (3) model employment and earnings impacts in four separate periods offset from the date of first service. Because the date of first service can fall anywhere within a quarter, that quarter is excluded from the analysis other than for use as a period of demarcation separating pre-service from post-service periods. Depending upon the date of first service, this alignment procedure results in 12 quarters of pre-service earnings periods and 26 quarters post-service quarters for individuals in this cohort.

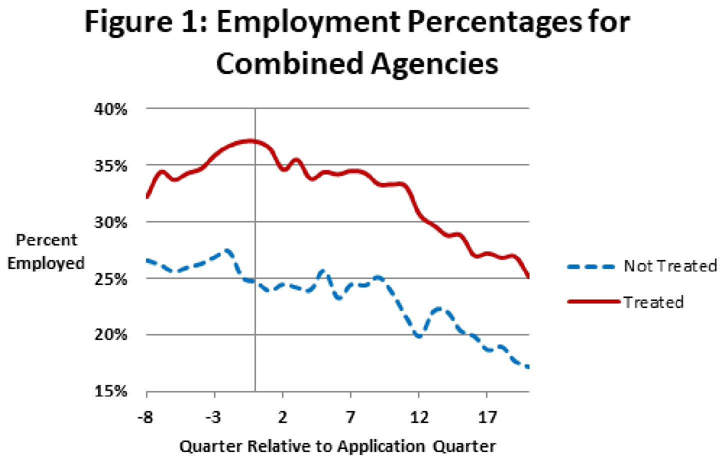

Figure 1: Employment Percentages for Combined Agencies

receipt.[20] Previous analyses of VR use labor market outcomes soon after case closure. The figure shows, first of all, that clients receiving substantial VR services (treated) have significantly higher employment rates than applicants not receiving service (untreated). The range of post-service employment rates is from 17.2% to 25.7% for not treated and from 25.1% to 36.5% for treated. Bell and Mino (2015) report an employment rate of 37% for people with vision impairments, a moderate amount higher than ours but similar to most of the literature (see Bell, 2010).

The ranges of rates, by themselves, tell us only that service recipients are better connected to the labor market than service non-recipients. It does not imply that service receipt improves employment rates. In fact, the treated and untreated curves start out 8 quarters prior to VR admission, differing by 5.6%, increase to a 9.3% difference 2 quarters prior to admission, are 9.8% different 4 quarters after, and are 7.9% different 20 quarters after admission. Many researchers would measure the improvement in employment by comparing the difference (treated minus untreated) in employment rates after VR admission to the difference in employment rates prior to admission, possibly with a correction for selection bias. Using this approach (without a selection correction), there is no obvious improvement in employment outcomes of service receipt.

Figure 2 has the same structure as Figure 1 but for mean quarterly earnings for those who are employed. As with Figure 1, those treated have higher earnings than those not treated. However, the difference in earnings between treated and not treated increases after service receipt and permanently. Thus, service receipt is positively associated with quarterly earnings. In both figures, it is clear that focusing on employment and earnings at closure or soon after the end of service receipt would lead to misleading results. All four curves in the two figures move significantly over the course of the 20 quarters after VR admission.

While these figures illustrate the association between VR services and labor market outcomes, we caution readers not to draw causal conclusions about the effects of VR from these descriptive associations. Dean et al. (2015, 2017, 2018, 2019) show that the type of analysis associated with Figures 1 and 2 lead to seriously biased estimates of VR returns. In particular, a) it is important to control for explanatory variables of the type listed in Table 2; b) it is important to understand that different service types of the kind displayed in Table 1 have different effects on labor market outcomes; and c) it is important to control for the endogeneity of service choices. Leonard, D'Allura, and Horowitz (1999) find that *assistive technology*, *training*, and *orientation & mobility* improve employment outcomes. But they have no controls for endogeneity. Also, missing from the analysis of Figures 1 and 2 is the effect of the financial crash of 2008 on aggregate unemployment. We follow the approach in Schmidt et al. (2019) to control for such an effect. We divide time up into 4 multi-quarter segments as in equations (2) and (3) and allow the intercept to vary for each segment, for each state, and for both labor market outcomes.

3.3 Other Data

Labor market outcomes may be influenced by local conditions. To account for local labor market differences, we construct measures of log employment rates using county level data from the BEA on population size and number of people employed,

[20] This figure and the next also are displayed in Clapp et al. (2020).

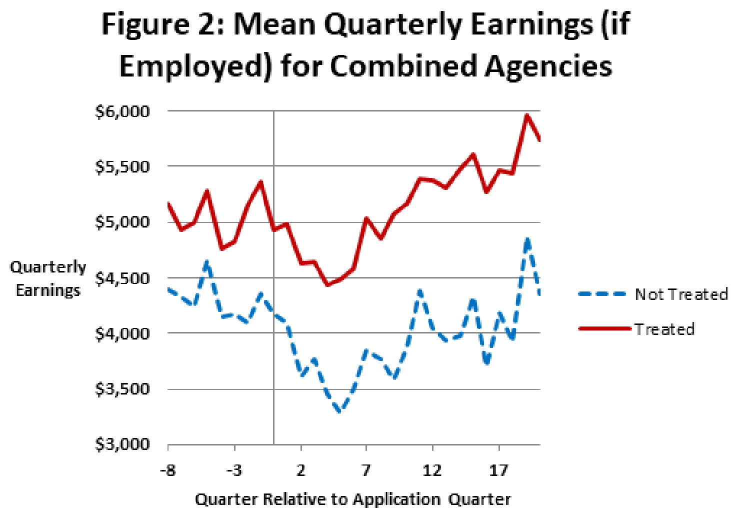

Figure 2: Mean Quarterly Earnings (if Employed) for Combined Agencies

disaggregated by county (BEA, 2010). These measures are merged to the VR client data using geographic identifiers that allow us to match each VR client with their county of residence.

We also control for biases due to individuals working for the federal government or crossing state lines to work. These cause a bias because both groups of workers are not covered by UI and are therefore not in our UI employment data.[21] Using data from Eye on Washington (2015) and US Census Bureau (2015), we construct a table of proportion of county residents who work for the federal government or who cross state lines to work. Then, we transform them into two explanatory variables in our model. The transformed cross-state lines variable should be interpreted as the reduction in the UI reported employment rate due to cross-state workers. The transformed federal government variable may capture two effects: a) the reduction in the UI reported employment rate due to federal government employment and b) any true spill-over effect of the existence of federal government jobs on employment opportunities for VR clients. Details are available in Appendices A.3 and A.8.

4 Estimation Results

4.1 Estimates of Impact of VR Services

We begin by examining the estimated effect of services on labor market outcomes. Table 4 presents the estimates and associated standard errors for the pre-application coefficient estimates and the estimated short- and long-run effects of services on *employment propensity*, the latent variable z^*_{sit} in equation (2). The effects are allowed to vary across the three states, the nine different service types, and the three different time periods of interest relative to the initial service quarter. Given our rich labor market data, we are able to estimate both short-run (the first 10 quarters) and long-run (more than 10 quarters) effects of services and account for pre-service outcomes in the quarter prior to services as well as two or more quarters prior to the initial service. As noted in Section 2.1, inclusion of pre-treatment quarters is a way to account for the effect of endogenous selection into services.[22]

The first column of state level estimates in Table 4, which displays estimates for the quarters prior to the initial service, provides evidence that selection is endogenous. A significant proportion of the coefficients associated with two or more quarters prior to the initial service are substantial and statistically different than zero. Clients in Maryland with high

[21] Dean et al. (2017) report that these groups represent approximately 12% of employment in Virginia. However, data from Ann Banton at the Virginia Department for the Blind and Visually Impaired implies small numbers with the average proportion of VR recipients who become employed by the federal government relative to all of those employed between 2007 and 2012 is 3.6%.

[22] The quarter immediately prior to initial service provision is separated out because this quarter seems likely to have a distinct impact on selection and because of the well-documented variation in labor market behaviors just prior to the application period – the Ashenfelter dip (Ashenfelter, 1978). The Ashenfelter dip parameter estimates are not reported in Table 4, but they are available from the corresponding author.

Table 4: Service Effects on Employment Propensity

	Maryland			Oklahoma			Virginia		
	Before	SR Effect	LR Effect	Before	SR Effect	LR Effect	Before	SR Effect	LR Effect
Diagnosis & Evaluation	0.231 **	-0.055 **	0.219 **	0.328 **	-0.519 **	-0.790 **	-0.170 **	-0.184 **	0.095
	(0.034)	(0.027)	(0.040)	(0.036)	(0.033)	(0.047)	(0.065)	(0.055)	(0.101)
Training	-0.245 **	-0.020	0.276 **	-0.206 **	-0.034	0.493 **	0.135	-0.331 **	-0.310 **
	(0.054)	(0.038)	(0.052)	(0.055)	(0.042)	(0.069)	(0.084)	(0.071)	(0.112)
Education	-0.214 **	-0.255 **	0.176 **	-0.041	0.011	0.444 **	-1.280 **	0.607 **	1.232 *
	(0.075)	(0.055)	(0.086)	(0.047)	(0.040)	(0.061)	(0.149)	(0.129)	(0.167)
Restoration	0.277 **	-0.587 **	-0.555 **	0.640 **	-0.528 **	-0.653 **	0.317 **	-0.103 **	-0.026
	(0.069)	(0.046)	(0.086)	(0.038)	(0.033)	(0.046)	(0.069)	(0.044)	(0.073)
Maintenance	-0.225		-0.068	-0.068 **	-0.443 **	-0.585 **	0.081	-0.303 **	-0.769 **
	(0.051)		(0.064)	(0.034)	(0.031)	(0.048)	(0.060)	(0.051)	(0.103)
Placement	-0.739 **	0.344 **	-0.074	0.072	-0.403 **	-0.081	0.033	0.566 **	0.663 **
	(0.091)	(0.067)	(0.115)	(0.074)	(0.062)	(0.085)	(0.098)	(0.084)	(0.159)
Supported Employment	0.188	0.310 **	0.391 **	-0.345 **	0.496 **	0.402	-0.418 *	0.472 **	0.946 *
	(0.120)	(0.071)	(0.175)	(0.129)	(0.161)	(0.251)	(0.257)	(0.176)	(0.483)
Assistive Technology	0.218 **	-0.365 **	-0.321 **	-0.145 **	-0.413 **	-0.320 **	-0.644 *	0.252	0.445
	(0.047)	(0.034)	(0.065)	(0.043)	(0.034)	(0.051)	(0.352)	(0.283)	(0.422)
Orientation & Mobility	-0.428 **	-0.538 **	0.514 **				-0.072	-0.108	-0.375
	(0.114)	(0.101)	(0.149)				(0.276)	(0.283)	-0.446

Note: Items with double stars are statistically significant at the 5% level, and items with single stars are statistically significant at the 10% level.

employment probabilities prior to service are more likely to use *diagnosis & evaluation*, *restoration*, *supported employment*, and *assistive technology*, and they are less likely to use *training*, *education*, *maintenance*, *placement*, and *orientation & mobility*. Overall, these results suggest a complex and heterogeneous selection process where applicants are assigned to particular services based on underlying unobserved factors that are associated with pre-service labor market outcomes.

The second and third column of state level estimates in Table 4 display the estimated short- and long-run effects of services on *employment propensity*. These estimates are measured relative to the coefficients associated with pre-service measures in the first column.[23] For example, for clients in Maryland, prior to service provision the employment propensity for clients provided training is -0.245 less than for clients that do not receive these services. In the 9-quarters after the start of service provision, it falls to -0.265, and then, in the longer run, it rises to 0.031. Thus, after accounting for selection into service, the short-term effect of training is $0.245 - 0.265 = -0.02$, and the long-term effect is $0.245 + 0.031 = 0.276$. For Maryland, we observe that *diagnosis & evaluation*, *training*, *education*, *supported employment*, and *orientation & mobility* increase *employment propensity* in the long run, while *restoration*, *maintenance*, *placement*, and *assistive technology* decrease it. For Oklahoma, we observe that *training*, *education*, *supported employment*, and *assistive technology* increase *employment propensity* in the long run, while *diagnosis & evaluation*, *restoration*, *maintenance*, and *placement* decrease it. For Virginia, we observe that *diagnosis & evaluation*, *education*, *placement*, *supported employment*, and *assistive technology* increase *employment propensity* in the long run, while *training*, *restoration*, *maintenance*, and *orientation & mobility* decrease it.

