Diversity and Inclusion Research

Christopher M. Clapp
John Pepper
Robert Schmidt
Steven Stern

Evaluation of Vocational Rehabilitation Services

Return on Investment (VR-ROI) Models and Results

Springer

Diversity and Inclusion Research

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Christopher M. Clapp
Harris School of Public Policy
University of Chicago
Chicago, IL, USA

John Pepper
Economics
University of Virginia
Charlottesville, VA, USA

Robert Schmidt
Professor Emeritus of Economics
University of Richmond
Richmond, VA, USA

Steven Stern
Economics
Stony Brook University
Stony Brook, NY, USA

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Preface

Nearly 35 years ago, David Dean, along with various coauthors, embarked on a pioneering journey to examine the effectiveness of the Federal-State Vocational Rehabilitation (VR) program on employment outcomes using the tools of economics. Although labor economists had long studied the impacts of job training programs for disadvantaged populations, the VR program had not been subject to the same level of rigorous empirical analysis. David recognized this gap, and it became his professional mission and passion to bring the rigor of economics to the evaluation of VR services, which were poorly understood in terms of their long-term impact on employment outcomes for people with disabilities.

Early studies published in the 1990s demonstrated the potential of applying sophisticated econometric tools to the VR program (e.g., Dean & Dolan, 1991a, 1991b). This work revealed important insights about how VR services influence labor market outcomes, sparking greater interest and engagement among economists and policymakers alike. Despite this important progress, there was still a recognized need for improved evaluations of the program, as highlighted in several U.-S. Government Accountability Office reports (2005, 2012). These reports emphasized the shortcomings of existing performance measures and the urgent need for more credible and detailed evaluations.

In response, a team of economists – Chris Clapp, David Dean (deceased 2013), John Pepper, Robert Schmidt, and Steven Stern – in collaboration with VR agency staff who provided critical support and guidance, took up the challenge of developing a more robust evaluation framework for the VR program. This initiative, termed the VR-ROI (return on investment) project, aimed to provide more accurate assessments of the program's long-term economic impacts. Supported by funding from various agencies and informed by advisory groups, conference presentations, and publications, the VR-ROI model was born (see vrroi.org for more information).

Modern VR-ROI analysis seeks to address many of the limitations in previous VR performance data and evaluation methods. This new approach offers methodologically sound and feasible strategies that enable state VR programs to demonstrate their employment impacts and effectiveness for people with disabilities. As a result of our ongoing work, we have developed refined ROI models for state VR agencies that aim to produce more credible, data-based results. The central features of this paradigm are:

1. The use of readily available VR data on individual clients;
2. The linkage of administrative VR client data with longitudinal administrative data from state unemployment insurance systems to examine short- and long-run earnings and employment outcomes;
3. Estimation of the impacts of specific types of VR services separately for people with specific types of disabling conditions; and
4. The use of advanced econometric methods to ensure that the estimated effects are the result of VR services rather than other external factors.

This book represents the latest advances in VR-ROI analysis, and builds on the foundation established in the early 2000s by incorporating more recent data and expanding the analysis to cover additional states and services. By building on decades of analysis, our results present the most comprehensive and detailed application of the VR-ROI model to date. Focusing on applicants during Program Year 2012 (and spanning 2007–2012 for individuals who are blind or visually impaired), we use longitudinal quarterly employment and earnings data to assess the impacts of vocational rehabilitation services across five states: Kentucky, Maryland, North Carolina, Texas, and Virginia. For the first time, the VR-ROI model is used to simultaneously compare short- and long-term labor market outcomes across multiple state agencies and four distinct disability groups: mental illness (MI), physical impairment (PI), cognitive impairment (CI), and blindness and visual impairment (BVI).

We report the impacts of VR services across 19 separate state-disability categories. These estimates allow for nuanced understanding of how different VR services perform across states and disability types, and across the short and long run. This yields unparalleled insights into the diverse effects of vocational rehabilitation services, and, when compared with our earlier results from program year 2000, highlights the evolving benefits and challenges of VR programs over time. By offering this broad and in-depth evaluation, the book helps to bridge the gap between research and practice and to equip stakeholders with data-driven insights to enhance vocational rehabilitation programs for individuals with disabilities.

At the same time, the book relies on data from an earlier period, specifically 2012. A full and accurate picture of the return on investment (ROI) of vocational rehabilitation services requires a long window of data to assess not only the short-term but also the long-term effects of VR interventions. This extended timeframe allows us to capture the delayed benefits that often arise in employment and earnings outcomes for individuals with disabilities.

However, our focus on a comprehensive, longitudinal perspective means that the evaluations presented in this book may not directly reflect the current state of the VR program, especially given significant changes in recent years. Since 2012, the most notable change in the VR landscape has been the passage of the Workforce Innovation and Opportunity Act (WIOA) in 2014. The WIOA introduced new goals and measurable skill attainment metrics, particularly with respect to Pre-Employment Transition Services (Pre-ETS). Although our analysis predates these changes and cannot capture the long-run effects of WIOA, the methodology outlined in this book offers a robust framework for assessing the evolving impacts of VR services. Our approach conforms to or exceeds the standards set by WIOA and provides a valuable outline for how future studies can measure the effects of VR as the program continues to adapt.

In addition to the results we report from the 2012 VR program, there is much to learn by examining earlier periods. This book offers insights into how VR services have worked across different settings and client populations, and provides lessons that remain relevant for stakeholders today. By understanding how the program operated in previous periods, policymakers, administrators, and researchers can better anticipate future challenges and opportunities and apply these lessons to today's VR programs.

It is also clear that further research and refinements are needed to ensure that vocational rehabilitation services continue to evolve and improve. One of the key challenges ahead is the need for updated data that reflect recent changes in the VR program, particularly following implementation of WIOA. Future iterations of the VR-ROI model will incorporate data that captures the effects of WIOA-mandated changes, such as Pre-ETS and new performance measures. This will provide more current understanding of how these policy shifts are influencing the long-term employment outcomes of individuals with disabilities.

In addition to updating the data, there is a need to refine the VR-ROI model itself. Future work will focus on simplifying the model to enable VR agency staff to conduct timely ROI evaluations using the resources they have available. This may involve developing user-friendly tools that allow for quicker assessments while maintaining accuracy. Simplifying the model could also help administrators better understand service intensity—the amount of time and resources devoted to individual clients—and its relationship to client outcomes, which is crucial for efficient resource allocation.

Further, research should explore how VR services impact not only labor market outcomes but also broader, nonfinancial benefits such as independence, quality of life, and social integration. These outcomes are essential for understanding the full value of VR services, particularly in cases in which the financial ROI may appear less favorable. Incorporating these non-labor market outcomes into future analyses will yield a more holistic view of the program's impact and better inform decision-making by VR counselors, administrators, and policymakers.

We would not have been able to complete this book or our other work on the VR-ROI model and analysis without the input and assistance of many scholars, policy officials, and VR program administrators. We thank the agencies and staff directly involved in this project:

- Maryland's Division of Rehabilitation Services (MD DORS)
- Kentucky's Office of Vocational Rehabilitation (KY OVR)
- Virginia's Department for Aging and Rehabilitative Services (VA DARS) and the Department for the Blind and Vision Impaired (VA DBVI)
- North Carolina's Division of Vocational Rehabilitation Services (NC DVRS) and the Division of Services for the Blind (NC DSB)

- Texas' Division for Rehabilitation Services (TX DRS) and the Division for Blind Services (TX DBS), which are now combined into the Workforce Commission (TWC)

We also thank the Virginia VA DARS project staff, who provided invaluable help during the initial phases of the project, and staff at the George Washington University Center for Rehabilitation Counseling Research and Education (GW-CRCRE).

Among the many individuals who have commented on our work, we would like to thank Joe Ashley, Robert Froehlich, Katherine Hurley, Catherine Ipsen, Maureen McGuire-Kuletz, Vlad Mednikov, John Phelps, Jim Rothrock, Kirsten Rowe, Ann Stanfield, and David Stapleton. We also benefited from workshop participants at the Allied Social Science Associations (ASSA) Meetings, the Summer Econometric Society Meetings, the Association for Public Policy Analysis and Management (APPAM), Clemson University, Western Ontario University, Washington University, University of Southern California, University of Louisville, University of Zurich, Hong Kong University of Science and Technology, National University of Singapore, Singapore Management University, and the University of Maryland for excellent help and advice. Finally, we thank Barbara Nordin for her excellent editorial work at several stages of manuscript preparation.

Development of the VR-ROI approach has been generously funded by two grants from the National Institute of Disability, Independent Living, and Rehabilitation Research (NIDILRR) awarded to the University of Richmond: a 3-year Field Initiated Project (FIP) awarded in 2010 and a 5-year Disability and Rehabilitation Research Project (DRRP) awarded in 2014. Additional financial support has been provided by the Virginia DARS, the Virginia Department of Medical Assistive Services, the NIDILRR, and the University of Virginia Bankard Fund for Political Economy. More recently, the NIDILRR awarded a 3-year FIP grant to the GW-CRCRE for continued development of the VR-ROI approach.

Although the people and agencies listed above provided substantial constructive input and suggestions, they were not asked to endorse the conclusions. Responsibility for the final context of this book rests entirely with the authors. All conclusions and any errors are ours.

Chicago, IL, USA — Christopher M. Clapp
Charlottesville, VA, USA — John Pepper
Richmond, VA, USA — Robert Schmidt
Stony Brook, NY, USA — Steven Stern

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Contents

Introduction

1

1.1 Need and Target Population

More than 46 million people, or about 13.9% of the civilian, non-institutionalized population, live with a disability (Houtenville & Bach, 2024). Despite being a common condition among Americans, there are large disparities between the labor-market experiences of disabled people and their non-disabled peers. Only 44.5% of working-age adults with a disability are employed, compared with 78.9% of those without a disability (Houtenville & Bach, 2024). What's more, individuals with disabilities are often acutely affected by economic downturns. During the 2008 Great Recession, unemployment rates for those with disabilities far exceeded the rates of those without, and, as the economy recovered, people with disabilities experienced 20% longer unemployment durations than their non-disabled peers (Fogg et al., 2010). Schur et al. (2023) find that, while workers without disabilities suffered large job losses (of 15.5%) in the early days of the COVID-19 pandemic, their peers with disabilities saw larger declines in employment (18.9% on average, ranging from 15.6% to 31.1% depending on disability type).[1]

Compounding these challenges, employed people with disabilities earn less than their non-disabled peers. Prior to the COVID-19 pandemic, the median annual earnings of individuals with disabilities 16 years and older was $24,106, compared with $36,066 for those without disabilities (U.S. Census Bureau, 2019). This is partly because people with disabilities are more likely to hold part-time jobs (Brault, 2008). But, even among full-time workers, those with disabilities earn significantly less than their peers without disabilities. For example, Houtenville and Bach (2024) document that full-time workers with a disability earned $8331 less than those

[1] Ne'eman and Maestas (2023) report that, starting in the second half of 2021, the employment rate of those with disabilities recovered faster than the rate of those without.

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without a disability in 2022. They also report that 25.9% of working-age individuals with a disability live in poverty, compared with 11.5% for those without a disability.

1.2 Vocational Rehabilitation Program Background

To address these employment and economic disparities, the Federal-State Vocational Rehabilitation (VR) program has provided employment-related services to people with disabilities since the end of World War I. The program has evolved from its original mission of serving people with war- or occupation-related physical injuries to one that now serves individuals with physical, cognitive, psychiatric, and sensory disabilities, including transition-aged youth with disabilities (i.e., youth entering the labor market), in acquiring and maintaining competitive, integrated employment. More than 800,000 people are served by the VR program each year (U.S. Department of Education, 2022). In fiscal year 2020, the U.S. Department of Education, which oversees the program, allocated $3.4 billion in federal funds to the VR program, and states contributed another $905 million (U.S. Department of Education, 2022).

Despite increasing rates of disability over the past decade, federal funding for the public VR program has decreased by 13.3% in real terms in recent years (U.-S. Government Accountability Office, 2019). Unfortunately, these VR budget cuts have significant consequences for people with disabilities. When agencies receive more eligible applicants than they can afford to serve, applicants are assigned to priority categories based on the severity of their disability. Only individuals with the most significant disabilities receive services under what’s known as an “order of selection” (Ipsen & Stern, 2020). Based on the most recent Rehabilitation Services Administration (RSA) annual report, in 2019, more than half of all VR agencies (41 out of 78) were unable to serve all eligible individuals and implemented an order of selection to ration services (U.S. Department of Education, 2022). Eight VR agencies had to close all categories and were forced to place all new eligible applicants on a (priority-category-specific) waiting list.