Table 5 provides similar information for VR effects on conditional log quarterly earnings. The first column of state level estimates, which displays estimates for the quarters prior to the initial service, provides more evidence that selection is endogenous. A majority of the coefficients associated with quarters two or more quarters prior to the initial service are substantial and statistically different from zero. Clients in Maryland with high log quarterly earnings prior to service are more likely to use *diagnosis & evaluation*, *education*, *restoration*, *placement*, and *assistive technology*, and they are less likely to use *training*, *maintenance*, *supported employment*, and *orientation & mobility*.

The second and third columns of the state level estimates in Table 5 display the estimated short- and long-run effects of services on conditional log quarterly earnings. These estimates are measured relative to the coefficients associated with pre-service measures in the first column. For example, prior to service provision, the *log quarterly earnings* for clients provided *training* is -0.267 less than for clients that do not receive these services. In the two years after the start of service provision, it rises to -0.243, and then, in the longer run, it rises more to -0.100. Thus, after accounting for selection into service, the short-term effect of training is $0.267 - 0.243 = 0.024$, and the long-term effect is $0.267 - 0.100 = 0.167$. For Maryland, we observe that *training*, *education*, *supported employment*, and *orientation & mobility* increase *log quarterly earnings* in the long run, while *diagnosis & evaluation*, *restoration*, *maintenance*, *placement*, and *assistive technology* decrease *log quarterly earnings* in the long run. For Oklahoma, we observe that *training*, *education*, and *supported employment* increase *log quarterly earnings* in the long run, while *diagnosis & evaluation*, *restoration*, *maintenance*, *placement*, and *assistive technology* decrease

[23] This is similar to a difference-in-differences estimation methodology.

Table 5: Service Effects on log Conditional Quarterly Earnings

	Maryland			Oklahoma			Virginia		
	Before	SR Effects	LR Effects	Before	SR Effects	LR Effects	Before	SR Effects	LR Effects
Diagnosis & Evaluation	0.159 **	-0.093 **	-0.179 **	0.146 **	-0.272 **	-0.421 **	-0.098 *	-0.159 **	-0.285 **
	(0.035)	(0.033)	(0.054)	(0.028)	(0.036)	(0.061)	(0.055)	(0.072)	(0.112)
Training	-0.267 **	0.024	0.167 **	0.101 *	-0.053	0.024	-0.616 **	-0.214 **	-0.417 **
	(0.046)	(0.045)	(0.085)	(0.063)	(0.055)	(0.087)	(0.080)	(0.106)	(0.127)
Education	0.176 **	-0.001	0.592 **	0.379 **	0.003	0.535 **	-0.357 **	0.181	0.39 **
	(0.076)	(0.101)	(0.122)	(0.046)	-0.049	(0.079)	(0.164)	(0.166)	(0.131)
Restoration	0.241 **		-0.300 **	0.261 **	-0.155 **	-0.316 **	0.260 **	-0.138 **	-0.122
	(0.060)		(0.104)	(0.028)	(0.036)	(0.064)	(0.049)	(0.047)	(0.088)
Maintenance	-0.068	-0.067	-0.298 **	0.097 **	-0.091 **	-0.106	0.740 **	0.055	0.307 **
	(0.046)	(0.044)	(0.078)	(0.030)	(0.038)	(0.066)	(0.047)	(0.068)	(0.103)
Placement	0.040 **	-0.373 **	-0.462 **	0.045	-0.245 **	-0.298 **	-0.077	0.277 **	-0.036
	(0.129)	(0.096)	(0.157)	(0.079)	(0.086)	(0.114)	(0.095)	(0.104)	(0.140)
Supported Employment	-0.065	0.317 **	0.353 **	0.435 **	-0.350 **	0.144	0.501 **	0.108	0.54 *
	(0.112)	(0.086)	(0.113)	(0.128)	(0.173)	(0.276)	(0.184)	(0.174)	(0.299)
Assistive Technology	0.184 **	0.074 *	-0.073	0.153 **	-0.278 **	-0.666 **	-0.585	0.784	1.366 **
	(0.046)	(0.045)	(0.077)	(0.041)	(0.036)	(0.064)	(0.372)	(0.494)	(0.411)
Orientation & Mobility	-0.474 **	-0.085	0.751 **				0.202	-0.587	-0.433
	(0.095)	(0.092)	(0.190)				(0.281)	(0.445)	(0.486)

Note: Items with double stars are statistically significant at the 5% level, and items with single stars are statistically significant at the 10% level.

log quarterly earnings in the long run. For Virginia, we observe that *education*, *maintenance*, *supported employment* and *assistive technology* increase *log quarterly earnings* in the long run, while *diagnosis & evaluation*, *training*, *restoration*, *placement*, and *orientation and mobility* decrease *log quarterly earnings* in the long run.

Overall, for long-run effects, service effects are positive for *education* and *supported employment* in all three states, and service effects are negative for *restoration* in all three states. *Training* has a positive effect in Maryland and Oklahoma, but is negative in Virginia. Interestingly, the signs for Maryland and Oklahoma are mostly consistent with the results reported in DPSSS, while the signs for Virginia differ. This might occur because Maryland and Oklahoma have combined agencies, while Virginia has a blind agency.

Goodness-of-fit graphs for service use, employment, and earnings are available in Appendix A.9. Overall, the estimated model fits the data reasonably well.

Because of the variation in the estimated effects over time and over labor market outcomes, it is difficult to infer the long-run benefits of each service. Accordingly, Figure 3 reports the median present value for 10 years of earnings flows (measured in $1,000). Almost half (42%) of the services have positive long-run benefits with some variation across states. *Diagnosis & evaluation* has negative long-run benefits for all three states. This is consistent with the estimated long-run benefits reported previously in Dean et al. (2015, 2017, 2018, 2019). *Training* has positive benefits in Maryland and Oklahoma and negative benefits in Virginia. *Education* has positive benefits in all three states. *Restoration* has negative benefits in all three states[24] while *placement* has negative benefits in Maryland and Oklahoma and positive benefits in Virginia. *Supported employment* has positive benefits in Maryland and Virginia and negative benefits in Oklahoma. *Assistive technology* has negative benefits in Maryland and Oklahoma and very large positive benefits in Virginia, reflecting the large estimated long-run effect on log quarterly earnings. *Orientation & mobility* has large positive benefits in Maryland and negative benefits in Virginia.[25]

Another notable feature of the discounted benefits calculations is the high degree of variability and skewness across the caseload, especially in Maryland and Oklahoma. The 95% confidence range for discounted benefits associated with *education* in Maryland, for example, range from $260 to nearly $126,500. Similarly, the discounted benefits associated with *supported employment* range from $3,500 to $129,400. One should also note that some services have potentially very large losses; for example, *placement* in Maryland (−$39,700). This variation in benefits occurs because of the nonlinearity of the model described in Section 2. For example, the coefficient capturing the effect of each service on *log quarterly earnings* is a linear effect. But this implies that its effect on quarterly earnings is exponential; i.e., the effect depends on what quarterly earnings would have been without the service.

[24] We were told by Oklahoma VR staff that, around 2007, the Tulsa VR office shared the same building with some other government assistance agencies. Many clients of the other agencies who needed eye glasses were told to stop by the VR office and request them as an important step in going back to work. Many such people received eye glasses (which is part of *restoration*) and then made no more attempt to re-enter the labor force. To some degree, this may help explain the large negative effect of *restoration* on labor market outcomes in Oklahoma. While the Tulsa eye glasses problem says very little about the effectiveness of *restoration* in Oklahoma, it is still part of a “true” effect of those services.

[25] *Orientation & mobility* services are rarely available in Oklahoma as purchased services.

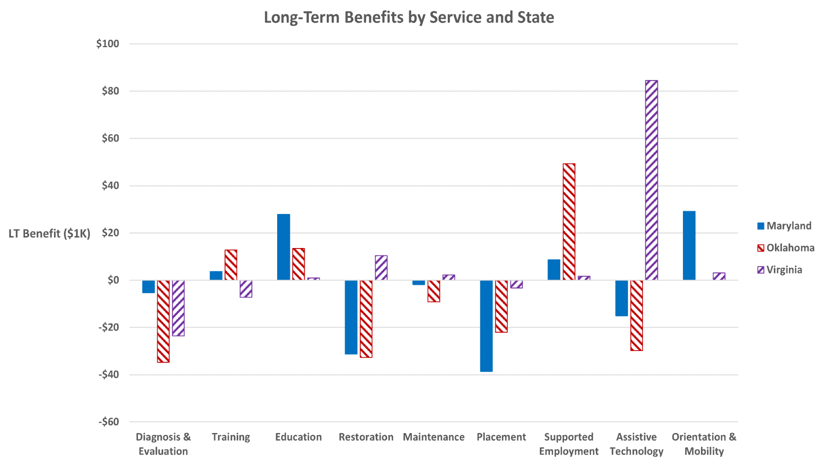

Figure 3: Median Present Discounted Benefits by Service and State

4.2 Estimates of Counselor Effects[26]

The parameters associated with the counselor instruments discussed in Sections 2 and 3.1.3 are estimated to be 0.344** for the counselor effect and 0.077* for the missing counselor effect. The counselor effect should be interpreted as $\partial E y^*_{sij}/\partial \zeta_i$ where y^*_{sij} is the latent variable associated with receipt of service j in equation (1) and ζ_i is the counselor variable defined in Appendix A.3; note that these effects are restricted to be the same across different services. The coefficient estimate associated with the missing counselor variables are the effect on y^*_{sij} when the relevant counselor does not have enough other clients to compute a set of counselor effects. The estimates imply that the counselor effects have a large and statistically significant effect on service receipt. There is an adjustment allowed for Oklahoma because the instruments were constructed in a different way. The estimate is 0.069* which means that $\partial E y^*_{sij}/\partial \zeta_i$ in Oklahoma is estimated to be $0.344+0.069 = 0.413^{**}$.

4.3 Estimates of the Covariance Structure

Our model has a rich error covariance structure, as seen in equation (4). This allows for the possibility that unobservables associated with service provision are correlated with unobservables associated with labor market outcomes. The factor loadings for *Factor 1* in Table 6 for labor market outcomes have different signs, suggesting that they are accounting for something like the unobserved component of willingness to work. The service factor loadings are all negative (the same sign as the factor loading on labor market employment); i.e.; those people who have a strong desire to work have less interest in utilizing services. This may explain part of the negative results reported in Tables 4 and 5. The labor market factor loadings for *Factor 2* have the same sign, suggesting that they are accounting for unobserved ability (which would increase *employment propensity* and *log quarterly earnings*). None of the factor loadings for *Factor 2* are statistically significant,[27] suggesting that unobserved ability and unobserved desire to use services are uncorrelated. The estimates of the other elements of the error structure are reported in Appendix A.1. All of the estimated covariance terms are relatively large and suggest significant amounts of randomness and serial correlation in labor market outcomes. The estimate of the log earnings error σ_w is quite large, implying that a standard deviation in quarterly earnings due to unobserved factors is $8,681 in Maryland, $8,208 in Oklahoma, and $10,279 in Virginia.[28] It is unclear how much of this variation is due to variation in wages and how much is due to variation in hours.

[26] Appendix A.4 presents and evaluates the estimates of the the demographic characteristics on the propensity to use different services (y^*_{sij} in equation (1)) and the effects of demographic, socioeconomic, and disability-related characteristics on the two labor market outcomes of interest (z^*_{sit} in equation (2) and w_{sit} in equation (3). For the most part, the observed characteristics do not have statistically significant effects on service receipt, while almost all of the coefficient estimates in the labor market equations are statistically significant with the expected signs.

[27] The factor loading in *Factor 2* for *orientation & mobility* is restricted to equal 0 because of an identification requirement.

[28] We use the weighted average log quarterly earnings after service receipt from Table 5 to get an estimate of the lognormal mean parameter μ. Then, we plug μ and σ^2_w into

$$StdDev = \left[\exp\left\{\sigma^2_w - 1\right\}\exp\left\{2\mu + \sigma^2_w\right\}\right]^{1/2}.$$

Table 6: Factor Loadings

	Factor 1		Factor 2	
	Estimate	Std Err	Estimate	Std Err
Services				
Diagnosis & Evaluation	-0.362 **	0.087	-0.031	0.074
Training	-0.409 **	0.127	0.021	0.106
Education	-0.712 **	0.156	0.236 *	0.130
Restoration	-0.447 **	0.100	-0.016	0.082
Maintenance	-0.662 **	0.100	-0.062	0.081
Placement	-0.441 **	0.173	0.090	0.166
Supported Employment	-0.402 *	0.241	0.110	0.255
Assistive Technology	-0.457 **	0.117	-0.159 *	0.099
Orientation & Mobility	-0.795 **	0.269		
Labor Market				
Employment	-0.135 **	0.007	0.694 **	0.005
log Conditional Quarterly Earnings	1.658 **	0.011	1.011 **	0.007

Note: Double-starred items are statistically significant at the 5% level, and single-starred items are statistically significant at the 10% level.