Although the VR program has been perceived as playing an important role in helping persons with disabilities engage in gainful employment (Loprest, 2007), there has long been a desire for credible demonstrations of the value of public investments in the Federal-State VR services program. The Workforce Investment Act of 1998 (WIA) amended the Rehabilitation Act of 1973 to require that programs that fall under its authority be evaluated regarding “their cost, their impact on related programs, and their structure and mechanisms for delivery of services, using appropriate methodology and evaluative research designs” (Public Law 105–220, 1998). Yet a subsequent study by the U.S. Government Accountability Office (2005, 2012) identified a number of weaknesses in existing performance measures and concluded that better metrics and evaluation methods could improve VR’s performance.

This critical need for more rigorous and credible evaluations of the efficacy and return on investment of the VR program was codified in the 2014 Workforce Innovation and Opportunity Act (WIOA) which sought to address concerns about demonstrating program value by requiring that standards and practices be coordinated across workforce programs, including VR (Public Law 113–129, 2014).[2] To that end, WIOA outlined six primary performance indicators that require longitudinal data to measure post-VR employment and earnings (in addition to educational and employer-relevant outcomes). To ensure accountability via data-driven, evidence-based evaluation techniques, the indicators are to be adjusted based on economic conditions and participant characteristics via statistical modeling. In response to WIOA, the National Institute on Disability, Independent Living, and Rehabilitation Research's (2018) Long-Range Plan highlighted a "need for valid models of return on investment that are usable by state VR agencies" (p. 15) and to "lay the groundwork for the development and use of evidence-based VR practices" (p. 16).

1.3 The VR-ROI Model

To improve the efficacy of the VR program and employment outcomes for people with disabilities, we developed a return on investment (ROI) model that estimates the value created by VR services relative to the cost of providing those services (Clapp et al., 2019, and Chap. 3). We build on pioneering work on VR program evaluation in a set of papers by the four authors and David Dean, but also use more recent data and evaluate more state VR agencies. As a result, this book provides the most up-to-date and comprehensive analysis of the VR program in the United States: We describe our VR-ROI model and explain how it can be used to assist VR recipients, counselors, agency administrators, and policymakers in making evidence-informed decisions.

The VR-ROI model measures the effect of VR services on two labor market outcomes (employment and earnings) in both the short and long run. To do so, we combine administrative data on all applicants during a single year, State Fiscal Year 2012, from VR agencies in five states (Kentucky, Maryland, North Carolina, Texas, and Virginia).[3] These data contain information on an applicant's disabilities, other demographics, VR services received, and the cost of the purchased services, but not on the individual's labor market outcomes. To obtain quarterly employment and earnings data, the VR agencies merge information from their state unemployment insurance (UI) system with the VR data.[4] The UI agencies provided us with quarterly earnings for at least nine quarters prior to the application quarter and at least 20 quarters after.

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[2] Concerns about demonstrating program efficacy are not specific to the evaluation of workforce programs like VR. It is part of a broader push toward "evidenced-based policymaking" in the Federal government (Abraham et al., 2018; Public Law 115–435, 2019).

[3] In Virginia and Maryland, we use 6 years of data for people who are blind or vision-impaired because there are not enough observations in only one year.

[4] We are not able to determine the identities of VR applicants from this data, because state agencies removed all personally identifiable information before providing us with the data.

The model contains service choices depending on the characteristics of clients and outcomes depending on client characteristics and service choices. We separately estimate the VR-ROI model using data from the five states for individuals with four types of disabilities: mental illness (MI: anxiety disorders, depressive and other mood disorders, personality disorders, schizophrenia, and other mood disorders); physical impairment (PI: internal and musculoskeletal); cognitive impairment (CI: intellectual disability and learning disability); and blindness or visual impairment (BVI). We report both estimates of the effect different categories of VR services have on client labor market outcomes and the ROI on the receipt of those services for each combination of state and disability category.

We obtain results that are somewhat disappointing (see Chap. 5). For instance, many services yield negative benefits, and costs are larger than overall benefits.

1.4 Benefits of This Book

This book's unique content provides a number of benefits, including the following:

- ROI models and methodology that conform to or exceed WIOA standards. We do not address several important topics, such as Pre-ETS, goals, and measurable skill attainment. This is because WIOA was passed too recently to enable measuring its long-run effects. However, our methodology still provides an outline for how to measure the effects of VR as it evolves with time.
- Knowledge that directly benefits individuals with disabilities, VR program administrators/counselors, and policymakers in several respects. Our results provide information for VR consumers about the effectiveness of different services for people with different types of disabilities that assists them in making informed service provision choices. This enables administrators and policymakers to better understand which aspects of VR service provision require modification to become more effective.
- A valuable guide for VR agencies seeking to understand the key issues involved in performing ROI analysis, replicate our analyses, and calculate their own ROI estimates using either our sophisticated model or simpler models.
- A roadmap for future areas of inquiry to better understand the ROI of the VR program.
- A discussion of the trade-off between rapid short-term benefit evaluation and slow long-term benefit evaluation. We strongly argue for the advantages of long-term benefit evaluation while acknowledging the advantages of short-term evaluation.

The rest of the book is organized as follows. Chapter 2 reviews the relevant literature. Chapter 3 describes the data, the model, and the method of estimation used in the model. Chapter 4 summarizes the state and national data available to do VR estimation and evaluation. Chapters 5 and 6 are the heart of the book and report new empirical results for the five states. Chapter 5 presents estimated ROI results using our VR-ROI model, and Chap. 6 compares those results with those obtained using a much simpler model. Chapter 7 provides a discussion of relevant ethics issues. It also provides conclusions and discusses possible directions for future work.

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2 Literature Review

2.1 Introduction

Return on investment (ROI) analysis of state vocational rehabilitation (VR) agencies is a way to evaluate the efficacy of a VR program. Several different formulas can be used to make this comparison, but all compare program benefits with costs in some manner. For every dollar spent on services provided to a VR client, the ROI reports how many extra dollars (in present value terms) the client earns as a result. Although an ROI measure is straightforward to calculate given its components, credibly estimating program benefits and costs from available data can be difficult (King & O'Shea, 2003; Clapp et al., 2019). In this chapter, we review the empirical literature on VR program ROI estimation. To do so, we first provide an overview of the basic conceptual issues involved in estimating VR program benefits and costs and, ultimately, the ROI of VR programs. Our aim is to highlight some of the key issues in ROI evaluations and how the approaches used in the VR literature have evolved over time, not to provide an exhaustive how-to guide. McGuire-Kuletz and Tomlinson (2015) and articles in the special issue introduced by Schmidt et al. (2019b) provide a more detailed guide to ROI analysis of VR programs.[1]

We proceed by discussing the conceptual issues inherent in estimating program benefits in Sect. 2.2. Next, we discuss a methodology for determining VR costs in Sect. 2.3. In Sect. 2.4, we briefly preview how these components are combined to calculate an ROI. This allows us to compare the results from a set of VR research papers by the authors of this book. Finally, Sect. 2.5 concludes with guidance on what we've learned about what constitutes an effective ROI analysis in the VR context based on that line of research.

[1] This chapter builds on the discussion in Clapp et al. (2019). For a more technical discussion of the econometric issues in evaluating job training programs, see Heckman et al. (1999). For a history of workforce training programs, see Barnow and Smith (2016).

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2.2 Estimating the Benefits of VR on Labor Market Outcomes

Estimating the impact of VR services on the individuals who receive them requires conducting what is broadly known as a program or impact evaluation.[2] These evaluations are not unique to the VR context and seek to answer the question, "What change did the intervention *cause* in the outcome of interest?" Interventions are broadly defined as any action that affects the outcome but are often a type of policy, program, or service provided. In contrast to program inputs or outputs, outcomes of interest are measures that directly matter in the lives of those the program serves. In the VR context, employment and wages are the outcome measures in most studies (Boeltzig-Brown et al., 2017; Phillips et al., 2021) because they are relatively straightforward to observe and record, but other measures of a VR recipient's overall well-being are important areas for future research (e.g., the ability to live independently).[3]

If individuals have improved employment outcomes after participating in a VR program, how do we determine whether those improvements can be attributed to the VR intervention? In order to determine a causal effect (whether the changes directly result from the program) the actual, observed, realized outcomes of those who are treated are compared with the counterfactual outcomes that would have resulted in the absence of the program or intervention, and vice versa (Smith, 2000; Hollenbeck & Huang, 2006). Since these outcomes are what would have occurred in a different state of the world, they are, as their name implies, counterfactual and unobservable. What would have happened to the employment and earnings of VR recipients had they not received services? What would have happened to the employment and earnings of non-recipients had they received VR services? Since these outcomes cannot be observed, regardless of setting, the counterfactual outcomes problem is inherent in all program evaluations and not just those of VR. This is often referred to as the fundamental problem of causal inference. The counterfactual outcomes problem can be thought of as a missing data problem because the outcomes of the treated are missing in the untreated state and the outcomes of the untreated are missing in the treated state. Thus, the data we observe alone cannot be used to address the issue. Instead, to design a credible evaluation, we must come up with a reasonable proxy for the missing counterfactual outcome.

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[2] There is a large literature on program evaluation methods (which are also referred to as causal inference or applied econometric techniques). For textbook treatments of this topic, we refer the reader to Bueno de Mesquita and Fowler (2021) and/or Huntington-Klein (2021). More advanced treatments include those by Cameron and Trivedi (2005), Manski (2007), Wooldridge (2010), and Hansen (2022).

[3] Ideally, the outcomes analyzed would measure all benefits and costs of VR to all members of society, including both VR recipients and non-recipients. Relevant recipient outcomes include labor market outcomes, such as employment and wages, and harder-to-measure outcomes that account for the overall welfare of the recipient, such as the ability to live independently. Non-recipient outcomes include increases in family or caregiver wages that come with the individual's independent living skills as well as reductions in recipient interactions with the social welfare or criminal justice systems that indirectly affect non-recipients more broadly.

No single solution to the counterfactual outcomes problem is valid in all settings. The solution to be used depends on the context of the evaluation being conducted and the available data, but all require making assumptions about the conditions under which a measure constructed from observed data is a credible approximation of the outcome in the counterfactual state.[4] These assumptions are often referred to as identifying assumptions or an identification strategy; that is, the assumption used to recover or "identify" counterfactual outcomes and thus the (usually causal) parameter the evaluator is interested in for policy reasons (e.g., the benefits created or resulting from participation in a VR program). The credibility of the program evaluation and whether the estimates derived are useful or misleading hinges on how plausible these assumptions are in the given context.

As an illustration, Bua-Iam and Bias (2011) calculate the ROI for West Virginia's VR program. To do so, they estimate program benefits as mean labor market outcomes (e.g., gross wages, taxes paid) in the "treated group": individuals who received VR services. They do not compare this outcome with any "untreated group" that did not receive VR services and thus serves as proxy for what would have happened to treated individuals in a counterfactual state in which they did not receive VR services. In doing so, they note that because of the special nature of the population studied, "it is not realistic to use individuals who did not receive services as an accurate control group." While concerns about the challenges of finding a realistic comparison group for VR service recipients are warranted (Bua-Iam et al., 2013; Dean & Dolan, 1991a), Bua-Iam and Bias (2011) make a strong assumption. By not including a measure of what the outcomes would have been for VR clients, the research design implicitly assumes a counterfactual state in which those who receive VR services would have had no earnings in the absence of the program. While this may have been the case for some individuals, the assumption is unlikely to hold for all individuals, and thus it strains credibility to interpret the findings as a causal effect of VR.

As further contrast, we proceed by first discussing descriptive estimators that recover correlations and should not be interpreted as causal. Then, we review different program evaluation designs that can be used to estimate the impact of VR services and, ultimately, the ROI of a VR program: single-difference (cross-sectional or before-after), difference-in-differences, experimental, instrumental variables, and structural estimators. Several systematic reviews indicate that these techniques are common in the VR literature, albeit to varying degrees (Saunders et al., 2006; Pruett et al., 2008; Fleming et al., 2013; Lenz et al., 2014; Boeltzig-Brown et al., 2017; Lund & Cmar, 2019; Phillips et al., 2021).[5]

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[4] See Bruyére and Houtenville (2006) for an overview of potential data sources and Stern et al. (2019) for a discussion of issues surrounding data in the VR context.

[5] Fleming et al. (2013), Boeltzig-Brown et al. (2017), and Lund and Cmar (2019) focus their reviews on studies related to public-sector VR provided by state agencies. Saunders et al. (2006), Pruett et al. (2008), Lenz et al. (2014), and Phillips et al. (2021) define VR more broadly (e.g., by also including studies of services provided by nonprofits or in a rehabilitation hospital or long-term care setting).

We want to underscore the fact that no single research design will apply in all settings, and there may be situations in which the counterfactual outcomes problem cannot be fully addressed (Manski, 2007). Since the data alone cannot resolve the counterfactual outcomes problem, program evaluations must rely on identifying assumptions, and the results are sensitive to these assumptions. In reviewing these identification strategies, we reiterate that the credibility and validity of each approach is always context-dependent and based on whether the underlying assumptions (whether they are stated or left implicit) are reasonable in the context of the study.[6] The job of the researcher is to be clear about the research design and transparent about the assumptions being made. Readers and policymakers must determine whether those assumptions are reasonable, acknowledge relevant limitations, and use only findings based on plausible assumptions to inform their decisions.

2.2.1 Descriptive Designs: Correlational Estimators

Descriptive estimators do not estimate causal impacts. Rather, they report means of or correlations between different variables of interest. The former can be thought of as stating a fact, and the latter makes a comparison: the extent to which the values of those variables tend to occur together. Determining these correlational relationships requires only covariation between the variables. With covariation, we can conduct statistical inference. However, without a plausible way to approximate a comparison between a realized and a counterfactual outcome (in addition to the covariation), we cannot go beyond descriptive analyses to draw a causal inference.[7] This is the distinction the well-known adage makes: Correlation does not equal causation.

Given these limitations, we begin by discussing descriptive estimators for two reasons. First, because they are the most common type of empirical analyses in the VR literature. Boeltzig-Brown et al. (2017) review over 500 studies that evaluate VR programs and report that "most studies were descriptive or correlational in nature rather than explanatory." Lenz et al. (2014) find that the fraction of articles in their review that use descriptive designs is increasing over time. Second, because

[6]We also note that since these identifying assumptions are made about fundamentally unobservable counterfactuals, well-meaning people can disagree as to whether they are plausible in the given context. Also, science is not static, and there are methodological innovations over time. Techniques that were once favored because they made seemingly reasonable assumptions may be shown to have been flawed or superseded by improved approaches that are more palatable.

[7]Correlational estimates can be thought of as equivalent to the sum of the true causal effect, bias, and statistical noise. The bias results from a systematic deviation between the true but unobserved, counterfactual outcome and proxy used for that outcome. The proxy may be explicitly acknowledged by the researcher or left implicit. Statistical noise results from using a single sample from the population to estimate a parameter of that population because using different samples will result in different estimates of the same parameter.

descriptive estimators are useful in three ways, despite their limitations: they provide an account of the state of the world, can be used to make predictions, and can inform causal analyses (Bueno de Mesquita & Fowler, 2021).

Descriptive analyses may have intrinsic interest because they provide insights into broader trends and patterns in the data (Boeltzig-Brown et al., 2017). For instance, Houtenville and Bach (2024) report the number and fraction of people in the U.S. with a disability over time. These counts and percentages are important descriptive facts because they inform the size of the population VR agencies serve.[8] The authors also correlate disability with different variables of interest such as education, employment, and poverty measures. From both practical and policy perspectives, it is undoubtedly important to know that individuals with disabilities are 14.4 percentage points more likely to live in poverty. But this correlational evidence does not tell us why individuals with disabilities are more likely to live below the poverty line. It could be that disability causes poverty (e.g., through limited employment opportunities) or that poverty causes disability (e.g., because of lack of access to healthcare). We cannot say one way or the other based on solely correlational evidence.

One area in which correlations are sufficient is for making predictions. This can be done relatively straightforwardly with linear regression models or by using more complex specifications such as regularization models or decision trees. The techniques and approaches used to do so are sometimes referred to as machine learning (ML) algorithms. Rather than using program evaluation techniques to determine the effect of a policy on an outcome, as is the main focus of this chapter, ML models attempt to uncover generalizable patterns in the data they are trained on (Mullainathan & Spiess, 2017). They then use those recovered patterns to predict what will happen out-of-sample.[9] Why a particular variable is related to the outcome of interest is not important to the overall generalizable pattern, and one should not interpret the direction or magnitude of individual effects or variable importance measures in ML models. Applications of ML techniques to the VR field are infrequent relative to the use of program evaluation techniques (Hill et al. (2022) is a recent exception).

Descriptive studies can also serve as the building blocks of subsequent causal analyses by informing the researcher and the reader of important variation and patterns in the raw data. For instance, in a series of recent papers that evaluate VR services for clients with vision impairments, Clapp et al. (2020a) is a precursor analysis to the full structural model described in Clapp et al. (2024a) that estimates the causal effect of VR services on the labor market outcomes of individuals with blindness or vision impairments. As we detail in Sect. 2.2.6, the model in Clapp et al. (2024a) builds on existing theory and models of VR's impacts on individuals with

[8] So important, in fact, that the first sentence in Chap. 1 reports these statistics.

[9] Doing so requires choosing model flexibility to optimally trade off two competing factors in the in-sample model fit that determine prediction accuracy in the out-of-sample data: bias and variance (James et al., 2023).

other types of disabilities. But the analysis in Clapp et al. (2020a) illustrates how the model needs to be modified to account for the unique features of the visually impaired sample (e.g., that not enough individuals are served by any single state agency to use the preexisting framework, so the researchers modify the model to accommodate pooling observations from three states).

More generally, descriptive studies can guide causal analyses by illuminating previously unexamined areas that are ripe for future causal analyses. For instance, Ne'eman and Maestas (2023) document interesting patterns in the employment of individuals with and without disabilities during and after the COVID-19 pandemic. Most interestingly, they show that individuals with disabilities returned to the labor force during the pandemic recovery period faster than those without disabilities. Determining what caused this disparity remains an open question, but their study provides suggestive evidence regarding potential causal explanations that can be tested in future work.[10]

Similarly, Briscese et al. (2024) provide descriptive information on take-up of tax-advantaged Achieving a Better Life Experience (ABLE) accounts that allow individuals with disabilities to save more than a resource limit on the financial assets of those who receive Supplemental Security Income (SSI) benefits. This "Wave 1" study (List, 2020) is an exploratory analysis that can guide and inform research on a nascent program that is not yet offered in all 50 states.

Descriptive analyses are informative and valuable, but we must acknowledge their limitations. Dean et al. (2014) do so explicitly: "While these results may suggest that VR programs can effectively reduce SSDI/SSI roles for certain subgroups, we caution against drawing this type of causal conclusion from the evidence presented in this paper. Rather, this analysis is largely descriptive, and does not formally address the fundamental methodological problem involved in drawing such inferences from observational data." Unfortunately, this type of acknowledgment of the limitations of the analysis is often left implicit in the literature, so the onus of determining how to interpret findings and use them appropriately to inform decisions is left to the reader.

2.2.2 Evaluation Designs: Single-Difference Estimators

We next turn to estimators that attempt to recover causal effects. Cross-sectional estimators are a straightforward way to estimate the impact of a program or policy. They are calculated as the difference in the *sample means* of realized outcomes for

[10]For instance, Ne'eman and Maestas (2023) show that individuals with disabilities working in occupations that accommodate remote work saw their relative rate of employment increase faster than those working in non-remote occupations during the pandemic recovery period. This finding is consistent with the pandemic causing an increase in the feasibility of remote work accommodations, which benefited individuals with disabilities. However, the evidence provided does not test this hypothesis directly and cannot be used to assert that remote work causes an increase in the employment of individuals with disabilities.

treated and untreated individuals at a given point in time. Because they are relatively easy to calculate, they are frequently applied by academics and practitioners alike. Boeltzig-Brown et al. (2017) find that 35.5% of the studies they reviewed used cross-sectional estimators.

Unfortunately, this seemingly intuitive estimator may come with drawbacks regarding the credibility of the underlying assumptions. By comparing a group of individuals who receive VR services with an untreated group of individuals that do not, a cross-sectional research design assumes that the only difference between the two groups is due to the VR services one group received. This is a strong assumption if there are preexisting differences between the groups that are not accounted for (e.g., in age, sex, disability status, severity of disability, or motivation to find a job). Since these types of differences often lead individuals to self-select into the treated or untreated group based on what is optimal for them, violations of this assumption are often referred to as selection problems. If this is the case, the cross-sectional estimator recovers the sum of both the true effect of the program and bias arising from the effects of the other confounding differences between the groups that influence the outcome.

A common approach to address this concern is by statistically controlling for observed factors such as age, sex, and disability status using regression or matching techniques (Hollenbeck et al., 2005; Hollenbeck & Huang, 2006, 2008, 2016; King et al., 2008; Hollenbeck, 2009; Heinrich et al., 2013; Renfro et al., 2013; Smith et al., 2015; Andersson et al., 2024).[11] Using a cross-sectional estimator requires the assumption that the researcher has statistically controlled for, or conditioned on, all confounding differences between the treated and untreated groups that cause bias.[12] This assumption is problematic if there are unobserved drivers of selection that either are not measured (e.g., severity of disability) or cannot be measured (e.g., motivation). Regardless of the reason, if these possible differences between groups cannot be observed, they cannot be controlled for, and the cross-sectional estimator will be biased.

Assuming that individuals who *choose* treatment are a good counterfactual for those who do not (i.e., there is no selection) is often a strong assumption in the VR context. An obvious potential solution to mitigate the problem is to analyze only the group of individuals who receive treatment but doing so at two points in time. This is a second type of single-difference estimator known as a “before-after” or “pre-post” estimator. Although less popular than cross-sectional estimators, before-after estimators are still common in the VR literature and are gaining in popularity. Boeltzig-Brown et al. (2017) report that 6.5% of the papers in their review of studies from 1970 to 2008 use before-after estimators. In a review that spans a subsequent period, from 2007 to 2018, Phillips et al. (2021) report that the fraction of studies using

[11] For more detail on matching techniques, see Heckman et al. (1997, 1998), Dehejia and Wahba (1999, 2002), and the discussion in Smith and Todd (2005a, b) and Dehejia (2005).

[12] If so, VR service receipt is said to be conditionally randomly assigned even if it is selected unconditional on covariates.

before-after estimators increased to 29%. As an example of this technique, Harper-Anderson (2018) uses a pre-post approach to estimate the effects of the Workforce Investment Act (WIA) program and the Trade Adjustment Assistance (TAA) program, which are then used as inputs for ROI analysis of the returns to those programs.

Before-after estimators compare the mean outcomes of treated individuals before and after they receive VR services. This research design uses the pre-treatment outcome as a proxy for what the outcome would have been in a counterfactual post-treatment period without treatment. It requires assuming that there are no time-varying confounders; that is, the only thing that changed from the before period to the after period is that the individuals being studied received VR services. If this is not the case, for instance, due to changes in the individual's health that led her to seek VR services or changes in the economy that affected her job prospects, the before-after estimator will be biased by the effects of these confounding changes over time that also influence the outcome.

In Chap. 6, we will demonstrate how these single-difference estimators can be calculated using data from North Carolina's VR agency. In doing so, we contrast cross-sectional and before-after estimators with a more commonly used estimator that requires weaker assumptions that can be more plausible. We introduce that estimator in the next section.

2.2.3 Evaluation Designs: Difference-in-Differences Estimators

If a researcher has access to longitudinal or panel data that vary both cross-sectionally and temporally (e.g., observations on individuals at multiple points in time), she can calculate a difference-in-differences (DID) estimator. Perhaps because of this additional data requirement and despite early examples (Dean & Dolan, 1991b; Dean et al. 1999), DID estimators are less common in the VR literature. They account for only 0.7% of estimators used in papers reviewed by Boeltzig-Brown et al. (2017) and 10% of those subsequently reviewed by Phillips et al. (2021). Coupled with a wave of relatively recent DID papers, this provides anecdotal evidence that the use of this technique is becoming more prevalent in the literature (Uvin et al., 2004; Hollenbeck & Huang, 2006, 2016; Hollenbeck, 2009; Wilhelm & Robinson, 2013; Maryns & Robertson, 2015; Clapp et al., 2020a, b).

DID estimators are double-difference estimators. They combine elements of cross-sectional and before-after estimators by comparing time trends in the outcome for treated and untreated individuals. Doing so relaxes the no-selection and no-time-varying-confounders assumptions to what is known as the common or parallel trends assumption (Roth et al., 2023).[13] Instead of assuming that the observable *mean* in the untreated *comparison* group is equal to the unobserved counterfactual *mean* in the

[13]The DID estimator also requires the assumption that there is no anticipation; that is, VR services have no effect on those who receive them before they are treated. Otherwise, changes in the outcome of interest before and after service receipt reflect both the true, causal effects of VR and bias from a confounding anticipatory effect in the pre-service period.

treated group (as in a cross-sectional estimator), the parallel trends assumption requires that the observable *trend* in the untreated *comparison* group is a proxy for the unobserved counterfactual *trend* in the *treated* group. In other words, the untreated group reveals the counterfactual trend in the employment outcome for VR clients.