5 Net Present Value

In this section, we examine the social welfare implications of VR services by comparing the estimated benefits and costs of the program. In particular, we compute the net present value (NPV)[29] for each person receiving services. For each sample individual receiving some service, we use the estimated model to compare the expected flow of benefits they would get with the service package they received relative to the flow of benefits they would get with no services. As shown in Figure 3, many services have negative mean long-run effects on labor market outcomes.

5.1 Marginal and Fixed Costs

Using data from the state agency, we approximate cost as

$$C_{si} = f + \sum_{j=1}^{J} y_{ij} c_j$$

where f is fixed costs, y_{ij} is an indicator for receipt of service j by person i (as defined in equation (1)), and c_j is the average cost associated with service j computed as the ratio of "mean expenditure" and "% with positive expenditure." As shown in Table 7, there is significant variation in marginal costs across both services and states.[30] These marginal costs are significantly higher for people who have vision impairments than those who have other impairments.

Using the approach in Schmidt et al. (2019), we estimate overhead costs f based on each state's spending by fiscal year as reported to the US Social Security Administration. Fixed costs per client are estimated to be $\$4,160$ in Maryland, $\$4,634$ in Oklahoma, and $\$19,686$ in Virginia.[31] These fixed costs estimates are approximately 1.3 times as high for vision-impaired VR clients than other VR clients in Maryland and Oklahoma and are approximately 3.2 times as high as in the general agency in Virginia.[32] In general, Virginia has the highest costs; this might occur because, unlike the combined Maryland and Oklahoma agencies, the Virginia agency is a blind agency. It also might be that the estimate of fixed cost, especially in Virginia, is biased upwards because we do not allow for agency-provided services. The cost of such services are included in fixed cost when they really should be in the marginal cost in agency-provided services.

[29] The NPV is $-C_{si} + \sum_t \beta^t B_{sit}$ for some chosen discount factor β where C is the total cost and B_t is the benefits in quarter t. DPSSS compute rates of return instead of NPV. It is not useful to compute the rate of return here because all NPVs are negative.

[30] These mean cost estimates have not been discounted, and thus will be inflated to the extent the purchased services are provided over long periods.

[31] An alternative method described in Dean et al. (2015, 2017) gives us similar estimates.

[32] We do not compute separate estimates based on client-specific information on purchased services and spell length. We choose to use only an average fixed cost because the model and estimation procedure used to infer benefits allows neither service duration nor actual expenditures to affect labor market outcomes.

Table 7: Mean State- and Service-Specific Marginal Costs of Purchased Services

Service	Maryland	Oklahoma	Virginia
Diagnosis & Evaluation	\$474	\$385	\$437
Training	\$2,086	\$2,767	\$2,761
Education	\$3,745	\$4,590	\$7,499
Restoration	\$1,199	\$2,848	\$2,078
Maintenance	\$1,284	\$1,423	\$4,062
Placement	\$999	\$1,185	\$4,252
Supported Employment	\$1,935	\$4,793	\$2,740
Assistive Technology	\$3,286	\$3,222	\$2,189
Mobility & Orientation	\$2,678	\$658	\$4,465

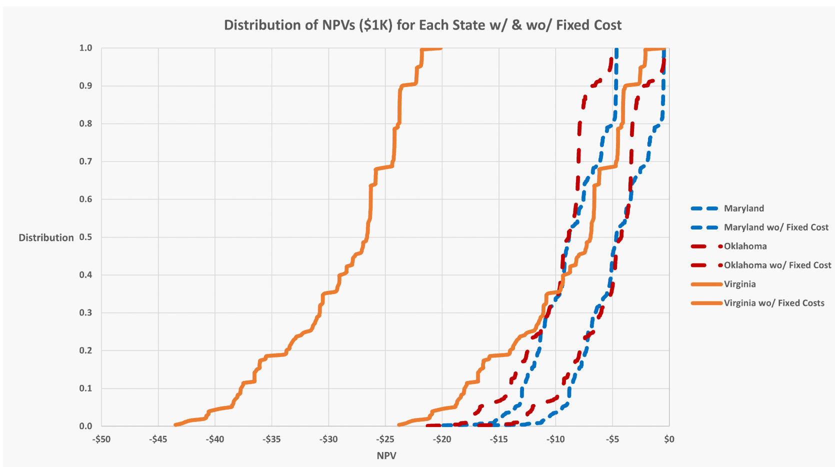

Figure 4: Distribution of NPVs (\$1K) for Each State w/ & wo/ Fixed Cost

5.2 Estimated NPV

We simulate the distribution of NPVs for VR consumers using the structural model estimates summarized in Section 4 with and without the inclusion of fixed costs.[33] For each state, Figure 4 displays the curve including fixed costs (to the left) and the curve excluding fixed costs (to the right). We compute the median NPV of the provided services relative to the NPV of receiving no services using 20-year post-treatment observation quarters for those individuals who received some service[34] and using an annual discount factor of 0.974 (Office of Management and Budget, 1992). These are shown in Figure 4. For example, in Virginia, when including fixed costs, the proportion of VR clients with a NPV less than $-\$30,000$ is 35% (because the Virginia curve passes through the point $(-30, 0.352)$. In Maryland, the estimated median NPV is $-\$8,820$ with a 90% confidence range of $(-\$13,570, -\$4,660)$. In Oklahoma, the estimated median NPV is $-\$8,870$ with a 90% confidence range of $(-\$16,660, -\$5,180)$. In Virginia, the estimated median NPV is $-\$26,610$ with a with a 90% confidence range of $(-\$38,540, -\$22,210)$. When we exclude fixed costs, the distribution moves far to the right for Virginia and a more modest amount for Maryland and Oklahoma.

The dominant fact implied by this figure is that, in each of the three states, all clients have negative NPVs whether

[33] This simulation has a similar structure to the one used to compute marginal effects reported in Section 4.1. But here we compute the present discounted value of the actual treatments provided by each VR agency rather than a conjectured treatment for single service j. Formally, we first compute the short- and long-run effect of the program for each individual:

$$\Delta_i = v_{ik}(y_{si}) - v_{ik}(0)$$

where $v_{ik}(y_i)$ is the estimated labor market earnings under the realized services y_i and $v_{ik}(0)$ is the estimated earnings that would be observed if no services were provided.

[34] This restriction reduces the total sample size from $1,778$ to $1,374$.

Table 8: Comparison of Actual and Best Choices

	Maryland		Oklahoma		Virginia	
	Actual	Best	Actual	Best	Actual	Best
Diagnosis & Evaluation	0.726	0.000	0.678	0.000	0.557	0.000
Training	0.383	0.562	0.165	0.000	0.375	0.000
Education	0.204	0.562	0.136	0.000	0.166	0.435
Restoration	0.249	0.000	0.647	0.000	0.565	0.000
Maintenance	0.624	0.000	0.458	0.000	0.858	0.435
Placement	0.204	0.000	0.062	0.000	0.213	0.000
Supported Employment	0.084	0.562	0.036	0.000	0.095	0.435
Assistive Technology	0.524	0.000	0.260	0.000	0.126	0.435
Orientation & Mobility	0.070	0.562	0.038	0.000	0.111	0.000

Note: Shaded items are used to maximize NPV.

fixed costs are included or not. These negative results occur because, a) as seen in Figure 3, many services that are used frequently have negative mean benefits; b) even for many of those services with positive benefits, the marginal cost of the service dominates the median benefit; and c) fixed costs are quite large, especially in Virginia. Virginia's DBVI performs worse than the combined agencies in Maryland and Oklahoma. This is true because of the higher fixed and marginal costs of the Virginia blind agency. This is in contrast to Cavenaugh, Giesen, and Pierce (2000), Warren-Peace (2009), and Cavenaugh (2010) who find that blind agencies perform better than combined agencies for people who have impaired vision.

There has been a significant decline in the number of separate agencies,[35] going from 27 in 1982 (U.S. Dept of Education, 1982) to 25 in 2010 (Cavenaugh, 2010) to 24 in 2021 (RSA, 2021). Cavenaugh (1999) says that blind consumers prefer separate agencies, while those with other types of disabilities prefer a combined agency. Cavenaugh, Giesen, and Steinman (2006) find that the mean earnings at closure of legally blind consumers were significantly higher in separate agencies than in combined agencies. Warren-Peace (2009) finds similar results, while Capella (2001) finds that the type of agency has no effect on later earnings. Cavenaugh (1999) find mixed results with respect to labor market earnings, but blind agencies do better than combined agencies with respect to employment.

Finally, one important implication of these results is that consumers are not maximizing their NPVs when choosing services. Table 8 shows how actual and NPV-maximizing choices differ. For example, the percentage of consumers in Maryland who use training is 28.2%, but NPV would be maximized if it had been used by 56.2%. Overall, the results in Table 8 show that, while the mix of service choices is quite broad, the NPV-maximizing choices are limited only to the services that have positive returns.[36] In Oklahoma, the best that can be done is to not use any service leading to a NPV equal to the negative of the fixed cost. For the Maryland and Virginia, there are many cases of consumers who could have reached a positive NPV with a different set of service choices.

5.3 Discussion: Possible Reasons for Negative Returns

It is not obvious how to explain the large number of negative estimated service effects and the resulting negative NPV estimates. This implies that receipt of the service somehow hurts the client's labor market skills. Certainly, we are not recommending that Oklahoma's VR agency be shut down or that all VR consumers in Maryland be forced to use only the four services shaded in Table 8 (training, education, supported employment, and orientation & mobility). Rather, we would recommend that consumers use this information along with other factors to remain informed and active participants in the selection of appropriate services. VR agencies should use it to plan for improving those services that might be lagging in estimated quality.

There two general explanations for these negative effects. First, the model may be misspecified in ways that lead to negative biases in the estimated rates of return or upward biases in the fixed costs estimates. Second, even if there are negative labor market effects, there may be other important and substantial benefits of VR that are not included in our analysis.

In terms of the model, the basic structure and identification assumptions seem credible. In fact, using this model and

[35] Cavenaugh (1999) cites Hopkins (1991) to state that, at one point, 45 states had separate blind agencies. Unfortunately, Hopkins (1991) no longer seems to exist.

[36] The maximizing choices are less than 100% used by VR consumers. For example, for Maryland, the four shaded services should be used together by 56.2%. The variation in the best choice occurs because of variation in explanatory variables and random variation in errors.

similar data, DPSSS find that VR has a positive median quarterly rate for return for clients with physical impairments (35.1%), cognitive impairments (7.7%), and mental illness (3.7%).[37] Still, there are unique features of the VR program for clients with vision impairments that may not be captured in the model. Examples include: a) Some people apply for VR services to receive something of value such as eyeglasses (see the Tulsa discussion in Section 4.1) with no intention of participating in the labor market. This might explain the negative results for restoration (the service that provides eye glasses). b) Some clients are receiving services and not working at the beginning of the long run. In our data, about 35% of people are still receiving services at the end of 10 quarters. c) Some services may give the client information that she will not be successful in the labor market. She might act on that information and choose to reduce work or not to work. Dean et al. (2017) proposed similar arguments with respect to the effect of the receipt of diagnosis & evaluation on later receipt of SSDI payments. d) Finally, unlike prior work in DPSSS, we include clients with prior spells of VR service receipt. Dean et al. (2015) shows that the bias from inclusion of such observations is significant; but some biases are positive, and some are negative.

In addition to possible biases, our analysis focuses exclusively on labor market effects of VR and does not include possible nonpecuniary benefits including, for example, independent living skills. The deviation between actual and NPV-maximizing service choices is reminiscent of the discussion in the literature on estimates of the Roy model where non-pecuniary characteristics of jobs can have large effects on the job choices that people make (Berkovic and Stern, 1991; Keane and Wolpin, 1997; Taber and Vejlin, 2020). The Roy model analogue in our work is that VR service consumers have other important unobserved factors that affect what service choices they make. These might include which VR services have reputations for being difficult or unpleasant (or fun and easy), which are close to where the person lives (Ipsen, Jain, and Stern, 2023), which provide non-labor market benefits such as independent living skills (e.g., glasses in Tulsa), or which will lead to jobs with better non-pecuniary characteristics. At least to some degree, the deviation between actual and NPV-maximizing choices is caused by these other unobserved factors. VR clients with vision impairment may be more interested in improving independent living skills even if it does not lead to participation in the labor market. Warren-Peace (2009) reports that VR clients who are legally blind have much higher rates of non-competitive closures from VR (29.5%) than VR clients with any other disabilities (1.5%). People who close non-competitively never meant to get a job.[38]

Acquiring independent livings skills is especially important for those with vision impairments. The American Council of the Blind (2020) states,

> "What is indisputable is that people who have recently experienced the loss of most or all of their vision need to learn adaptive skills different in kind from other disability groups. Certified professionals are necessary to deliver categorical services to blind or low vision clients, covered by insurances, regardless of age or vocational outcome for the purpose of returning them to independence and a participatory, contributing lifestyle in society."