Because this assumption pertains to trends, not means, the parallel trends assumption can be valid even if individuals self-select into VR service receipt based on persistent confounding differences between the groups. It is also valid if there are time-varying confounders (such as changes in labor market conditions), so long as those changes affect both groups equally. The parallel trends assumption is only violated if there is time-varying-selection bias: time-varying factors that differentially affect treated and comparison groups. As a general example, this would occur if macroeconomic changes in the labor market affected VR recipients with a certain disability status differently than those in the comparison group with a different status who did not choose services.[14]

While this assumption about how an unobservable trend would behave in a counterfactual state is fundamentally untestable, it is common for researchers to test the validity of this assumption by checking for the presence of parallel trends in pre-intervention outcomes. This is often referred to as checking for "parallel pre-trends." The intuition is that if the treated and comparison groups were on parallel paths before intervention, it is suggestive evidence that they would have continued that way in the absence of the intervention.[15]

The plausibility of the parallel trends assumption and the credibility of a DID estimator depend on the untreated comparison group the researcher chooses. Potential comparison groups include those who apply for VR services but withdraw from the program after being determined eligible (i.e., Status 30 closures) and those who are screened out by a counselor (e.g., persons determined to be too severely disabled to benefit from VR services or not having a sufficiently severe disabling condition). A concern common to these comparison groups is that the service decision may depend on either the client's or the counselor's beliefs about the client's future labor market prospects. If so, this is a potential form of time-varying selection bias that would bias DID estimates of the impacts of VR and the subsequent ROI calculation.[16]

[14]We provide an example of the DID estimator in Chap. 6, Sect. 6.1.1.1 See Chap. 18 of Huntington-Klein (2021) for a graphical depiction of the estimator and discussion of the parallel-trends assumption.

[15]This is commonly done by plotting trends in the raw data (for an example, see Figs. 3.1 and 3.2 in Chap. 3) or by estimating an event study (for an example, see Figures 1 through 7 in Biasi et al., 2021). An event study is a more general version of a DID model that splits the single post/treated DID effect into differential DID effects over time. For an overview of the technique, see Miller (2023).

[16]Much like with cross-sectional estimators, we can address time-varying selection based on observable factors by conditioning on those factors (Hollenbeck & Huang, 2006, 2016; Maryns & Robertson, 2015).

2.2.4 Evaluation Designs: Experimental Estimators

Instead of using a DID approach, an alternative way to address the selection problem is to use an experimental estimator. The experimental approach requires changing how the data are created. Since selection bias occurs when an individual chooses whether to receive VR services, in conducting an experiment or randomized controlled trial (RCT), the researcher randomizes individuals into service receipt. Individuals who are randomized into receiving services constitute the treatment group. The control group consists of those who randomly do not receive VR services. Due to randomization, individuals in the control group reveal what would have happened to the treatment group had they not received services. This occurs because experimental randomization negates self-selection and ensures that any preexisting differences are balanced across the groups.[17] By comparing outcomes between treated and control groups, confounding differences between the groups that influence the outcome are differenced out, and only the true effect of the VR intervention remains.

Experiments are often referred to as the "gold standard" in program evaluation techniques (List & Rasul, 2011). Although well-known experimental evaluations of job-training programs have been performed (LaLonde, 1986), in the VR context, several legal/ethical and practical considerations prevent this approach from being widely implemented.[18] First, VR clients are a vulnerable population, and state VR agencies are legally required to provide services to eligible individuals (Dean & Dolan, 1991a; Pruett et al., 2008; Johnston et al., 2009; Yin et al., 2023).[19] This makes it practically impossible to randomize whether an individual receives state-agency VR services. Second, VR agencies provide individualized plans that are jointly determined by the client and her counselor. The individualized nature of VR renders blinding difficult (Johnston et al., 2009) and may lead to bias (known as Hawthorne effects) if clients or counselors alter their behavior because they know they are part of an experiment (Levitt & List, 2011). The fact that clients and counselors choose a plan that consists of a bundle of services also makes conducting such experiments difficult. This means that, even if one is able to randomize whether clients receive (their unique bundles of) services, it would be difficult to determine which particular service or combination of services caused the result (Dijkers, 2009; United States Government Accountability Office (U.S. GAO), 2009; Boeltzig-Brown et al., 2017).

[17] This applies only if individuals comply with their experimental assignment and thus prevent selection after randomization.

[18] See Froehlich et al. (2019) for a general discussion of ethical issues relevant to VR ROI research.

[19] The only exception is when demand for VR services exceeds what can be supplied given the agency's budget. In such cases, the state will operate under an Order-of-Selection (OOS) regime to ration services to applicants (Ipsen & Stern, 2020).

Finally, other concerns are common to all experiments, such as noncompliance with experimenter randomization and the possibility that experimental results may not generalize to individuals, contexts, interventions, and time periods that differ from those in the experiment; that is, they may not be externally valid (List & Rasul, 2011; List, 2020). Dean and Schmidt (1999) posit that attrition may be a particular concern in the VR context because of the long durations and variable requirements of VR treatments.

In light of these hurdles, it is not surprising that only 2.4% of the studies reviewed by Boeltzig-Brown et al. (2017) use an experimental design.[20] The Individual Training Accounts (ITA) experiment that randomized Workforce Investment Act (WIA) recipients to different levels of counselor guidance regarding the use of ITA funds is a notable application of experimental techniques in the disability literature (D'Amico & Salzman, 2004; Perez-Johnson et al., 2011; Decker & Berk, 2011). So is the Ameri et al. (2018) field experiment, which randomized whether researcher-generated job applications disclosed that the applicant had a disability and found that doing so caused a 26% decrease in the number of callbacks received.

2.2.5 Evaluation Designs: Instrumental Variables Estimators

An alternative to using researcher-generated random assignment of individuals to receipt of VR services is to exploit a naturally occurring source of randomization. This is done by using an observed variable, called an instrument, that influences whether individuals receive VR services but is assumed not to directly influence the outcome being studied. Boeltzig-Brown et al. (2017) do not specifically categorize whether the studies in their review use instrumental variables (IV) estimators, but they report that 2.4% of the studies reviewed use quasi-experimental techniques. IV estimators are an example of such a technique, and thus this provides an upper bound on the use of IV estimators in the VR literature.

IV estimators are more common in the economics literature. While instrumental variation can come from a number of sources such as natural phenomena such as weather (Moretti & Neidell, 2011; Schlenker & Walker, 2015; Deryugina et al., 2019; Anderson, 2020; Heft-Neal et al., 2020; La Nauze & Severnini, 2021), government policies (Deshpande, 2016; Ebenstein et al., 2017; Deshpande et al.,

[20] In contrast, 57% of the studies reviewed by Phillips et al. (2021) report experimental estimates. While this difference may be due, in part, to a change in methodological approaches over time, it is most likely driven by differences in the sample selection criteria across the two systematic reviews. Boeltzig-Brown et al. (2017) review studies of state-agency-provided VR, the majority of which (94.5%) focus on labor market outcomes. In contrast, Phillips et al. (2021) conduct a similar but broader review, in that their search criteria do not restrict their sample to studies of state-federal VR system evaluations. Also, a smaller fraction of the studies reviewed (67%) evaluate employment outcomes. Thus, many of the interventions in the Phillips et al. (2021) review occur in contexts that better lend themselves to an experimental approach (e.g., cognitive behavioral therapy interventions provided to relatively small groups in a community mental health setting).

2021; Deshpande & Mueller-Smith, 2022; Gelber et al., 2023; Bishop et al., 2023) or even experiments themselves (Heckman, 1996; Chetty et al., 2016; Brandon et al., 2022; Christensen et al., 2023; Bhatt et al., 2023), the most relevant type of instrument for the VR context is known as an examiner design (Kling, 2006; Doyle Jr, 2007, 2008; Maestas et al., 2013; French & Song, 2014; Dahl et al., 2014; Aizer & Joseph Jr, 2015; Autor et al., 2019; Gross & Jason Baron, 2022; Arnold et al., 2022; Baron et al., 2024; Black et al., 2024).[21]

Dean et al. (2015, 2017, 2018, 2019), Schmidt et al. (2019a, b), and Clapp et al. (2024a) (DPSSC hereafter) use examiner-design IV techniques as part of a broader structural model that allows categories of VR services to have different effects on labor market outcomes.[22] As a similar but more straightforward example, Yin et al. (2023) estimate the net impact of VR services using the propensity of a client's quasi-randomly assigned VR counselor to provide services to their clients via an Individualized Plan for Employment (IPE) as an instrument for whether an individual receives VR services.

In order to understand how an IV research design identifies the effects of VR, it is helpful to think of how it works in relation to an experimental estimator. Consider a simplified but intuitive example: there are only two counselors at a hypothetical agency. One counselor assigns all of her clients to receive VR services, and the other counselor provides VR services to none of her clients. If clients are randomly assigned to these two counselors, they are effectively participating in an experiment when they apply for VR services via their counselor assignment.

In reality, there are two important differences relative to this simplified example. First, clients may not be perfectly randomly assigned to counselors. To address this issue, the researcher may again use regression analysis to statistically control for observed factors that influence assignment. Thus, even if clients are not fully randomly assigned to counselors, assignment may be conditionally random. Second, counselors are not so extreme in their assignment propensities. An experiment based on perfect compliance is essentially the extreme or limiting case of an IV.

2.2.6 Evaluation Designs: Structural Model Estimators

Given concerns about the validity of the parallel-trends assumption, which is required for a credible DID analysis, and barriers to conducting experimental analyses in the VR context, our line of research described in this book (also see DPSSC) combines the basic structure of the DID model with IV techniques to estimate the impact of VR services under more plausible assumptions. Akin to statistically controlling for important factors as is common with other identification

[21] For a technical treatment of IV techniques, see Heckman and Vytlacil (2007a, b). For a less technical treatment that targets applied researchers, see Cornelissen et al. (2016). For details about examiner design IVs and their validity, see Frandsen et al. (2023).

[22] We discuss these research designs in more detail in the next section.

strategies, these assumptions essentially render the previously detailed IV assumptions more reasonable by conditioning on pre-treatment trends that may influence which services a client receives.

We develop and estimate a structural model that jointly describes how different types of treatments are selected and how labor market outcomes for VR clients are determined.[23] In contrast to the approach used in much of the program evaluation literature of estimating only the net effect of the VR program as a whole (e.g., Yin et al., 2023), our VR-ROI model allows different categories of VR services to have unique effects.[24] This approach is consistent with the data: the majority of VR recipients receive more than one type of service. It also provides greater insights into how VR works and the efficacy of individual services.

There is another important contrast between the VR-ROI model and more traditional approaches. We jointly estimate both employment and earnings instead of treating them as independent outcomes and estimating two separate models, as is common in the literature. This modeling approach yields a more accurate depiction of reality, enables us to use more of the information in the data to identify model parameters, and makes it straightforward to interpret our earnings effects as being conditional on employment.[25]

Across numerous articles, we have applied the VR-ROI model in a variety of contexts. Tables 2.1 and 2.2 summarize these papers and highlight key elements of the sample frame, model specification, and results across papers.[26] They primarily

[23] Chap. 3 provides a full description of the VR-ROI model. Structural models are not common in the VR literature but are more often used by economists to evaluate training (and other) programs (e.g., Aakvik, 2003; Frölich et al., 2004; Aakvik et al., 2005).

[24] Leonard et al. (1999) and Giesen and Hierholzer Lang (2018) are exceptions to the common approach in the literature; they estimate the effects of individual services, albeit using different methods.

[25] As with most of the program evaluation literature, we do not model VR client application decisions (Heckman et al., 1999; Imbens & Wooldridge, 2009). Thus, the model parameters we estimate are conditional on program application. The external validity of our findings is limited to the extent to which individuals who seek VR services differ from those who do not. If so, this would be particularly limiting when trying to identify the effects of policies that scale the VR program or otherwise impact (the distribution of unobserved factors relevant to labor market outcomes in) the pool of applicants. See List (2020, 2022) for general discussions of external validity and scaling. Dean et al. (2017) test whether the decision to apply for VR services is exogenous. They find no evidence of selection on unobservables in the VR application decision. Ipsen and Stern (2020) model the decision to apply for VR services and find that individuals who live in rural areas are less likely to apply than their urban counterparts.

[26] In addition to the papers listed in the table, we have written several papers that extend the VR-ROI model in different ways. Clapp et al. (2020b) test whether access to quality public paratransit affects the efficacy of VR services. Clapp et al. (2024b) illustrate the shortcomings with using taxpayer, instead of social, measures of return on investment for VR programs by calculating and contrasting both measures for individuals in the Dean et al. (2015, 2017, 2018) samples. Clapp et al. (2024c) adds learning and intellectual disability classification decisions to the VR-ROI model to show how racial differences in the way disabilities are classified bias estimates of the racial employment and earnings gaps.

Table 2.1 Summary of VR-ROI sample frame

Article citation	Disability	Agency	Cohort application year	Sample size	Years
Dean et al. (2015)	CI	VA DARS	1988; 2000	1009; 1907	1984–2009
Dean et al. (2017)	MI	VA DARS	2000	1555	1995–2008
Dean et al. (2018)	PI	VA DARS	2000	2421	1995–2008
Dean et al. (2019)	TY	VA DARS PERT	2000	3073	1995–2008
Schmidt et al. (2019a)	CI, MI, PI	VA DARS; MD DORS	2007	4121; 5197	2004–2012
Clapp et al. (2024a)	BVI	VA DBVI; MD DORS; OK DRS	2007	1778	2004–2012

Notes: Virginia DARS = Department for Aging and Rehabilitative Services. PERT = Post-Secondary Education and Rehabilitation Transition program. Maryland DORS = Division of Rehabilitation Services. Virginia DBVI = Department for the Blind and Vision Impaired. Oklahoma DRS = Department of Rehabilitation Services. CI = Cognitive Impairment. MI = Mental Illness. PI = Physical Impairment. TY = Transition-aged Youth. BVI = Blind or Vision Impaired

Table 2.2 Summary of VR-ROI specifications and results

Article Citation	Outcome variables	# VR service types	Long run	Median annual ROR
Dean et al. (2015)	Emp, log Earn	6	8+ qtrs	20%
Dean et al. (2017)	Emp, log Earn, SSDI	6	8+ qtrs	19%
Dean et al. (2018)	Emp, log Earn	6	8+ qtrs	169%
Dean et al. (2019)	Emp, log Earn	6 + PERT	8+ qtrs	285%
Schmidt et al. (2019a)	Emp, log Earn	7	8+ qtrs	0%–18%
Clapp et al. (2024a)	Emp, log Earn	9	10+ qtrs	NPV < 0

Note: The six main VR services are: *Diagnosis & Evaluation*, *Training*, *Education*, *Restoration*, *Maintenance*, and *Other services*. The study with seven VR services replaces *Other services* with *Job Placement* and *Job Supports*. The study with nine VR services adds *Assistive technology* and *Orientation & mobility*. The Dean et al. (2015, 2017, 2018) studies report ROR estimates based on different cost estimates. For consistency, the table lists the estimates based on the upper bound of fixed costs, which results in more conservative RORs

differ by the disability type of VR clients being served: cognitive impairments (CI; Dean et al., 2015); mental illness (MI; Dean et al., 2017); physical impairments (PI; Dean et al., 2018); or blindness/vision impairments (BVI; Clapp et al., 2024a). Dean et al. (2019) focus on youth with disabilities and the Post-Secondary Education and Rehabilitation Transition (PERT) program.

They also differ according to the agencies providing services and the years being evaluated. Dean et al. (2015, 2017, 2018, 2019) analyze data for the 2000 cohort of individuals applying for services from Virginia's general VR agency, the Department for Aging and Rehabilitative Services (DARS).[27] Schmidt et al. (2019a) use 2007 applicant cohort data from two agencies: VA DARS and the Maryland Division of Rehabilitation Services (DORS). They separately estimate models for individuals with cognitive impairments, mental illness, and physical impairments (for a total of six agency-disability-specific sets of estimates). Due to the relatively small numbers of individuals with blindness/vision impairments, Clapp et al. (2024a) modify the specification to accommodate jointly estimating the model using pooled data on 2007 applicants from three agencies: the Virginia Department for the Blind and Vision Impaired (DBVI), Maryland DORS, and the Oklahoma Department of Rehabilitation Services (DRS).

All analyses examine the effects of VR services on two labor market outcomes: employment and conditional log earnings.[28] Following Dean et al. (2002), papers based on the 2000 applicant cohorts model the effects of the receipt of six categories of VR services: *Diagnosis & Evaluation*, *Training*, *Education*, *Restoration*, *Maintenance*, and *Other Services*.[29] Analyses of 2007 applicant cohort data model seven or nine service types. Schmidt et al. (2019a) replace *Other Services* with *Job Placement* and *Job Supports*, and Clapp et al. (2024a) add *Assistive Technology* and *Orientation & Mobility* because they are specific to the population being analyzed.

Since individuals select into which VR services are optimal for them, the mean outcome for those who opted not to receive the service is unlikely to recover the missing counterfactual outcome. Thus, comparing outcomes between those who receive a service and those who do not is likely to yield a biased measure of the true effect of the service. In order to determine the true causal effect of each service, DPSSC use examiner-design IVs for each service: the proportion of the counselor's and the field office's other clients who receive the particular service are an IV for receipt of that service.[30]

All analyses separately estimate both the short-run and long-run effects of each of these services on each of the outcomes. Except for Clapp et al. (2024a), the short run is defined as the first eight quarters post-application, and the long run begins with the

[27] In addition, Dean et al. (2015) separately estimates the VR-ROI model on both the 1988 and 2000 cohorts of VA DARS applicants.

[28] Dean et al. (2017) also model the receipt of receipt of Social Security Administration (SSA) Disability Insurance (DI) and Supplemental Security Income (SSI) program benefits as a third outcome.

[29] Variable names are italicized for clarity. In addition to the six VR service categories, Dean et al. (2019) also model the receipt of PERT services. For descriptions of the types of services included in each of the categories, see Chap. 3, Sect. 3.2.

[30] Dean et al. (2019) also instrument for the receipt of PERT services with programmatic restrictions on the number of students who can enroll in PERT. All of the DPSSC studies impose the restriction that coefficients on the examiner IV first-stage counselor and field office effects do not vary across services.

ninth quarter after application.[31] In contrast to the short-run outcomes often analyzed in the literature and collected by the Rehabilitation Service Administration (RSA), DPSSC show that short- and long-term outcomes often differ. As a result, focusing exclusively on the effects of the VR program at closure or shortly after is likely to cause decision-makers to draw incorrect conclusions.

The final column of Table 2.2 summarize results of the DPSSC studies. Before discussing the findings from these analyses, we first provide additional detail on how they are calculated. So far, we have reviewed how benefits are calculated. But conducting a ROI analysis also requires estimating costs. We now turn our attention to costs.

2.3 Estimating the Costs of VR

In contrast to the estimation of VR benefits, estimating the cost of VR is relatively straightforward because there is no counterfactual outcome to consider. Costs can be calculated from realized service and administration expenditures data. This information is tracked by the state agency and reported on RSA-911 Case Service Reports. However, there are still important nuances in how one determines agency expenditures on each service provided. VR services are not all equivalent with respect to cost. They can be provided in a combination of ways: as a "purchased service," a "similar benefit," and/or an "in-house benefit." Purchased services are provided by an outside vendor and are paid for with agency funds. Similar benefits are procured and provided by other government agencies or not-for-profit organizations, and there is no cost to the VR agency for these services. In-house benefits are provided internally by employees of the VR agency.

How one estimates the cost of a given service depends on how the service was provided. Agency data directly report the amount paid for purchased services but not for similar benefits or in-house services. Unfortunately, expenditures on similar benefits are not tracked and cannot be included in our ROI calculations. To estimate expenditures on in-house services, DPSSC calculate total spending by the agency for all cases closed during the given fiscal year. Netting out purchased service costs and dividing by the caseload in the given fiscal year provides an estimate of both non-purchased service costs and fixed costs (e.g., administrative overhead).[32] Although this approach is likely to produce a noisy and possibly biased estimate of the true costs of providing in-house benefits, it is the best possible estimate of

[31] In Clapp et al. (2024a), the margin between the short and long runs occurs at ten quarters post-application because individuals with visual impairments require services for a longer period of time than those with other disabilities.

[32] This produces an agency-wide cost estimate. Since the DPSSC studies are disability-type specific, there's then an adjustment made by calculating the ratio of the agency's in-house expenditures to purchased service expenditures. That ratio is then applied to the total purchased service expenditures for each disability sample to estimate total in-house expenditures for the sample.

those costs that can be calculated given the constraints imposed by the lack of data.[33] In addition, to assess the sensitivity of ROI estimates to uncertain costs, several of the DPSSC papers report multiple ROI estimates based on different estimates of cost.

2.4 VR-ROI Model Findings

We defer a detailed explanation of how estimated benefits and costs are used to conduct ROI and rate of return (ROR) analyses to Sect. 3.2. Intuitively, these analyses compare the value of the benefits of services with the cost of providing the services, albeit in different ways. The ROI is the ratio of the present value of all future benefits to the cost. The ROR is the interest rate that equates the present value of those future benefits and the cost. With that basic background, we return to our review of the findings in DPSSC.

As summarized in the final column of Table 2.2, the results of the DPSSC papers are relatively consistent within the application cohort examined, but not across cohorts. This is despite the fact that they all use similar data and model specifications. For instance, papers based on 2000 applicant cohorts consistently find that VR has positive effects on the labor market outcomes of recipients, and annual RORs for median individuals in the samples in Dean et al. (2015, 2017, 2018, 2019) studies range from 19% (mental illness) to about 285% (PERT).[34]

In contrast, papers based on the 2007 applicant cohort find mixed effects. Schmidt et al. (2019a) find positive returns of VR for those with mental illnesses (in both Virginia and Maryland) and for those with physical impairments in Virginia. But the annual RORs for median individuals in half of the six agency-disability samples are zero (Virginia CI, Maryland CI, and Maryland PI). This occurs, in part, because some services have a negative effect on client labor market outcomes.[35] For 2007 cohort individuals with blindness or a vision impairment, the estimated effects of individual VR service categories are frequently negative. This results in estimated costs of VR that exceed the net present values (NPV) of estimated benefits, and thus RORs are not meaningful measures.

[33] Cost estimates would be biased if individuals with a given disability type (e.g., cognitive impairments or mental illness) systematically have more or less expensive in-house service costs relative to those with other types of disabilities. This is an issue because total expenditures are reported at the agency level and are not disaggregated by disability type.

[34] For instance, this means that half of the individuals with a mental illness had an annual ROR greater than 19% and the other half experienced a lesser return.

[35] Although not listed in the table, in addition to the significant heterogeneity in the effects of VR services across disability types and time periods evident in the table, all of the DPSSC papers find evidence of substantial variation in the efficacy of different VR services. Analyses that fail to account for these important details are likely to draw misleading inferences about the impacts of VR services.

As noted previously, for some services, we find negative estimated labor market effects. An open question is why the VR-ROI model estimates that the receipt of VR can have a negative effect for certain services and overall. Taken at face value, a negative effect estimate implies that VR services reduce the individual's productivity or in some other way reduce her employability and/or compensation in the long run. While no effect (i.e., neither helping nor harming the individual in the labor market) is plausible, it is unlikely that VR renders individuals worse off relative to the counterfactual in which they did not receive services. These issues are discussed more fully in Sect. 5.3.

There are two potential explanations for this finding: (1) either the model is mis-specified and produces biased estimates or (2) VR provides important non-market benefits that affect client decisions (but are not captured in the model).[36] A mis-specified model means that, despite the controls and quasi-experiment implied by the IV, the VR-ROI model is not able to estimate the appropriate counterfactual regarding what would have happened if individuals who were treated had not received VR services. It is not immediately obvious why this would be the case, nor what rather specific type of misspecification would manifest itself in some of the DPSSC sample frames but not others.

Important nonpecuniary benefits of VR service receipt (e.g., improved independent living skills) can result in negative estimated effects if individuals and their counselors are making service choices that optimize over these outcomes, and those choices deviate from what is optimal with respect to the labor market alone.[37] We do not observe information about non-labor market outcomes, so we cannot include these outcomes in the VR-ROI model. Boeltzig-Brown et al. (2017) report that this data restriction is common to the vast majority of all studies of state-agency-provided VR in the literature. Almost 95% focus on labor market outcomes. Yet, unstudied nonpecuniary outcomes are undoubtedly important. The long-range plan NIDILRR used to guide its sponsored-research priorities has three areas of emphasis (NIDILRR, 2024), and employment outcomes are only one of the three. The other two interconnected types of outcomes NIDILRR emphasizes (community living and participation; health and function) are distinctly nonpecuniary. Their importance in the lives of individuals with disabilities has also been demonstrated by the broader VR literature outside of the public VR agency context (Saunders et al., 2000; Fleming et al., 2013). Most telling, Balcazar et al. (2023) surveyed VR counselors

[36] See Sect. 5.3 of Clapp et al. (2024a) for additional discussion of the potential causes of negative estimated returns.