There is a large body of literature supporting this statement. For example, Soong, Lovie-Kitchin, and Brown (2001), Brody et al. (2002), Eklund et al. (2005), Brody et al. (2006), Zijlstra et al. (2009), Christy et al. (2010), Girdler et al. (2010), Ballemans et al. (2012), Ehrlich et al. (2017), and van Nispen et al. (2020) all discuss the benefits of low vision-centered vocational rehabilitation in terms of improved independent living skills, especially for those in old age. Eklund et al. (2005) measure benefits in terms of reductions in home health expenditures and similar costs. Fleming, Fairweather, and Leahy (2013) provide evidence that for people with disabilities (not limited to vision impairment) non-labor market outcomes have large effects on quality of life (QOL).[39] Binns et al. (2012) conclude that rehabilitation services for people who are vision-impaired result in improved clinical and functional ability outcomes, but the effects on mood, vision-related quality of life (QOL) and health-related QOL are less clear. Notably, labor market outcomes are not even considered. Frick et al. (2007) find that people with blindness have very high formal and informal care costs (on average, about $5,500/year). To the degree that VR services can help make people with vision impairment have better independent living skills, they can reduce the costs of such care. Köberlein et al. (2013) measure home care and caregiving costs for people with vision impairments and find large costs which improvements in independent living skills might mitigate. Crews and Campbell (2004) identify ADL and IADL problems affected by vision impairment many of which can be mitigated with VR services.

We have no way to measure the improvement in independent living skills because our only source of information about outcomes is strictly limited to quarterly earnings data. Other work has made progress in measuring the value of such skills (see, for example, Dolan, 1997; Brown et al., 2001; U.S. AHRQ, 2002; Eklund et al., 2005; Frick et al., 2010; Fleming,

[37] Although the models are similar, there are other differences besides our focus on clients with vision impairments. These include a) the use of more states, b) the use of the 2007 cohort instead of the 2000, and c) the longer time horizon.

[38] There is some belief that people who close non-competitively have disability problems that make it too difficult to get a job. Also, some non-competitive closures correspond to getting a job in a sheltered workshop.

[39] Quality of Life is measured using a scale developed by the World Health Organization, called ICF (World Health Organization, 2002).

Fairweather, and Leahy, 2013). We expect that improvement in independent living skills is a primary goal of VR clients with vision impairment and that the return to such improvement is large.

6 Conclusions

Over the last twenty years, there have been a number of state-level return on investment evaluations of VR services produced by economic consulting firms or university research bureaus (e.g., Heminway and Rohani, 1999; Uvin, Karaaslani, and White, 2004; Hollenbeck and Huang, 2006; Kisker et al., 2008; Wilhelm and Robinson, 2010; Austin and Lee, 2014; and Mathematica, 2017). None of these focus on people with vision impairments. Also, these reports have a number of serious shortcomings which are addressed in this paper. First, using the model described in Section 2, we formally account for the possibility that selection into the treatment is endogenous. Second, by focusing on clients with vision impairment, we allow the estimated effects of treatment to vary with the clients' limiting conditions. In contrast, these state-level reports do not distinguish between clients with mental illness, cognitive impairments, sensory impairments, or physical impairments. The effects of the programs are heterogeneous, and restricting the impact to be constant across all disability groups leads to biased estimates (DPSSS). Third, unlike the evaluations prior to DPSSS, we examine the impact of specific types of services rather than just a single treatment indicator. We find that services have very different impacts on labor market effects. Finally, we observe labor market outcomes 2 years before and 5 years after application for VR services. In this analysis, being able to estimate the long-run return is critical as it significantly differs from the short-run return.

For evaluation of VR services, there is a critical trade-off between estimation of long-run labor market effects and estimation of effects quickly. Almost by definition, it is impossible to estimate long-run labor market effects quickly because the researcher has to wait to start his analysis until enough time has passed to observe long-run effects. For example, for this paper, we used VR service data from 2007 which meant we had to wait until 2015 to start analysis. By 2015, the VR world changed dramatically with the passage of the Workforce Innovation and Opportunity Act (WIOA) in 2014, thus reducing the value of estimates from years prior to its passage. Even today, with no more dramatic changes, there is much emphasis placed on fast estimates relative to long-run estimates. Both have value. Short-run, fast estimates are more valuable to agencies addressing issues they face today. On the other hand, long-run estimates are what the VR community should care about when evaluating the effectiveness of programs. A VR program whose benefits depreciated after two years would not be a good program. Only long-run estimates can provide information about the long-run labor market effects of VR programs.

Our results suggest a complex picture of the impact of VR services on labor market outcomes. Pre-program labor market differences vary across the nine service types and across states. Short-run and long-run effects vary. Overall, we find that VR services have small and frequently negative average returns. We also find much variation in the return across VR participants.

7 Appendices

Appendix A.1: Covariance Structure

The covariance matrix of the errors $u_i' = (u_{i1}^y, u_{i2}^y, .., u_{iJ}^y, u_{i1}^z, u_{i1}^w, .., u_{iT}^z, u_{iT}^w)$ implied by the structure in equation (4) of the paper is

$$\underset{(J+2T)\times(J+2T)}{\Omega} = \begin{pmatrix} \underset{J\times J}{A} & \underset{J\times 2T}{B'} \\ \underset{2T\times J}{B} & \underset{2T\times 2T}{C} + \underset{2T\times 2T}{D} \end{pmatrix}$$

where

$$A = \begin{pmatrix} \sum_k (\lambda_{1k}^y)^2 & \sum_k \lambda_{1k}^y \lambda_{2k}^y & \cdots & \sum_k \lambda_{1k}^y \lambda_{Jk}^y \\ \sum_k \lambda_{1k}^y \lambda_{2k}^y & \sum_k (\lambda_{2k}^y)^2 & \cdots & \sum_k \lambda_{2k}^y \lambda_{Jk}^y \\ \vdots & \vdots & \ddots & \vdots \\ \sum_k \lambda_{1k}^y \lambda_{Jk}^y & \sum_k \lambda_{2k}^y \lambda_{Jk}^y & \cdots & \sum_k (\lambda_{Jk}^y)^2 \end{pmatrix},$$

Table A.1: Other Covariance Terms

	Estimate		Std Err
Labor Market Time Series St. Dev.	0.160	**	0.001
Labor Market Time Series Correlation	0.772	**	0.004
Idiosyncratic log Conditional Earnings St. Dev.	0.883	**	0.003

Note: Double-starred items are statistically significant at the 5% level.

$$
\begin{aligned}
C &= H \otimes Q_T \\
\underset{2\times 2}{H} &= \begin{pmatrix} \sum_k (\lambda_k^z)^2 & \sum_k \lambda_k^z \lambda_k^w \\ \sum_k \lambda_k^z \lambda_k^w & \sum_k (\lambda_k^w)^2 \end{pmatrix}, \\
\underset{T\times T}{Q_T} &= \begin{pmatrix} 1 & 1 & \cdots & 1 \\ 1 & 1 & \cdots & 1 \\ \vdots & \vdots & \ddots & \vdots \\ 1 & 1 & \cdots & 1 \end{pmatrix}
\end{aligned}
$$

$$
D = \underset{2\times 2}{\Omega_\zeta} \otimes \frac{1}{1-\rho_\eta^2} \begin{pmatrix} 1 & \rho_\eta & \cdots & \rho_\eta^{T-1} \\ \rho_\eta & 1 & \cdots & \rho_\eta^{T-2} \\ \vdots & \vdots & \ddots & \vdots \\ \rho_\eta^{T-1} & \rho_\eta^{T-2} & \cdots & 1 \end{pmatrix},
$$

and

$$
\begin{aligned}
B &= q_T \otimes F, \\
\underset{T\times 1}{q_T} &= \begin{pmatrix} 1 \\ 1 \\ \vdots \\ 1 \end{pmatrix}, \\
\underset{2\times J}{F} &= \begin{pmatrix} \sum_k \lambda_{1k}^y \lambda_{1k}^z & \sum_k \lambda_{2k}^y \lambda_{1k}^z & \cdots & \sum_k \lambda_{Jk}^y \lambda_{1k}^z \\ \sum_k \lambda_{1k}^y \lambda_{1k}^w & \sum_k \lambda_{2k}^y \lambda_{1k}^w & \cdots & \sum_k \lambda_{Jk}^y \lambda_{1k}^w \end{pmatrix}.
\end{aligned}
$$

The estimates of the primitives, other than the factor loading estimates in Table 8, associated with the covariance matrix are reported in Table A.1.

Appendix A.2. Likelihood Function

The parameters of the model are $\theta = (\theta_y, \theta_z, \theta_w, \Omega_\zeta)$ where

$$
\begin{aligned}
\theta_y &= \left(\beta_j, \lambda_{j1}^y, \lambda_{j2}^y\right)_{j=1}^J, \\
\theta_z &= \left(\gamma, \lambda_1^z, \lambda_2^z, \rho_\eta^z, \left[\left(\alpha_{sjk}^z\right)_{s=1}^3\right]_{j=1}^J\right), \text{ and} \\
\theta_w &= \left(\delta, \lambda_1^w, \lambda_2^w, \rho_\eta^w, \sigma_w^2, \left[\left(\alpha_{sjk}^w\right)_{s=1}^3\right]_{j=1}^J\right).
\end{aligned}
$$

We estimate the parameters of the model using maximum simulated likelihood (MSL). The likelihood contribution for observation i from state s is

$$
L_{si} = \int L_{si}(u_{si})\, dG(u_{si} \mid \Omega) \tag{5}
$$

where

$$
L_{si}(u_{si}) = L_{si}^y(u_{si}^y) \prod_{t=1}^T L_{sit}^{zw}(u_{sit}^z, u_{sit}^w),
$$

$$L_{si}^{y}\left(u_{si}^{y}\right)=\prod_{j=1}^{J}\frac{\exp\left\{X_{si}^{y}\beta_{j}+u_{sij}^{y}\right\}}{1+\exp\left\{X_{si}^{y}\beta_{j}+u_{sij}^{y}\right\}}, \tag{6}$$

$$L_{sit}^{zw}\left(u_{sit}^{z},u_{sit}^{w}\right)=\left[L_{sit}^{0}\left(u_{sit}^{z},u_{sit}^{w}\right)\right]^{1-z_{sit}}\left[L_{sit}^{1}\left(u_{sit}^{z},u_{sit}^{w}\right)\right]^{z_{sit}}, \tag{7}$$

$$L_{sit}^{0}\left(u_{sit}^{z},u_{sit}^{w}\right)=1-\Phi\left(\gamma_{s0}+X_{sit}^{z}\gamma+\sum_{k=1}^{K}\sum_{j=1}^{J}\alpha_{sjk}^{z}d_{sik}y_{sij}+u_{sijt}^{z}\right), \tag{8}$$

$$\begin{aligned} L_{sit}^{1}\left(u_{sit}^{z},u_{sit}^{w}\right) &= \frac{1}{\sigma_w}\phi\left(\frac{w_{sit}-\delta_{s0}-X_{sit}^{w}\delta-\sum_{k=1}^{K}\sum_{j=1}^{J}\alpha_{sjkl}^{w}d_{sik}y_{sij}-u_{sit}^{w}}{\sigma_w}\right)\cdot \\ & \quad \Phi\left(\gamma_{s0}+X_{sit}^{z}\gamma+\sum_{k=1}^{K}\sum_{j=1}^{J}\alpha_{sjk}^{z}d_{sik}y_{sij}+u_{sijt}^{z}\right). \end{aligned} \tag{9}$$

and $G\left(u_{si}\mid\Omega\right)$ is the joint normal density with covariance matrix Ω described in Appendix A.1. It is straightforward to simulate the integral in (5) using well-known methods described in Stern (1997).[40] The functional form of the conditional likelihood contribution associated with observed program choices, $L_{si}^{y}\left(u_{si}^{y}\right)$ in equation (6), follows from the assumption in equation (1) that the idiosyncratic errors are iidEV. The functional form of the conditional likelihood contribution for labor market outcomes, $L_{sit}^{zw}\left(u_{sit}^{z},u_{sit}^{w}\right)$ in equations (7), (8), and (9) follow from the normality assumption for $\left(\upsilon_{sit}^{z},\upsilon_{sit}^{w}\right)$ and the bivariate normality assumption for $\left(\zeta_{sit}^{z},\zeta_{sit}^{w}\right)$ in equation (4). The log likelihood function is $\log L=\sum_{s=1}^{3}\sum_{i=1}^{n}\log L_{si}$.

Appendix A.3: Local Labor Market Variables

U.S. Census Bureau (2015) provides 2006-10 commuting flows between United States counties. These can be used to determine the number of people commuting out-of-state during this five-year period. We then divide these numbers by the employed population in 2006 to create the proportion of employed people in each county who work in other states (and therefore are not reported in UI data). This proportion is used for all quarters.

We also can use data from Eye on Washington (2014) to get information on the number of federal employees for each state for the years 2002, 2006, 2008, 2010, and 2012. We then divide these numbers by the employed population in 2002 to create the proportion of employed people in each county who work for the federal government in those years. We use both interpolation and extrapolation for the years not covered by the source.

The last step in the construction of each variable is to translate the proportions into units similar to variables on the right-hand side of equations (2) and (3). Let ζ_{ti} be proportion of population employed in county i and ζ_{ei} be proportion of population employed in county i excluding federal workers. Define

$$\begin{aligned} \zeta_{ti} &= \int\Phi\left(X_{ij}\psi_{ti}\right)dF_i\left(X_{ij}\right); \\ \zeta_{ei} &= \int\Phi\left(X_{ij}\psi_{e}\right)dF_i\left(X_{ij}\right). \end{aligned}$$

Define p_{tij} as the probability of j from county i being employed and p_{eij} be the probability that j is employed in a non-federal job. Assume that ψ_{ti} differs from ψ_e only in the constant. Then the adjustment to p_{tij} when only p_{eij} can be observed (estimated) is

$$p_{tij}=\Phi\left(\psi_{t0i}+X_{ij}\psi\right)$$

where ψ_{t0i} is the solution to

$$\int\left[\Phi\left(\psi_{t0i}+X_{ij}\psi\right)-\Phi\left(\psi_{e0}+X_{ij}\psi\right)\right]dF_i\left(X_{ij}\right)=\zeta_{ti}-\zeta_{ei}.$$

[40] In theory, the parameter estimates are consistent only as the number of independent draws used to simulate the likelihood contributions goes off to infinity. However, Börsch-Supan and Hajivassiliou (1992) show that MSL estimates perform well for small and moderate numbers of draws as long as good simulation methods are used, and Geweke (1988) shows that the simulation error occurring in simulation-based estimators for a significant class of models is of order $(1/n)$ when antithetic acceleration is used. We simulate all errors except for η and ε with antithetic acceleration (Geweke, 1988) and then compute likelihood contributions conditional on the simulated errors. This is similar to simulation methods described in Stern (1992) and McFadden and Train (2000).

If we take a second-order Taylor series approximation around the mean value of X_i in county i's general population, then we get

$$\begin{aligned}
\zeta_{ti}-\zeta_{ei} &= \int \Big[\Phi\left(\psi_{t0i}+\overline{X}_i\psi\right)-\Phi\left(\psi_{e0}+\overline{X}_i\psi\right)+\left(\phi\left(\psi_{t0i}+\overline{X}_i\psi\right)-\phi\left(\psi_{e0}+\overline{X}_i\psi\right)\right)\left(X_{ij}-\overline{X}_i\right)\psi \\
&\quad +.5\left(\phi'\left(\psi_{t0i}+\overline{X}_i\psi\right)-\phi'\left(\psi_{e0}+\overline{X}_i\psi\right)\right)\left(X_{ij}-\overline{X}_i\right)\psi\psi'\left(X_{ij}-\overline{X}_i\right)'\Big]dF_i\left(X_{ij}\right) \\
&= \Phi\left(\psi_{t0i}+\overline{X}_i\psi\right)-\Phi\left(\psi_{e0}+\overline{X}_i\psi\right)+\left(\phi\left(\psi_{t0i}+\overline{X}_i\psi\right)-\phi\left(\psi_{e0}+\overline{X}_i\psi\right)\right)\int\left(X_{ij}-\overline{X}_i\right)\psi dF_i\left(X_{ij}\right) \\
&\quad +.5\left(\phi'\left(\psi_{t0i}+\overline{X}_i\psi\right)-\phi'\left(\psi_{e0}+\overline{X}_i\psi\right)\right)\int\left(X_{ij}-\overline{X}_i\right)\psi\psi'\left(X_{ij}-\overline{X}_i\right)'dF_i\left(X_{ij}\right) \\
&= \Phi\left(\psi_{t0i}+\overline{X}_i\psi\right)-\Phi\left(\psi_{e0}+\overline{X}_i\psi\right) \qquad (10) \\
&\quad -.5\left[\left(\psi_{e0}+\overline{X}_i\psi\right)\phi\left(\psi_{e0}+\overline{X}_i\psi\right)-\left(\psi_{t0i}+\overline{X}_i\psi\right)\phi\left(\psi_{t0i}+\overline{X}_i\psi\right)\right]\int\left(X_{ij}-\overline{X}_i\right)\psi\psi'\left(X_{ij}-\overline{X}_i\right)'dF_i\left(X_{ij}\right)
\end{aligned}$$

The first-order term disappears because

$$\int\left(X_{ij}-\overline{X}_i\right)\psi dF_i\left(X_{ij}\right)=0.$$

The second-order term is the effect due to the curvature of Φ. If we ignore it, we get

$$\zeta_{ti}-\zeta_{ei}=\Phi\left(\psi_{t0i}+\overline{X}_i\psi\right)-\Phi\left(\psi_{e0}+\overline{X}_i\psi\right);$$

$$\psi_{t0i}=\Phi^{-1}\left[\left(\zeta_{ti}-\zeta_{ei}\right)+\Phi\left(\psi_{e0}+\overline{X}_i\psi\right)\right]-\overline{X}_i\psi$$

We can take a first-order Taylor series approximation with respect to $(\zeta_{ti}-\zeta_{ei})$ around 0 to get

$$=\Phi^{-1}\left[\Phi\left(\psi_{e0}+\overline{X}_i\psi\right)\right]+\left(\zeta_{ti}-\zeta_{ei}\right)\frac{\partial}{\partial\left(\zeta_{ti}-\zeta_{ei}\right)}\Phi^{-1}\left[\left(\zeta_{ti}-\zeta_{ei}\right)+\Phi\left(\psi_{e0}+\overline{X}_i\psi\right)\right]|_{(\zeta_{ti}-\zeta_{ei})=0}-\overline{X}_i\psi.$$

Note that, in general,

$$y=f(x)\Rightarrow x=f^{-1}(y)\Rightarrow\frac{\partial x}{\partial y}=\frac{\partial f^{-1}(y)}{\partial y},\frac{\partial y}{\partial x}=\frac{\partial f(x)}{\partial x}\Rightarrow\frac{\partial x}{\partial y}=\left(\frac{\partial f(x)}{\partial x}\right)^{-1}.$$

Thus,

$$\begin{aligned}
\psi_{t0i}-\psi_{e0} &= \overline{X}_i\psi+\left(\zeta_{ti}-\zeta_{ei}\right)\left(\frac{\partial}{\partial\left(\zeta_{ti}-\zeta_{ei}\right)}\Phi\left[\left(\zeta_{ti}-\zeta_{ei}\right)+\Phi\left(\psi_{e0}+\overline{X}_i\psi\right)\right]\right)^{-1}|_{(\zeta_{ti}-\zeta_{ei})=0}-\overline{X}_i\psi \\
&= \left(\zeta_{ti}-\zeta_{ei}\right)\phi\left(\Phi^{-1}\left[\left(\zeta_{ti}-\zeta_{ei}\right)+\Phi\left(\psi_{e0}+\overline{X}_i\psi\right)\right]\right)|_{(\zeta_{ti}-\zeta_{ei})=0} \\
&= \left(\zeta_{ti}-\zeta_{ei}\right)\phi\left(\psi_{e0}+\overline{X}_i\psi\right) \\
&= \left(\zeta_{ti}-\zeta_{ei}\right)\phi\left[\Phi^{-1}\left(p_{ei}\right)\right].
\end{aligned}$$

The same analysis applies for adjustments for commuting out of state.

Appendix A.4: Counselor Effects in Oklahoma

In Maryland and Virginia, following DPSSS, we use as an instrument in equation (1) of the paper, a transformation of the proportion of other clients of the same counselor provided service j, i.e., a counselor effect. We are able to do so because in these states, individuals go to the closest field office, then, conditional on observables, they are randomly (or at least independently of services and labor market outcomes) assigned to a counselor. In contrast, in Oklahoma, the process is different. Neither field offices nor counselors are related to geography. The field office measure we observe in the data is not where counselors physically sat in 2007, and neither we nor the Oklahoma VR staff can determine the physical offices where caseworkers were located. Since field offices are not a measure of geography, they are not exogenous, so the counselor an individual is assigned to is not exogenous either.

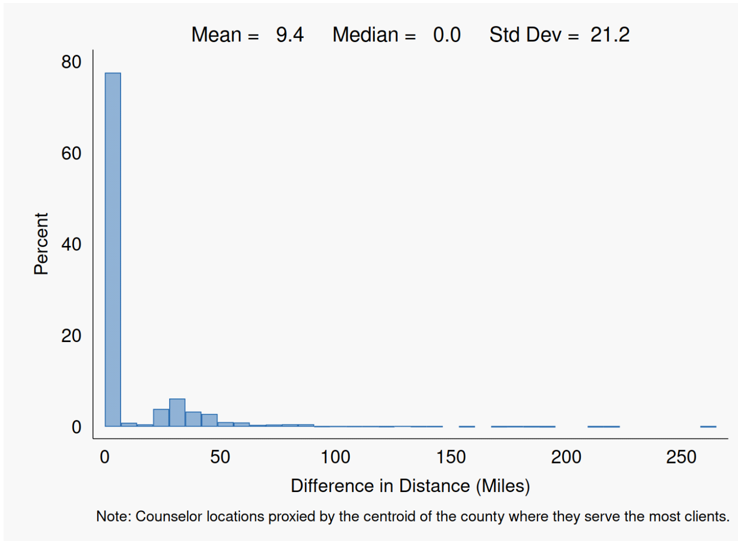

Figure 5: Difference in Distance (Miles)

To see why this is the case, note that we explore constructing a proxy for the counselor's location using the centroid of the county where the counselor serves the most clients, then calculating the instruments as we do in Maryland and Virginia. To assess the viability of this approach, we use Geographic Information System (GIS) techniques to calculate the straight-line distance between the counties where clients in Oklahoma reside and these proxy counselor locations. Figure 5 indicates that the location proxy appears to work well. Most clients for a given counselor come from a cluster of proximate counties, and almost all clients are within about 100 miles of the proxy counselor location our algorithm assigns to them (and most are much closer).

An instrument constructed from this proxy location is valid as long as the randomness associated with counselor choice is independent of unobserved characteristics of either the client or the environment that affect outcomes. Figure 6 shows the density of clients by the additional distance they travel to their counselor relative to going to the closest counselor. Consistent with exogenous counselor choice, 73.6% of all clients go to the closest proxy counselor location. But, for the remaining 26.4% of clients, the mean distance traveled is 35.7 miles. While this is likely due to missing information (e.g., we observe only the most recent client location in the data) or measurement error induced by our counselor assignment algorithm (e.g., using county centroids instead of actual VR office locations), we abandoned this approach because we cannot rule out that some clients choose their counselors. We also have no way to deal with potentially endogenous counselor turnover or reassignment of counselors to different locations. Thus, we cannot use the examiner or judge fixed effect design to construct our instrument as we did in Maryland and Virginia.[41]

Instead, we construct the instrument in an alternative way. We again use GIS techniques and calculate the straight-line distance between the counties where all pairwise combinations of VR clients in Oklahoma reside. Figure 7 plots the empirical density of these distances. Then, as opposed to using counselors to match a given client to other clients whose service choices should be predictive of their own, we use distance. Thus, the instrumental variable is based on the proportion of nearby individuals receiving the given service. This solves the aforementioned problems because the probability of receiving service is only a function of conditionally exogenous location characteristics. Instead of relying on unobserved characteristics of counselors (and conditional random assignment of clients to counselors), this approach relies on the unobserved characteristics of location and an assumption that the unobserved location-specific effect on service is independent of the location/service-specific effect on labor market outcomes (conditional on the previously discussed controls for local labor market conditions).