[37] As an intuitive example, assume that all individuals who are interested in augmenting their independent living skills, but not finding employment, choose a particular service with their counselors that is geared toward their goals. Relative to those who do not receive the independent living service, recipients will be less likely to be employed. The model will attribute the resulting negative estimate to the effect of the service. But it is actually the result of bias from using the labor market outcomes of non-recipients who have fundamentally different goals as the proxy for the counterfactual regarding what would have happened to recipients of the service had they not received the service.

about what they view as a successful outcome for their clients. The survey was open-ended, so counselors could list multiple outcomes. While the majority reported that the client gaining employment defined success, 33% also counted an independent living result as a positive outcome for clients. Educational placements were viewed similarly by 30% of counselors. Unsurprisingly given these views, 18% of counselors provided services with an independent living goal in mind, and 10% provided services with an educational goal in mind. Developing ways for VR agencies to track these non-labor market outcomes and incorporating them in the VR-ROI model is a compelling topic for future research.

2.5 Conclusion

Numerous methodological approaches can be used to estimate the returns to VR. All must find a credible solution to the counterfactual outcomes problem that makes it difficult to determine the causal impact of VR on the lives of individuals with disabilities. While no single approach is valid for all contexts, the credibility of the methodology and resulting estimates depends on whether the assumptions being made (either implicitly or explicitly) are plausible in the given context. When the identifying assumptions are reasonable, the estimates are convincing. When the assumptions fail to meet the standards of credibility, the evaluation and associated estimates it yields are likely to be misleading and should not be used to make decisions, evaluate programs, or inform policy.

While most analyses of VR programs use either single- or double-difference approaches and find large, positive returns to participating in the VR program, there are reasons to be concerned about the validity of these approaches. First, they often focus on short-run effects. DPSSC show that short-run effects fail to capture the full impact of VR in the lives of the people it serves. Second, the assumptions required to account for selection into treatment may not be plausibly satisfied, even after matching or conditioning on a robust set of control variables. Further, not all research using these techniques provides sufficient evidence to assess the plausibility of these assumptions (e.g., by demonstrating that pre-trends are parallel with an event study when conducting a DID analysis). Given the nature of how VR services are provided and that DPSSC demonstrate that selection plays an important role in inferences on the effect of VR services, the concern is that how clients and counselors make those decisions biases the results of cross-sectional (matching) and DID estimation approaches. Third, these simpler estimators are unable to account for heterogeneity in the effects of the services received and among the different types of VR clients being served. Failure to confront and overcome these limitations is likely to result in incomplete and potentially inaccurate understanding of how VR programs work.

In contrast, the VR-ROI model addresses all of these concerns. It evaluates the labor market outcomes of VR recipients over a long time horizon, both before and after the provision of services. It formally models selection into treatment and addresses concerns regarding selection using IVs that approximate an otherwise infeasible experiment and by conditioning on trends in pre-VR outcomes. It estimates the effects of specific categories of VR services instead of simply the average effects of different packages of VR services based on the design of the program specific to each individual served. Each DPSSC analysis focuses on individuals with a specific disability and tailors the VR-ROI model to account for the relevant details of that population and nuances regarding how they interact with the VR program. Although more complex to understand and estimate, the VR-ROI model mirrors the complexity of VR programs and their clients. These intricacies are important for understanding the full picture of how VR impacts the lives of the individuals it serves.

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Introduction to Rate of Return, Modeling, and Estimation

3

3.1 Introduction

The goal of our work is to measure the effectiveness of vocational rehabilitation (VR) services. Relative to earlier work, we add the following important features:

- We use long-term data and distinguish between short-term and long-term effects.
- We allow effects to vary by disability type, service type, and state.
- We control for other demographic, socioeconomic, and disability characteristics.
- We use a structural model to estimate the relevant effects and control for endogeneity problems.

Each of these issues is discussed in the rest of this chapter.

3.1.1 Effectiveness

The first issue is how to measure effectiveness. While there are many possible definitions of "effectiveness," the one used throughout this chapter is the increase (possibly negative) in the quarterly employment probability and earnings conditional on working. Later, we use these to compute the long-run net present value of the benefits of the program minus the costs. "Long-run" means how earnings change over a relatively long period of time. We use the long run based on the evidence in Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019), who show that short-run and long-run effects on employment and earnings are different. The net present value is the sum of discounted benefits (earnings) minus fixed and marginal costs. To do this, we need a measure of effectiveness and a way to estimate effectiveness. In this chapter, we describe our model, how to estimate the parameters of the model, and how to use them to estimate the return on investment (ROI) and rate of return (ROR).

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3.1.2 Heterogeneity

Our model has useful features for learning about the effectiveness of VR. First, we allow estimates of effectiveness to possibly differ for each disability group, service, and state. This type of variation is called heterogeneity. We allow for heterogeneity across disability groups and service effects because leaders across states have strong intuition that such heterogeneity is important and because we have found such heterogeneity to be empirically important in previous work (Dean et al., 2015, 2017, 2018, 2019; Schmidt et al., 2019; Clapp et al., 2022). For example, with respect to services, Dean et al. (2015) find that *Education*, *Diagnosis & Evaluation*, *Training*, and *Other Services*[1] are the most effective for people with cognitive impairments (as measured by long-run discounted benefits); Dean et al. (2017) find that *Training*, *Restoration*, and *Other Services* have the largest labor market effects for people with mental illness; and Dean et al. (2018) find that *Education* and *Restoration* have the largest effects for people with physical disabilities. We would not know this without allowing for heterogeneity across disability types and services.

In most of the literature (for example, Dean and Dolan, 1991; Dean et al., 1999; Heckman et al., 1999; Aakvik et al., 2005; Imbens & Wooldridge, 2009), only one group of recipients (usually aggregated over multiple disability types) is measured, and service receipt is binary: Either one receives services or not. Other papers (for example, Cook et al., 2005; Burns et al., 2007; Campbell et al., 2010; Cimera, 2010), focus only on one service type, one disability type, or both.

Dean et al. (2015, 2017, 2018, 2019) demonstrate the effects of ignoring heterogeneity: Each paper shows that the estimated labor market outcomes for those who received some service relative to those who received no service are quite different than the estimated effects when controlling for all sources of heterogeneity. For example, Figs. 3.1 and 3.2 from Dean et al. (2017), illustrate the effects of any "treatment" (the receipt of any VR service) relative to no treatment for people with mental illness in a 2000 application-year cohort on employment and quarterly earnings. These effects are quite different from those that include the effects of the different types of heterogeneity. Among other things, the shapes of the curves in Figs. 3.1 and 3.2 change when we control for other factors. In the rest of this analysis, we control for heterogeneity in disability type, service type, and observed demographic variables. We find that estimates vary significantly by disability type, service type, and observed demographic variables as a result.

Apart from its statistical importance, a powerful reason for including heterogeneity is that it is in sync with a core tenet of the VR program. Services are individualized to a participant's interests, skills, abilities, capacity, and informed

[1] In Dean et al. (2015, 2017, 2018, 2019), there are six service types: *Diagnosis & Evaluation*, *Training*, *Education*, *Restoration*, *Maintenance*, and *Other Services*. All references to service types are italicized. *Other Services* include interpretive services for deaf or foreign language, attendant care, peer counseling, tools and equipment, miscellaneous non-medical, and personal attendant services for payroll.

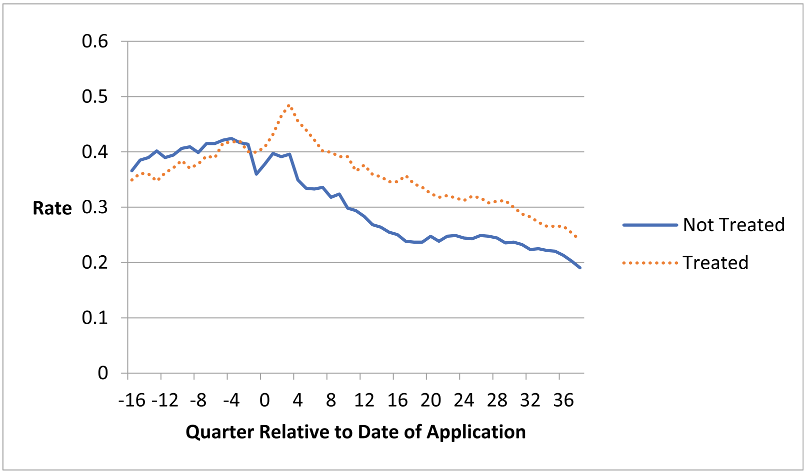

Fig. 3.1 Employment rates. Source: Dean et al. (2017), Fig. 5

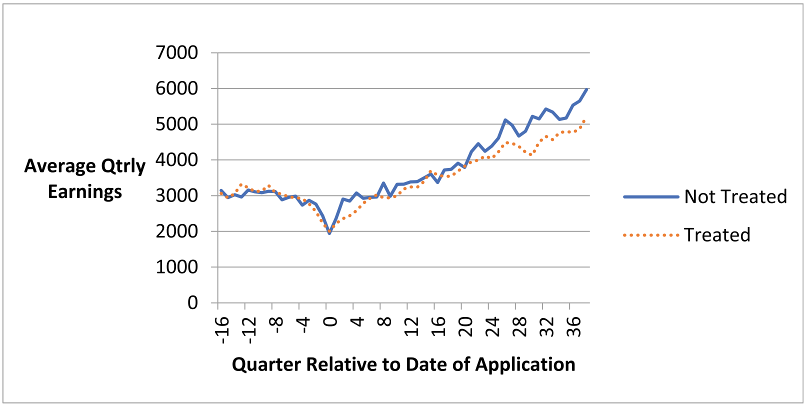

Fig. 3.2 Average quarterly earnings if employed. Source: Dean et al. (2017), Fig. 6

choice. Accordingly, the model with heterogeneity allows for variation in services for the participant. The ROI discussion in Sect. 3.4 is developed for each client and then can be aggregated to obtain an agency ROI number or distribution. Thus, the model is built with a focus on the individual client's ROI. At its core, the VR program is intended to serve and assist each client in achieving their identified employment and other goals, and thus the VR-ROI model is consistent with the VR program's focus.

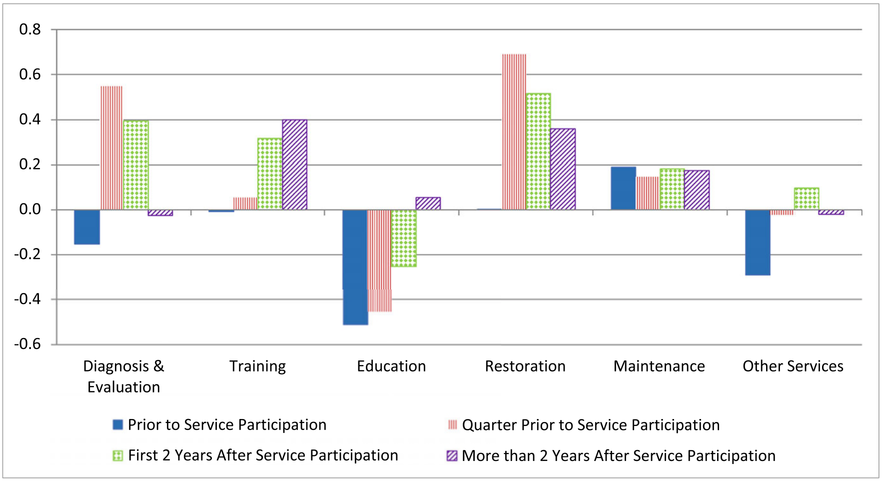

Fig. 3.3 DARS purchased service effects on employment. Source: Dean et al. (2018), Fig. 5

3.1.3 Short- and Long-term Evaluation

The second feature of our modeling approach is that we allow the short- and long-run effects of VR to differ. Most of the literature uses data from the national RSA-911 Case Service Report (see Chap. 4). Prior to the 2014 Workforce Innovation and Opportunity Act (WIOA), the only measure of labor market outcomes in these data were weekly earnings shortly after closure of an individual's case. Since WIOA's passage, the RSA-911Case Service Report also includes quarterly earnings for the first four quarters after the exit quarter. Both are problematic in that "closure" and "exit quarter" are not particularly meaningful concepts in the economic analysis and, more importantly, they have nothing to say about labor market outcomes in the longer run. Later in this chapter, we discuss the importance of including long-run effects. Here, we provide an example of the difference between short-run effects and long-run effects.

Figure 3.3 shows the labor market effects as estimated by our model on employment for each service type separately (one source of heterogeneity) for a 2000 application year cohort of people with cognitive impairments. Further, time relative to service application date, is decomposed into four periods. First, as shown by the solid blue bars in Fig. 3.3, are the effects on employment two or more quarters prior to application date (pre-application block). Clearly, subsequent services cannot affect pre-application employment and earnings. Rather, this block provides a relative starting point for those who subsequently receive the service. Second, as shown by the bars with vertical maroon lines, are the effects in the quarter prior to application date. In the literature, this is referred to as the Ashenfelter dip

(Ashenfelter, 1978) and is controlled for but not of real interest.[2] Third, as shown by bars with olive diamonds, is the effect in the short run, the first eight quarters after application (short-run block). Fourth, as shown by bars with purple diagonal lines, are the effects in the long run, nine or more quarters after application (long-run block). The measures of interest are differences in the height of the short-run block and the pre-application block and differences in the height of the long-run block and the pre-application block. We can see that short-run and long-run effects are quite different. For example, if an analyst measured a short-run effect using national RSA-911 Case Service Report data for *Education*, he would grossly underestimate the total effect of *Education*. On the other hand, the effects of *Diagnosis & Evaluation* and *Restoration* work in the opposite direction. Short-run effects are significantly larger than long-run effects.