To explain how we construct the instrument for Oklahoma clients, it is helpful to first explicitly define how the Maryland and Virginia analogs are calculated. Letting i index clients, $-i$ index clients other than i, and j index services, we define $m^j_{-i,i}$ to be a binary indicator equal to 1 iff clients $-i$ and i both receive service j. Similarly, $m^l_{-i,i}$ are binary indicators equal to 1 iff clients $-i$ and i both see the same counselor. In Maryland and Virginia, the instrument for receipt of service j

[41] This identification strategy is used in a variety of contexts in the literature (Kling, 2006; Doyle, 2007, 2008; Belloni et al., 2012; Di Tella and Schargrodsky, 2013; Maestas, Mullen, and Strand, 2013; Dahl, Kostøl, and Mogstad, 2014; French and Song, 2014; Dobbie and Song, 2015; and Dobbie, Goldin, and Yang, 2018.)

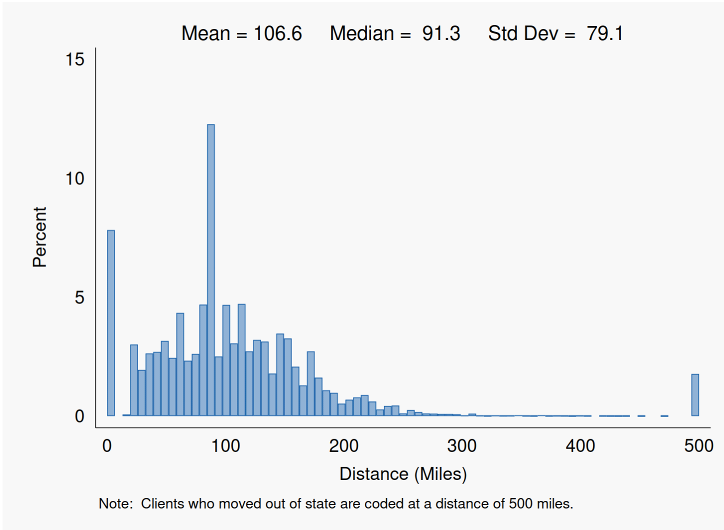

Figure 6: Density of Distance

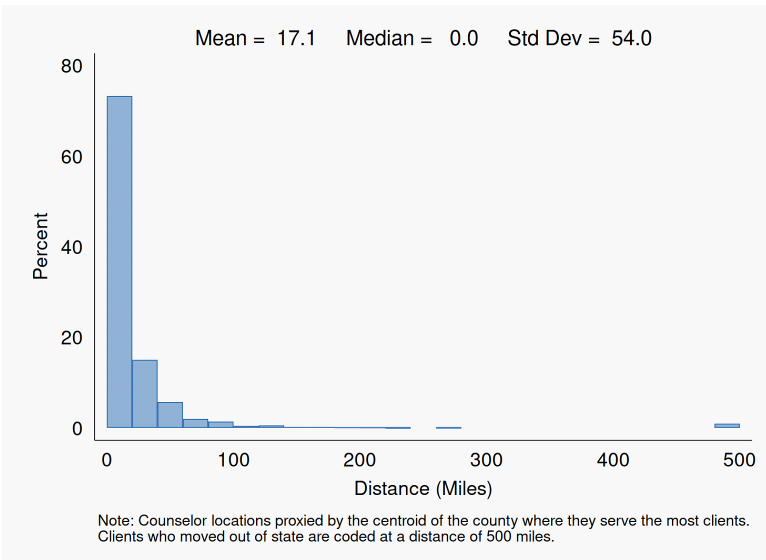

Figure 7: Density of Distance between Counselor and Client

is the proportion of other clients in our cohort for the individual's counselor receiving a particular service,

$$r_{ij}^{l} = \frac{\sum_{-i\neq i} m_{-i,i}^{j} m_{-i,i}^{l}}{\sum_{-i\neq i} m_{-i,i}^{l}}.$$

In Oklahoma, since we cannot directly observe or treat proxies for $m_{-i,i}^{l}$ as exogenous measures, we instead define the instrument using an estimable kernel function ($n(\cdot)$) of the distance between $-i$'s and i's locations ($\Delta_{-i,i}$) as

$$r_{ij}^{D} = \frac{\sum_{-i\neq i} m_{-i,i}^{j} n\left(\Delta_{-i,i}\right)}{\sum_{-i\neq i} n\left(\Delta_{-i,i}\right)}$$

where

$$n(\Delta_{-i,i}) = \exp\left\{-0.5\left(\frac{\min\left(\Delta_{-i,i}, 100\right)}{25}\right)^{2}\right\}.$$

This is a generalization of the examiner or judge fixed effect design used in Maryland and Virginia. In contrast to using a binary measure to link clients ($m_{-i,i}^{l}$), this specification uses a distance weighting function ($n(\cdot)$). This continuous function ranges from 0 (for clients 100 miles apart) to 1 (for clients from the same location). Just as the other measures limit the predictive power of other clients to those visiting the same counselor, this measure caps the effect of neighboring client service decisions to those within 100 miles.

In Virginia and Maryland, we use a transformation of the proportion of other clients from the same counselor provided service j, i.e., a counselor effect. We transform the counselor effects using an inverse normal distribution function to make it more likely that, as the counselor effects vary, their effect on service probabilities can vary by approximately the same amount. To consider why this is attractive, consider a counselor who almost always uses a particular service. We want to allow for the possibility that this will imply that all of the clients of the counselor are very likely to receive that service. Limiting the counselor effects to vary between $(0, 1)$ makes it harder for that to occur. On the other hand, using an inverse distribution function for a distribution with the real line as support makes the range $(-\infty, \infty)$.

While such a transformation makes sense analytically, in practice, it might cause problems for values of the untransformed effect at or near the boundaries. We propose a "fix" that both makes sense and solves the boundary problem. In particular, we propose replacing the untransformed effect r_{ij} with

$$r_{ij}^{*} = \left(1 - \omega_i\right) r_{ij} + \omega_i \overline{r}_j \tag{11}$$

where $\overline{r}_j$ is the mean value of r_{ij} across all counselors, $\omega_i = \kappa_i^{-1}$, and κ_i is the number of clients seen by counselor i. This specification allows the counselor effect to be more important for those counselors who have many observed clients. In fact, it has a certain Bayesian flavor to it.

There are some respondents who have missing counselor information or who have a counselor with no other clients. Because of such cases, we include a set of dummies for missing counselor effects.[42]

Tables A.2 and A.3 provide information about the moments of the transformed counselor effects. One can see that there is significant variation in both. There is some evidence of left-tailed skewness but no unreasonable outliers. The lack of outliers occurs despite zeroes for some services for some counselors because of the weighted average inherent in equation (11).

For Oklahoma, instead of conditioning on counselors, we use a kernel-weighted function of counselor choices where the argument of the kernel function is the geographical distance between each pair of VR clients. The transformation is the same. Table A.4 provides information about the moments of the transformed instruments in Oklahoma.

Appendix A.5: Estimates of the Impact of Covariates

Table A.5 displays the estimates of the effects of demographic characteristics on the propensity to use different services (y_{sij}^{*} in equation (1)). For the most part, the observed characteristics do not have statistically significant effects on service receipt. This is consistent with similar results in DPSSS. But there are some interesting exceptions. We find that VR clients have different preferences for receipt of service across the three states as indicated by the variation in state-specific constants. Next, assistive technology is used more by women than men (-0.635), while age decreases use of *diagnosis & evaluation* (*-0.890*), *training* (-2.172), and *maintenance* (-1.315). The training effect is consistent with a Mincer (1974) model. There are a few specific disability/service effects that are statistically significant but not enough to construct patterns. Severity of disability, as measured by *most significant disability* generally have large and statistically significant effects on service receipt.

[42] In fact, when a counselor has only one other client, we treat it as missing also.

Table A.2: Descriptive Statistics for Inverse-Normal Instruments of Counselor Effects in Maryland

Service	Mean	Std Dev	Minimum	Maximum
Diagnosis & Evaluation	0.594	0.543	-0.716	1.663
Training	-0.558	0.762	-2.344	1.353
Education	-1.461	0.680	-2.792	0.142
Restoration	-1.029	0.468	-1.998	0.821
Maintenance	0.641	0.501	-0.927	2.251
Placement	-1.506	0.789	-2.682	0.551
Supported Employment	-1.840	0.738	-2.860	0.093
Assistive Technology	0.420	0.653	-1.424	1.536
Orientation & Mobility	-1.307	0.505	-2.677	0.181

Note: # Obs = 490

Table A.3: Descriptive Statistics for Inverse-Normal Instruments of Counselor Effects in Virginia

Service	Mean	Std Dev	Minimum	Maximum
Diagnosis & Evaluation	1.207	1.178	-1.953	2.446
Training	-0.844	1.427	-2.404	1.890
Education	-2.246	0.717	-2.768	-0.092
Restoration	-0.912	1.491	-2.495	1.781
Maintenance	-0.021	1.045	-1.955	2.235
Placement	-1.187	1.246	-2.727	1.660
Supported Employment	-1.419	1.199	-2.788	1.631
Assistive Technology	-1.207	1.158	-2.159	1.274
Orientation & Mobility	-2.192	0.719	-2.761	-0.763

Note: # Obs = 412

Table A4: Descriptive Statistics of the Oklahoma Service Instruments

Service	Mean	Std Dev	Minimum	Maximum
Diagnosis & Evaluation	0.238	0.184	-0.262	0.862
Training	-1.081	0.167	-1.849	-0.770
Education	-1.250	0.244	-4.265	-0.485
Restoration	0.141	0.143	-0.125	0.682
Maintenance	-0.277	0.179	-0.604	0.679
Placement	-1.644	0.356	-2.764	-1.243
Supported Employment	-1.913	0.292	-2.781	-1.086
Assistive Technology	-0.764	0.243	-1.566	-0.370
Orientation & Mobility	-2.044	0.423	-4.265	-0.756

Note: # Obs = 767

Table A.5: Effect of Explanatory Variables on Service Provision

	Diagnosis & Evaluation	Training	Education	Restoration	Maintenance	Placement	Supportive Employment	Assistive Technology	Orientation & Mobility
Constant: Maryland	-0.295	-2.376 **	-0.396	-2.184 **	-1.044 **		-1.022	-0.792 *	-2.004 *
	(0.319)	(0.662)	(0.514)	(0.410)	(0.391)		(1.041)	(0.461)	(1.262)
Constant: Oklahoma	0.094	-2.268 **	-0.068	0.030	-0.668 *		-1.350	-0.958 **	-1.597
	(0.332)	(0.648)	(0.483)	(0.371)	(0.394)		(0.950)	(0.478)	(1.455)
Constant: Virginia	-1.258 **	-2.161 **	0.061	-0.198	0.120	-3.079 **	-0.436	-1.326 **	0.124
	(0.375)	(0.692)	(0.577)	(0.410)	(0.426)	(0.441)	(1.146)	(0.604)	(1.411)
Male	0.226 *	0.147	-0.352	0.149	-0.120	0.410 *	-0.063	-0.804 **	-0.518
	(0.128)	(0.178)	(0.227)	(0.145)	(0.143)	(0.267)	(0.390)	(0.167)	(0.360)
White	-0.130	-0.266	-0.096	0.088	-0.054	-0.646 **	-0.075	-0.229	-0.570
	(0.149)	(0.204)	(0.270)	(0.176)	(0.174)	(0.283)	(0.432)	(0.203)	(0.418)
Native American	-0.415	-0.368	0.351	-0.185	0.109	-1.090	0.421	-0.193	-1.931
	(0.279)	(0.425)	(0.412)	(0.295)	(0.309)	(0.731)	(0.839)	(0.344)	(1.649)
HS Diploma	0.221	0.161	-0.068	0.050	0.198	0.519	0.367	-0.221	-0.725
	(0.168)	(0.267)	(0.324)	(0.188)	(0.181)	(0.345)	(0.420)	(0.174)	(0.427)
Some College	0.429 **	0.207	0.366	0.289	0.131	0.445			-0.516
	(0.183)	(0.276)	(0.312)	(0.204)	(0.192)	(0.379)			(0.459)
College Degree	-0.319	0.030	-0.143	-0.537 *	0.024	0.140	-0.389	0.063	-1.069 *
	(0.232)	(0.359)	(0.468)	(0.281)	(0.257)	(0.535)	(0.911)	(0.254)	(0.666)
Education Missing	0.523	0.642	0.220	0.380	0.298	1.253	0.312	1.269	0.447
	(0.745)	(1.268)	(1.796)	(0.838)	(0.923)	(1.356)	(9.729)	(1.632)	-7.718
Age	-0.995 **	-2.613 **		-0.695	-2.092 **	-0.774		-1.023 **	-0.390
	(0.445)	(0.668)		(0.482)	(0.489)	(0.841)		(0.510)	(1.209)
Cognitive Impairment	0.624	-0.608		-0.798	-1.049	(0.156)	1.396 *	-1.494 *	-0.067
	(0.500)	(0.638)		(0.995)	(0.644)	(1.071)	(0.719)	(0.831)	(1.503)
Hearing Impairment	0.337	-0.017	-0.396	0.812 **	0.238	0.756 *	-0.698	-0.290	-0.302
	(0.271)	(0.408)	(0.562)	(0.287)	(0.262)	(0.472)	(0.778)	(0.297)	(0.684)
Physical Impairment	0.150	-0.297	0.253	-0.065	0.051	0.212	-0.693	-0.454 **	-0.206
	(0.132)	(0.194)	(0.247)	(0.149)	(0.143)	(0.281)	(0.403)	(0.163)	(0.337)
Learning Disability	-0.463	1.033 **	-0.282	-1.599 **	0.046	0.741	-0.019	-1.007	-1.033
	(0.408)	(0.436)	(0.679)	(0.659)	(0.469)	(0.683)	(0.884)	(0.702)	(2.283)
Mental Illness	0.035	-0.194	0.532	-0.142	0.147	0.086	0.693	-0.647 **	-0.511
	(0.210)	(0.335)	(0.359)	(0.244)	(0.267)	(0.459)	(0.524)	(0.290)	(0.636)
Substance Abuse	0.612	0.307	-0.947	0.420	-0.034	0.598	-0.022	-0.504	-0.878
	(0.415)	(0.564)	(1.212)	(0.410)	(0.444)	(0.700)	(0.788)	(0.734)	(1.697)
Disability Significant	0.445 **	1.480 **	-0.249	0.346	0.543 *	1.088 *	-0.941	-0.057	-0.999
	(0.226)	(0.562)	(0.399)	(0.265)	(0.301)	(0.586)	(1.045)	(0.433)	(1.289)
Disability Most Significant	0.539 **	2.082 **	-0.106	0.372	1.363 **	1.656 **	0.278	1.569 **	0.707
	(0.190)	(0.554)	(0.397)	(0.245)	(0.281)	(0.510)	(0.832)	(0.380)	(1.229)
Government Assistance	-0.217 *	0.476 **	-0.306	-0.367 **	0.242 *	0.353	0.461	0.465 **	0.099
	(0.121)	(0.168)	(0.265)	(0.144)	(0.148)	(0.302)	(0.407)	(0.151)	(0.358)
Veteran	-0.075	0.119	-1.947	0.440	-0.486	0.133	-0.820	-0.238	-1.232
	(0.340)	(0.543)	(1.363)	(0.360)	(0.363)	(0.707)	(1.551)	(0.424)	(1.412)
Congenital Blindness	-0.176	-0.022	0.719 **	0.015	0.235	-0.025	-0.858 *	-0.067	-0.538
	(0.173)	(0.224)	(0.261)	(0.202)	(0.201)	(0.392)	(0.515)	(0.231)	(0.603)
Young Age Dummy	-0.794 **	1.515 **	-0.263	-1.239 **	-0.269	-0.274	0.226	-0.021	0.467
	(0.332)	(0.360)	(0.418)	(0.395)	(0.345)	(0.793)	(0.675)	(0.403)	(0.786)
Prior VR Spell Dummy	1.586 **	0.941 **	0.707 **	1.209 **	1.492 **	1.090 **	0.711	0.712 **	-0.708
	(0.284)	(0.284)	(0.359)	(0.259)	(0.240)	(0.372)	(0.588)	(0.263)	(1.596)

Note: Double-starred items are statistically significant at the 5% level, and single-starred items are statistically significant at the 10% level.

Table A.6 reports the effects of the demographic, socioeconomic, and disability-related characteristics on the two labor market outcomes of interest (z^*_{sit} in equation (2) and w_{sit} in equation (3)). For labor market outcomes, almost all of the estimates are statistically significant. Many of the estimates are as expected including positive effects of level of education on both employment and earnings,[43] positive effects of age on earnings,[44] and negative effects of age on employment.[45] Different disability types have different effects on labor market outcomes,[46] but severity of disability decreases both outcomes (-0.337, -0.219). Receipt of government assistance reduces both outcomes (-0.152, -0.233).[47]

There are also some surprising results. First, while being male has a positive effect on log earnings, it has a negative effect on employment.[48] Being White causes negative effects for both labor market outcomes,[49] and being Native American has positive effects for both labor market outcomes.[50] Having congenital blindness has positive effects on both outcomes as well. The federal employment adjustment factor and cross-state commuting adjustment factor, described in Section 3.3, have interesting estimates. The estimate for the cross-state commuting adjustment factor is negative (-1.362), implying that counties with significant cross-state commuting leading to under-reporting of employment by the UI data leads to a large reduction in the measured employment rate. On the other hand, the estimate for the federal employment adjustment factor is positive (0.790). If the dominant effect was the under-reporting of employment in UI (similar to the cross-state commuting effect), then we would expect the estimate to be negative. The fact that it is positive implies that federal jobs encourage the recruiting of people with vision impairments more than enough to dominate the federal employment adjustment estimate.[51]

The last set of estimates, displayed in Figures 8 and 9, are state-specific level spline estimates for employment and log earnings. Relative to effects prior to service (solid blue), Oklahoma exhibits increases in both employment and earnings in both the short and long run. On the other hand, Maryland and Virginia exhibit decreases in the short and long run for employment and small decreases in earnings in the short and long run.

Appendix A.6: Approximate Control for Selection Bias

Let z^w be a linear index for log wage, z^p be a linear index for partition probability, and $\widehat{w}\left(z^w, z^p\right)$ be the predicted log wage taking into account selection bias. Let $\{(w_j, s_j)\}_{j=1}^n$ be the set of log wage, (unobserved selection term) for a sample of size n. Define $\widehat{w}\left(z^w, z^p\right)$ as the semiparametric kernel-estimated log wage taking selection bias into account. Then,[52]

$$\widehat{w}\left(z^w, z^p\right) = \frac{\sum_j \sum_k \left(w_j + s_k\right) K\left(\frac{z_j^w - z^w}{b_n^w}\right) K\left(\frac{z_k^p - z^p}{b_n^p}\right)}{\sum_j \sum_k K\left(\frac{z_j^w - z^w}{b_n^w}\right) K\left(\frac{z_k^p - z^p}{b_n^p}\right)}$$

$$= \frac{\sum_j \sum_k w_j K\left(\frac{z_j^w - z^w}{b_n^w}\right) K\left(\frac{z_k^p - z^p}{b_n^p}\right)}{\sum_j \sum_k K\left(\frac{z_j^w - z^w}{b_n^w}\right) K\left(\frac{z_k^p - z^p}{b_n^p}\right)} + \frac{\sum_j \sum_k s_k K\left(\frac{z_j^w - z^w}{b_n^w}\right) K\left(\frac{z_k^p - z^p}{b_n^p}\right)}{\sum_j \sum_k K\left(\frac{z_j^w - z^w}{b_n^w}\right) K\left(\frac{z_k^p - z^p}{b_n^p}\right)}$$

$$= \frac{\sum_j w_j K\left(\frac{z_j^w - z^w}{b_n^w}\right) \sum_k K\left(\frac{z_k^p - z^p}{b_n^p}\right)}{\sum_j K\left(\frac{z_j^w - z^w}{b_n^w}\right) \sum_k K\left(\frac{z_k^p - z^p}{b_n^p}\right)} + \frac{\sum_k s_k K\left(\frac{z_k^p - z^p}{b_n^p}\right) \sum_j K\left(\frac{z_j^w - z^w}{b_n^w}\right)}{\sum_k K\left(\frac{z_k^p - z^p}{b_n^p}\right) \sum_j K\left(\frac{z_j^w - z^w}{b_n^w}\right)}$$

$$= \frac{\sum_j w_j K\left(\frac{z_j^w - z^w}{b_n^w}\right)}{\sum_j K\left(\frac{z_j^w - z^w}{b_n^w}\right)} + \frac{\sum_k s_k K\left(\frac{z_k^p - z^p}{b_n^p}\right)}{\sum_k K\left(\frac{z_k^p - z^p}{b_n^p}\right)}.$$

[43] Leonard, D'Allura, and Horowitz (1999), Capella (2001), Cavenaugh, Giesen, and Steinman (2006), and Bell (2010) find similar results for the effect of education on labor market outcomes for people who are vision-impaired.

[44] This is inconsistent with Capella (2001).

[45] This is consistent with Martz and Xu (2008).

[46] Cavenaugh, Giesen, and Steinman (2006) and Warren-Peace (2009) find that the existence of a secondary disability reduces employment propensity.

[47] Some government assistance programs, especially those for people with disabilities, include a maximum amount one can earn without losing benefits; thus, the negative effect on log earnings. Darensbourg (2013) finds similar results.

[48] Martz and Xu (2008), Warren-Peace (2009) and Darensbourg (2013) find that *male* has a positive effect on employment.

[49] This result is consistent with Warren-Peace (2009).

[50] This is inconsistent with Bell (2010).

[51] Schmidt et al. (2019) find similar results for both adjustment factors.

[52] There is no need to worry about correlation because $Corr\left(z_j^w, z_k^p\right) = 0 \forall k \neq j$.

Table A.6: Effect of Explanatory Variables on Labor Market Outcomes

	Employment		log Conditional Quarterly Earnings		
	Estimate	Std Err	Estimate	Std Err	Net Benefit
Male	0.196 **	0.012	0.461 **	0.010	0.467
White	0.304 **	0.015	0.169 **	0.012	0.054
Native American	0.772 **	0.025	0.399 **	0.021	1.562
HS Diploma	0.444 **	0.017	0.364 **	0.014	0.545
Some College	0.875 **	0.018	0.794 **	0.014	1.395
College Degree	0.931 **	0.023	1.344 **	0.018	2.457
Education Missing	0.122	0.105	-0.401 **	0.054	-0.129
Age	1.387 **	0.047			
Cognitive Impairment	-0.174 **	0.048	-0.033	0.024	-0.398
Hearing Impairment			-0.125 **	0.011	-0.125
Physical Impairment	-0.044 **	0.013	-0.310 **	0.040	-0.318
Learning Disability	-0.851 **	0.04	-0.180 **	0.017	-1.820
Mental Illness	-0.012	0.024	0.126 **	0.033	0.111
Substance Abuse	0.504 **	0.034	0.145 **	0.017	1.078
Disability Significant	0.393 **	0.02	-0.305 **	0.015	-0.154
Disability Most Significant	-0.163 **	0.018	-0.697 **	0.009	-0.684
Government Assistance	-0.61 **	0.013	-1.001 **	0.031	-1.160
Veteran	-0.784 **	0.035	0.209 **	0.011	-1.146
Congenital Blindness	0.473 **	0.016			
Fed Empl Adj Factor	-2.652 **	0.291			
Commuting Adj Factor	-0.385 **	0.157			
Young Age Dummy	-0.113 **	0.036	0.049	0.043	-0.122
Prior VR Spell Dummy	0.402 **	0.02	-0.109 **	0.020	0.413
Labor Market Employment	-0.128 **	0.025	-0.038	0.027	
Maryland Before Service	-1.698 **	0.064	4.668 **	0.032	
Maryland Ashenfelter	-1.711 **	0.152	3.919 **	0.126	
Maryland SR After Service	-1.702 **	0.064	4.262 **	0.040	
Maryland LR After Service	-2.192 **	0.067	4.404 **	0.044	
Oklahoma Before Service	-1.736 **	0.075	3.684 **	0.072	
Oklahoma Ashenfelter	-1.069 **	0.177	3.280 **	0.133	
Oklahoma SR After Service	-0.995 **	0.078	3.616 **	0.077	
Oklahoma LR After Service	-1.352 **	0.076	3.768 **	0.074	
Virginia Before Service	-1.963 **	0.059	4.139 **	0.038	
Virginia Ashenfelter	-1.301 **	0.184	3.843 **	0.130	
Virginia SR After Service	-1.906 **	0.062	3.789 **	0.043	
Virginia LR After Service	-2.408 **	0.066	3.603 **	0.042	

Notes:
1) Double-starred items are statistically significant at the 5% level, and single-starred items are statistically significant at the 10% level.
2) Net benefit is approximated as "$-\Phi^{-1}(x)c_1+c_2$" where $-\Phi^{-1}()$ is the inverse standard normal distribution function, x is the mean value of the explanatory variable across all three states, c_1 is the coefficient estimate for the employment effect, and c_2 is the coefficient estimate for the log conditional quarterly earnings effect.