3.1.4 Readily Available Data

The next feature of our modeling is that it relies on only readily available data to estimate model parameters. This is of great value because it facilitates analysis by other analysts and researchers.

The main sources of data are administrative agency data (including portions of the RSA-911 Case Service Report), merged quarterly earnings data from the state unemployment insurance (UI) office, and other data contained in national public data sets. The administrative data provide information, most importantly, on the demographics of each applicant, the type of disability he had, the county he lived in, and VR services he received. Frequently, only data on purchased services are available because of the data requirements of RSA. This lack of information on agency-provided services prevents us from measuring the effect of these services and biases our fixed cost estimates upward. We are humble in recognizing this problem.

The merged UI data provide information on the quarterly earnings of applicants for about 3 years prior to application and as many years as possible afterward. Agencies merge the two datasets using Social Security numbers.[3] National public data include information on county employment rates and other variables we will describe in more detail later in the chapter.

[2] Ashenfelter found that earnings were unusually low the quarter prior to starting a training program for econometric reasons not relevant to the true value of the program. Given that result, and assuming that it might also apply to VR programs, we exclude the quarter prior to application in the analysis.

[3] The agency removes Social Security numbers, names, addresses, and other personally identifiable information before transmitting the data via secure means.

3.1.5 Structural Modeling

The last feature of our approach is that we use structural modeling methods. This means that we create a mathematical model of individuals making relevant decisions that have later effects on outcomes of interest. For example, in this analysis, our model includes equations that determine which VR services one chooses and how those service choices, along with other characteristics of the individual, affect later labor market outcomes. We call each of the choices and each of the labor market outcomes dependent variables, and we call the variables affecting the dependent variables the explanatory variables. Finally, each equation has two parts that, when combined, equal each observed value of the dependent variable. The first portion is that explained by the explanatory variables and the second is the "error," or that which remains unexplained. A problem arises if any of the explanatory variables are correlated with dependent variables in other equations within the model. We call such an explanatory variable an endogenous variable, and its inclusion results in biased estimates. For example, service choices are endogenous in the labor market outcome equations because the errors in the service choice equations may be correlated with the errors in the labor market outcome equations. This could occur, for instance, if something important to each dependent variable is missing from each equation. That effect would be contained in each equation's error, capturing the effect of unobserved variables, and the errors would be correlated. An obvious candidate is motivation, which influences both participation in VR and success in the labor market yet is not readily measured and is thus excluded from both equations.

This approach is quite different from most VR evaluation papers in that, in most of the rest of the literature, there is no attempt to determine what VR services one wants to receive and, usually, there is no control for other characteristics of the VR service recipient.

There are advantages and disadvantages to structural modeling (as opposed to nonstructural modeling). The main advantages are as follows:

1. The structure of the model forces the modeler to interpret the results only in terms of the model. In particular, we cannot use possible explanations of results that rely on phenomena outside the structural model. For example, we cannot identify the effect of discrimination if it was not explicitly included in the model.
2. The estimates of a structural model can be used to evaluate specific policy proposals. In contrast, for the most part, nonstructural models have nothing to say about almost all specific policy proposals.
3. In a good structural model, one understands exactly what assumptions are being made. In nonstructural models, this is rarely the case. If the analyst does not understand the model assumptions, he does not understand the model.
4. Structural models are almost always significantly harder to estimate than non-structural models, and they usually require a significant amount of computer time to estimate. This makes it difficult to "cheat" by considering all possible explanations for the data, most of which do not make sense. One chooses a model and essentially sticks with it.

The main disadvantages are as follows:

1. Frequently, it is difficult to identify the mechanics of a structural model. This means that it takes much more time for a reader to understand how the model works and what parts of the results derive directly from the model structure versus those that are informed by the data.
2. As stated in (4) above, structural models are much more difficult to estimate than nonstructural models. This typically means that the analyst must have excellent programming skills and sufficient computing resources to estimate the model.

Overall, our approach to modeling has compelling advantages over alternative approaches. The heterogeneity we allow for renders our models somewhat more complex, but the complexity is itself important (Dean et al., 2015, 2017, 2018, 2019; Schmidt et al., 2019). We have a strong preference for structural modeling, though different analysts might come to different conclusions.

3.1.6 Summary Thoughts

Our focus on the long run has high value because what really matters is the long run. Imagine two worlds. In the first, a recipient earns more income from VR services for 2 years and then nothing after. In the second, the VR recipient earns more income, which lasts for the rest of the individual's life. The second world is better than the first, and, therefore, it is important to know how long VR service outcomes last (see also Butler et al., 1995).

However, there is a big cost to measuring long-run effects, because we must wait until the long-run effects can be measured. For example, if we are only interested in the first 2 years of effects, we must wait only 2 years to perform the analysis. On the other hand, if we are interested in the first 10 years of effects, we must wait 10 years to perform the analysis. Our strong sense is that, although most VR administrators are interested in long-run effects, they are not willing to wait long enough to get them. Multiple VR administrators have stated that it costs too much to measure long-run effects, because (1) they need answers much more quickly and (2) the VR environment changes so frequently that, by the time long-run estimates are available, they are no longer relevant. We sympathize with these arguments, and, in Chap. 6, we provide suggestions regarding which parts of our model can be ignored without harming the value of the results too much. However, we strongly believe that long-run estimates play an important role for VR administrators and policymakers. For example, having only short-run estimates frequently undervalues the benefits of VR services, and especially for youth transitioning from school to the labor market.

The next feature of our modeling is that we rely only on readily available data. In almost all cases, this has no downside. Of course, if some valuable data were restricted, we might want to use it even though it is not readily available. For example, in Dean et al. (2017), we used highly restricted data on the long-run receipt of Social Security benefits. These data enabled us to draw intriguing inferences

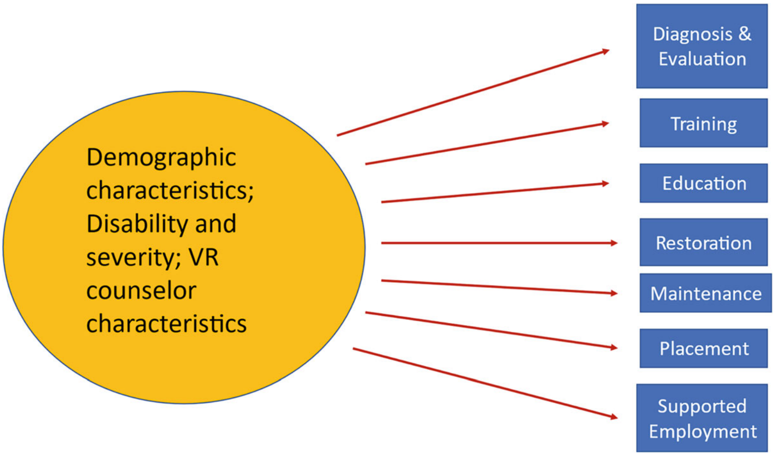

Fig. 3.4 VR choices

regarding the effects of VR receipt on Social Security benefit receipt. Still, as a guiding principle, using readily available data is the preferred approach.

3.2 Model

In this section, we provide an intuitive description of our structural model. A more precise and rigorous presentation is available in the two appendices. However, we have structured this book so that the reader can follow the discussion without ever having to refer to the appendices.

3.2.1 Service Choices

The first part of the model concerns the choices each VR applicant, in collaboration with her counselor, makes regarding which services to receive. In an intuitive sense, the applicant measures the value of each choice and chooses those that have positive value. Most models of this type are called binary discrete choice models and include probit and logit as special cases.[4] Our model is a little more complicated because the applicant is making multiple binary choices at the same time.

Figure 3.4 shows how each VR applicant, possibly with the help of a counselor, decides which set of VR services to receive. For each service in the right column, the applicant evaluates the value of receiving that service. The value of a service depends on the demographic characteristics of the applicant, the type and severity

[4] See almost any advanced undergraduate econometrics textbook or any graduate econometrics textbook to learn about probit or logit estimation.

of their disability, and the behavior of the applicant's counselor. Demographic characteristics include race, gender, level of education, type of disability, and severity of disability. With respect to the behavior of the counselor, the more often the counselor recommends a particular service to other applicants, the more value the applicant places on that choice. The applicant chooses to receive each service with positive value after considering personal costs, such as the time and effort the service requires. This kind of assumption is made throughout the economics literature. We allow the applicant to choose multiple services because they do so in the data.

With respect to demographic variables, the possible effect for each is that it affects either the difficulty of the received service or the level of satisfaction the recipients get from participating in the service. For example, it might be the case that women find it easier than men to obtain more *Education* or that people with mental illness have a difficult time participating in *Training*. The first example would make it more likely that women participate in *Education* services, and the second would make it less likely that people with mental illness would participate in *Training*. Of course, these are just possible examples. Whether they (and similar examples) are true can be learned only by examining relevant data.

Consider a different possible effect: demographic characteristics interact with different services in different ways to affect labor market outcomes. For example, it might be the case that labor market outcomes associated with receipt of VR *Training* services are better for young people than old people. This is a reasonable argument, but, as you will see a bit later, it is inconsistent with the overall model. This is because we do not include an interaction between age and *Training* in the labor market part of the model. We could include interaction terms of age with each of the service terms, but that would require adding seven explanatory variables: age multiplied separately by each of the available service choices. However, it is important that we limit the number of variables included in the model. Every added variable slows the estimation program and makes it more likely that there will be too much correlation among the explanatory variables (i.e., multicollinearity). This is a good example of how the model's structure imposes discipline on the modeling procedure.

We included the counselor's behavior because we needed a variable that affects choices but has no direct effect on labor market outcomes.[5] Such methodology has been used in the literature, such as Doyle (2007) and Maestas et al. (2013). We discuss this issue more precisely in Sect. 2.2.5 and the appendices.

As is true in any empirical exercise, we cannot explain all of the variation in any dependent variable of interest. Figure 3.4 shows that the value of each service depends on a set of explanatory variables, including demographic variables and counselor effects. However, even after controlling for these effects, we cannot perfectly explain all of the choices that are made. In any modeling/estimation procedure, we call the difference between the dependent variable and what can be explained by the explanatory variables the error; see Fig. 3.10 for an example. All

[5]This is called an instrument or instrumental variable.

empirical models include errors because no empirical model can explain all of the variation in the dependent variables.

In most nonstructural models, the researcher does not specify what the error represents. The opposite is true in structural models. There are two common descriptions of the error in structural models. First, it represents the effect on the dependent variable of explanatory variables not included in the model. Usually, such explanatory variables are excluded because they are not available in the data set being used. Second, it represents the influence of truly random effects on the dependent variable.[6] In Appendix 1, we specify characteristics of the errors of the model and how they are related to each other.

3.2.2 Labor Market Outcomes

The next part of the model to discuss is the determination of the two labor market variables we use in the model. The first is employment, and the second is log quarterly earnings conditional on being employed. In the data, both labor market variables can be constructed from the UI data for each quarter included in the model. Employment in a particular quarter is equal to 1 if positive earnings are observed in that quarter. Conditional log quarterly earnings in a particular quarter are the log of quarterly earnings (conditional on being positive) for each quarter. The log function[7] is a mathematical function that translates positive arguments according to the curve in Fig. 3.5. The log function is used frequently in math, statistics, and econometrics because it has nice mathematical features and, in many applications seems to facilitate fitting the data well (see, for example, Heckman and Walker, 1990 for a generalization).

Figure 3.6 describes the determination of the two labor market outcome variables, employment and conditional log quarterly earnings. Each outcome variable is assumed to be determined by a set of demographic variables, disability type and severity, the VR services chosen by the VR applicant, and some local labor market characteristics. The demographic variables are the same as in the service choice process described in Fig. 3.4. Local labor market variables are of three types:

1. The local (county) employment rate, defined as the ratio of people employed in the county to the county's population in the quarter. This is included because some counties have better job opportunities than others, and this should affect the probability of getting a job, potential earnings, or both.

[6]Philosophically, the second is subsumed into the first if there might be explanatory variables the researcher has not considered that appear to cause the randomness.

[7]"log" is an abbreviation for logarithm, and we use the natural logarithm throughout the analysis (see Fig. 3.6).