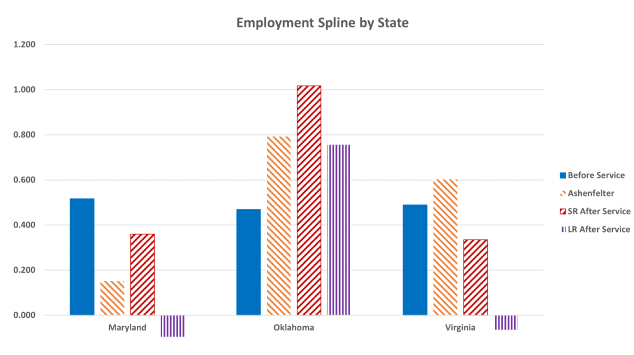

Figure 8: Employment Spline by State

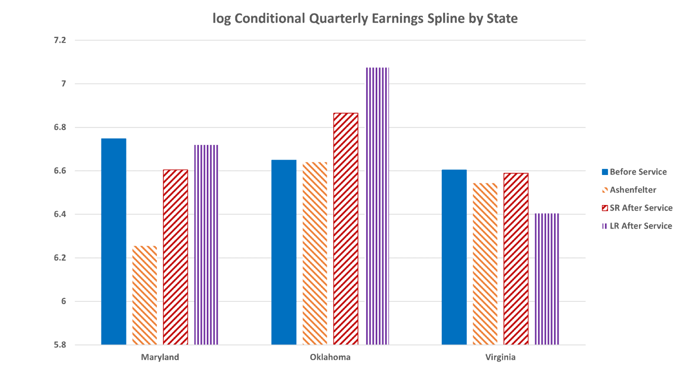

Figure 9: log Conditional Quarterly Earnings Spline by State

In order to implement so that there is true variation in the selection bias term, one must compute

$$z^p\left(z^w\right)=\frac{\sum_k z_k^p K\left(\frac{z_k^w-z^w}{b_n^w}\right)}{\sum_k K\left(\frac{z_k^w-z^w}{b_n^w}\right)}.$$

Figure 7 shows the results of using the approximate selection bias. Without selection bias the sample curve is above the 45° line, and it is relatively flat. The addition of selection bias causes a curve with a little more slope and below the 45° line.

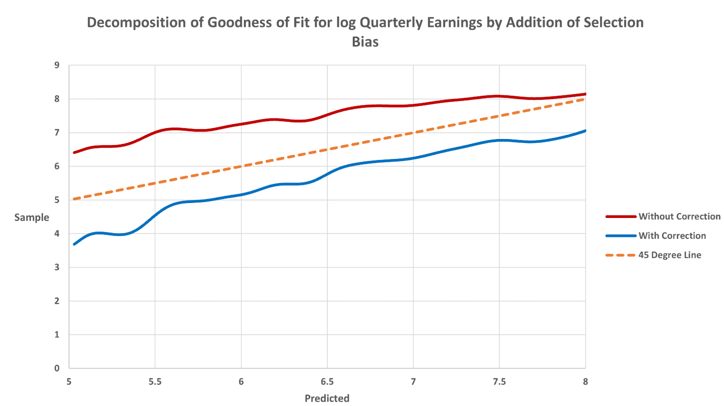

Figure 10: Decomposition of Goodness of Fit for log Quarterly Earnings by Addition of Selection Bias

Appendix A.7: Probability of Disability Conditional on Quarters Prior to Application

As discussed in Section 2, some people become vision-impaired just prior to VR application when they develop the disability. To account for the unobserved onset of vision impairments, we use the approach in the Dean et al. (2018) analysis of VR clients with physical disabilities. This approach infers the onset probability from negative breaks in pre-service labor market outcomes.

Figure 7 displays the estimated probability of the onset of disability prior to the quarter of application. So, for example, we estimate that just over 9% of VR clients became vision-impaired 12 quarters before applying for VR services.

Table A.7: Proportions of People with Disabilities Working for Local, State, or Federal Government

State	Any Disability	Just Blind or Vision-Impaired
Maryland	0.127	0.0200
Oklahoma	0.078	0.0137
Virginia	0.087	0.0118

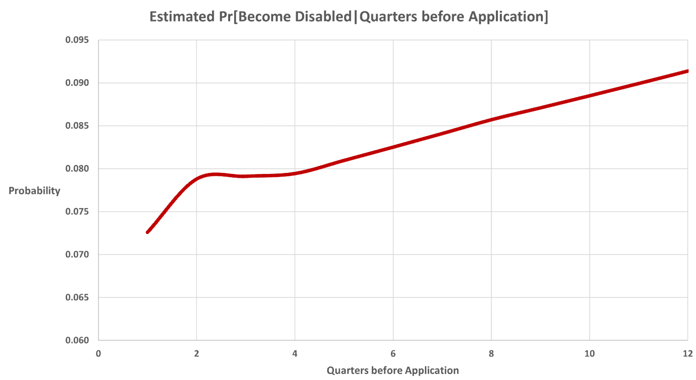

Figure 11: Estimated Pr[Become Disabled| Quarters before Application

Appendix A.8: Probability of Working for the Federal Government

We calculate the proportions of employed people with a disability who work for the government by state. We also calculate the analogous proportions for individuals who are vision-impaired.[53] The proportions are reported in Table A.7. This is not a perfect set of numbers as we would like the analysis to include only Federal government workers (and no local or state government workers). Also, we would prefer to limit the analysis to VR consumers. Yet, the numbers clearly show that employment of people with vision impairment is quite rare.

Appendix A.9: Specification Tests

We use standard goodness-of-fit measures to see how well we are predicting service provision probabilities. We plot the sample probabilities as a function of predicted probabilities as seen in Figure 7. The horizontal axis represents predicted service probabilities based on model estimates. The vertical axis represents sample service probabilities using a kernel estimator with the predicted probability as the kernel argument. Each curve corresponds to a different service. To the degree that curves are close to the (included) 45° line, the model does a good job of explaining the data. For the six services represented in the figure, they deviate enough from the 45° line, suggesting rejection of the model. But, in terms of economic significance, the curves do a very good job of explaining the data. There are also three services (*placement*, *supported employment*, and *orientation & mobility*) where the fit is not good. But, for all three, the curves do not fit well because they show too much curvature suggesting a bigger bandwidth might be warranted.[54]

[53] To do so, we use data from the 2012 American Community Survey (ACS) obtained from IPUMS USA (Ruggles et al., 2022). This is the earliest year that an indicator for a vision impairment is available in the 1-year, 3-year, and 5-year ACS samples. We restrict the sample to those who report being employed, are between the ages of 18 and 65 years of age, and are not living in group quarters. We are not able to identify individuals who work for the federal government directly. Instead, we define those who work for a federal, state, or local government institution as individuals with a North American Industrial Classification System (NAICS) industry code in the public administration category.

The ACS reports indicators for individuals with different types of disabilities that are defined in terms of cognitive, ambulatory, independent living, self-care, vision, and hearing diffculties. We define an individual as disabled if he report having any one of those diffculties. We classify individuals as vision-impaired if the respondent is blind or has serious diffculty seeing even with corrective lenses.

[54] The figure with too much curvature is available at steven.stern@stonybrook.edu

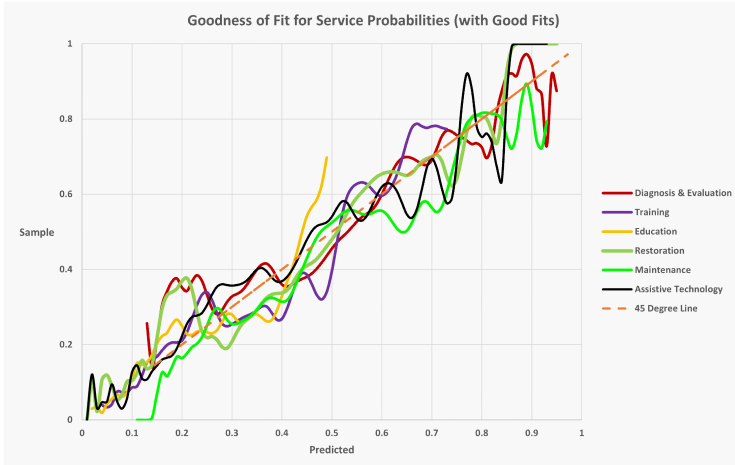

Figure 12: Goodness of Fit for Service Probabilities (with Good Fits)

We perform the same procedure for employment probabilities disaggregated into probabilities before and after service receipt.[55] Figure 7 plots the deviations between predicted and sample employment probabilities for the two periods in a way similar to Figure 7. Deviations between the 45° line and the other two *sample lines* at any particular predicted probability represent that part of employment probability that we are not predicting. The model reasonably tracks sample employment probabilities for most of the range of predicted employment probabilities. Also, to the degree that they differ from the 45° line, they differ by approximately the same amounts for before and after service implying that the biases cancel each other for the most part.

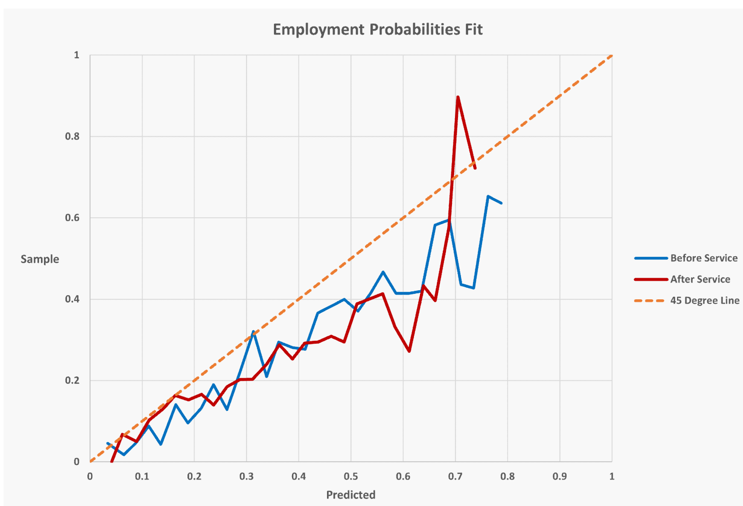

Figure 13: Employment Probabilities Fit

For earnings, we have to include expected selection bias into the predicted values. How we do this is explained in Appendix A.6. The results are shown in Figure 7. Here, we track the derivative of the curve very well. However, we overestimate the amount of selection bias but in a very consistent way for people with different characteristics. This may occur because the error structure in equation (4) implies a much more complex selection bias term than what we use.

[55] *Before service receipt* includes the quarter before receipt, and *after service receipt* includes quarters in the first 2.5 years after receipt and the longer run.

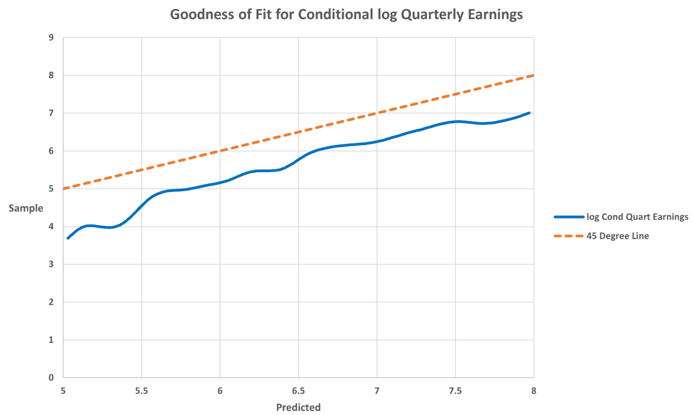

Figure 14: Goodness of Fit for Conditional log Quarterly Earnings

Finally, using a series of Lagrange Multiplier (LM) tests, we consider allowing for interactions among services and find strong evidence of no such interactions. We also test for service duration effects on employment propensity and conditional log quarterly earnings.[56] Again, we find strong evidence of no significant effects for any single interaction. However, taken as a whole, the corresponding χ^2 test statistic is statistically significant. This happens because the inclusion of duration affects the estimates of other parameters not part of the test.[57]

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[57] For this particular test, the effect of including service duration is to increase two factor loadings, $\lambda^y_{1,1}$ and $\lambda^y_{19,1}$ in equation (4).

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