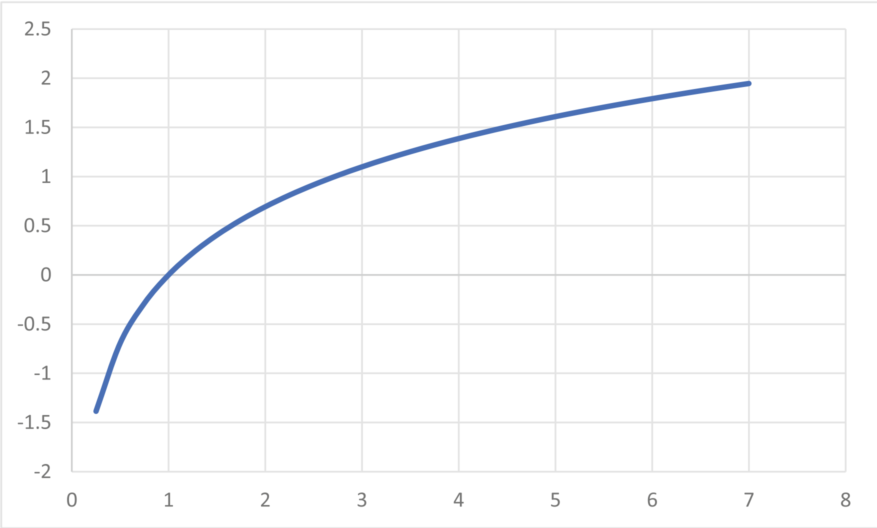

Fig. 3.5 Log function

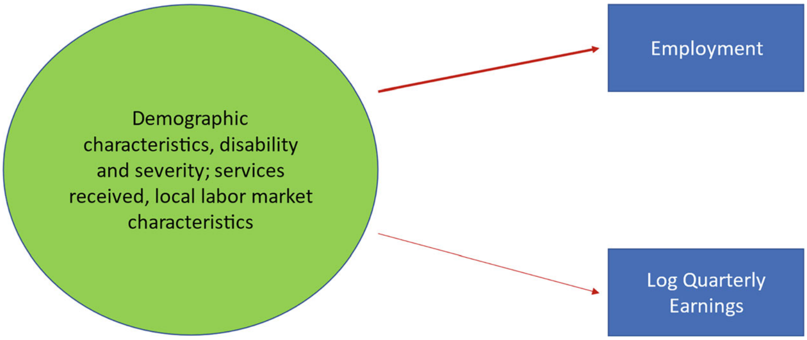

Fig. 3.6 Labor market effects

2. The proportion of people in the county who work for the federal government. This variable is allowed to affect the employment dependent variable because people who work for the federal government are not included in UI data. Also, having many federal jobs may change the health of the local labor market for people with disabilities. Therefore, this variable is allowed to affect both employment and conditional earnings.
3. The proportion of people in the county who commute across state lines to work. This variable is allowed to affect the employment dependent variable because people who work outside the state of residence are not included in that state's UI data. This variable is allowed to affect employment but not conditional earnings.

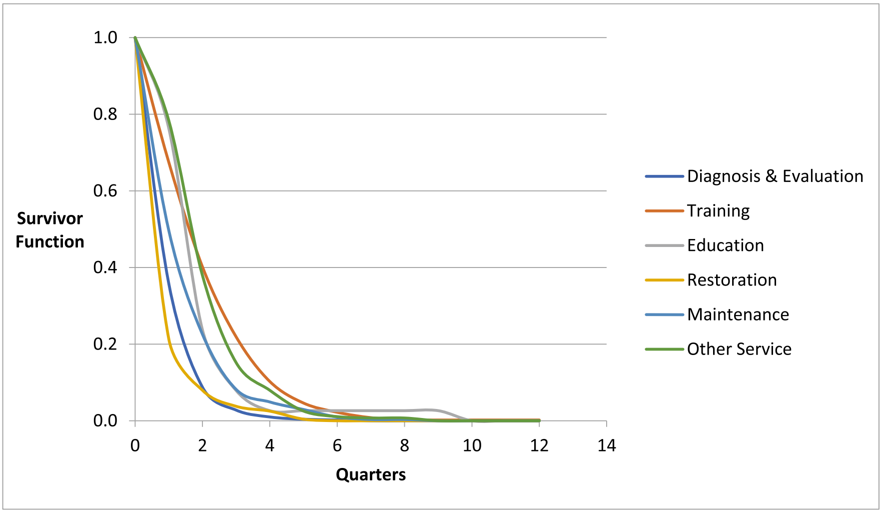

Fig. 3.7 Survivor functions for services

The variables associated with (2) and (3) can have two different effects. One is the intended correction for exclusion from the UI data. The other is that either variable might actually have a direct effect on employment.

For employment, the unobserved (or latent) dependent variable is the value to the individual of working. It is assumed that the individual chooses to work if and only if the value of working is positive. This is another example of the binary discrete model associated with service choice. However, in the service choice component of the model, the individual may choose multiple services. In contrast, in the employment model, the individual decides whether to work in each quarter of the data, and the errors associated with these choices may be correlated.

3.2.3 Other Issues

3.2.3.1 Service Receipt

As stated above, VR service receipt is assumed to occur in the same quarter as application to the VR agency. This assumption is obviously false, both in the sense that it might take some time for the applicant and his counselor to decide which services to use, and, more importantly, service receipt frequently takes more than one quarter. Figure 3.7 shows the probabilities of continuing to use each service at any time since the application date, called the survivor functions, for people with cognitive impairments from a 2000 application period cohort. There is a survivor curve for each VR service, and each curve shows the proportion of VR service recipients who were still receiving services each quarter. For example, the orange curve (for *Training*) has a height of approximately 0.2 at three quarters. This means that 80% (100%–20%) of those receiving VR training services had finished their training by the end of three quarters.

There are two lessons to take from Fig. 3.7. First, a significant proportion of service recipients use services that take more than one quarter. Second, there is significant variation in how long VR recipients use each service. The first fact provides strong evidence that the assumption that services last for one quarter is wrong. However, that, by itself, does not imply that the model is wrong; the researcher can simply interpret short-run benefits as being affected by VR service recipients who are still receiving service. In our work, we define the "short run" as the first eight quarters after application. Figure 3.7 shows that, for this group of applicants, very few clients receive any services beyond that period. The sole exception is *Education*, which includes college and graduate school.

The second fact is more problematic. Variation in the receipt of service strongly suggests that it is important to model not just the receipt of VR services but also the length of service receipt. This is problematic because of an endogeneity problem.[8] The researcher should be interested in how the length of VR service receipt affects labor market outcomes. However, the direction of causation is unclear. It might be the case that longer service receipt yields more benefits and positive effects on labor market outcomes. But, it might also be the case that those who have long spells of service receipt are problematic clients. If so, we should observe a negative effect of service length on labor market outcomes; recipients with the greatest need for long service receipt are also those least likely to succeed in the labor market.[9] Because of this endogeneity problem, we have chosen to pass on this issue for now. Suggesting how to deal with it requires extensive technical discussion, and we are not yet confident regarding a solution.

A related issue is expenditure on service receipt (cost). As was true for length of service receipt, there is significant variation in cost of service. The problem this creates is analogous to the service length problem. Specifically, it might be the case that the more spent on a client, the better prepared he is for the labor market. However, it might also be the case that clients who cost the most are those who would struggle in the labor market. This generates another endogeneity problem with no obvious solution. Thus, we pass on this issue for now.[10]

3.2.3.2 Time Decomposition

As discussed above, to allow for a flexible relationship between service receipt and employment, we decompose time into four time periods relative to the application

[8]Endogeneity is when some of the explanatory variables are correlated with the error in the equation. When this happens, in general, we obtain biased estimates of the effect of the endogenous explanatory variable.

[9]The solution for endogeneity is to construct an instrument; an exogenous (not endogenous) variable that affects service length but has no direct effect on labor market outcomes. We have so far not succeeded in constructing such an instrument for length of service receipt.

[10]We know of no papers in the literature that attempt to solve either of these endogeneity problems. We are presently working on those issues.

Pre-Application Quarters			A	Short-run Quarters			Long-run Quarters		
-15	to	-2	-1	1	to	8	9	to	15

Fig. 3.8 Time periods

date of the VR service recipient. These are illustrated in Fig. 3.8 and are offset from 0, the application date. The pre-application period includes quarters 2–15 prior to application. The box labeled "A" is the quarter before application and allows for an Ashenfelter dip. It is excluded from the analysis. We identify the short run as the first eight quarters after application (and, by assumption, the period during which most clients complete their service receipt). Finally, we identify the period beginning with the ninth quarter after application as the long run.[11]

We allow for the possibility that the effect of service receipt differs across the four time periods. We calculate the short-run effect of a service as the estimated effect of services during short-run quarters minus the estimated effect of services during the pre-application quarters. This is a measure of how much more valuable it is to work in the short run relative to prior to application. Similarly, the long-run effect of a service is the estimated effect of services during the long-run quarters minus the estimated effect of services during the pre-application quarters. This is a measure of how much more valuable it is to work in the long run relative to prior to application. One can see an example of this approach in Fig. 3.3.

We might say that the service effects discussed here are differences-in-differences measures. One of the differences applies to differences in time periods, and the other applies to differences between those who receive the service and those who do not.[12]

One might wonder how to interpret the service effects on employment prior to application (for example, the blue line in Fig. 3.3). We think of this as a measure of selection bias resulting from endogenous variables.[13] Another issue is that people with varying levels of success in the labor market prior to application might choose to apply for VR services at different rates (Vella, 1998). While this is important and interesting, it is not the focus of our analysis.

3.2.3.3 Disaggregation

Note that we allow employment to vary across disability type, demographic characteristics, local labor market characteristics, service type and receipt, and

[11] The boundary between the short and long runs are influenced by when most clients complete service. Figure 3.7 indicates that, with the exception of *Education*, nearly all clients with a cognitive impairment who applied in SFY 2000 completed service receipt within eight quarters of application. However, for the work described in Chap. 5 for applicants in SFY 2012, we extended the short run to ten quarters for those who are blind or vision impaired.

[12] Sect. 2.2.3, Angrist and Krueger (1999), and Brewer and McEwan (2000) provide more thorough discussions of differences-in-differences estimation.

[13] Sect. 2.2.2 Vella (1998) and Wooldridge (2002) provide more thorough discussions of selection bias. The correct definition is quite different from that used in papers outside of econometrics.

each of four time periods (see Fig. 3.8). All these sources of disaggregation are important, as is shown in Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019). For example, using a 2000 application cohort for youth transitioning into adulthood (Dean et al., 2019), the effect of *Training* on the value of working is 0.133 in the long run (Table 6 in Dean et al., 2019), and the effect of being white on the value of working is 0.089 (Table 9 in Dean et al., 2019). If we had excluded race from the set of demographic characteristics that potentially affect the value of working, then the estimated effect of *Training* on the long-run value of working would have been biased in the direction of the correlation between race and the receipt of *Training* services. Also, using the same example, if we had aggregated all service receipt into one binary variable, perhaps called "treatment," as in Fig. 3.1, we would have lost the opportunity to see how different services affect the value of working at different rates. Similarly, if we had aggregated all disability types, we would be unable to determine how service receipt and effect differ by disability.

The second labor market outcome we use is conditional log quarterly earnings for each quarter before and after application. Earnings in each quarter are conditional on being employed in that quarter (which is the same as having positive earnings in that quarter).[14] We transform earnings using the log function based on overwhelming evidence in the labor economics literature that doing so improves fit. We use quarterly earnings because the UI data report earnings by quarter.

3.2.3.4 Data Issues

An important issue is that, in most of the labor market literature, we think of earnings as hourly wage multiplied by hours worked in the quarter. The wage is a measure of the productivity of the worker, and hours worked is a statement about how much the worker wants to work and is able to find work. When we use earnings instead of hourly wage and hours, we cannot disentangle the effects of the different variables into productivity effects (wage) and willingness to work effects (hours). These are very different concepts and should be disentangled. Unfortunately, the UI data provide information only on quarterly earnings, so there is no way to disentangle earnings into wage and hours effects. This is simply an unfortunate characteristic of the UI data.

Figure 3.6 identifies the set of variables that affect both the probability of employment and conditional log quarterly earnings. One additional variable affects one but not the other: the proportion of people in the applicant's county of residence who cross state lines to work.

As we explained previously for employment effects, to allow for a flexible relationship between service receipt and conditional log quarterly earnings, we decompose time into four time periods relative to the application date of the VR service recipient, as depicted in Fig. 3.8. The periods used for employment are the same as those used for conditional log quarterly earnings. The benefits of separating

[14] Note that only positive numbers have logs.

service effects into the four periods, as in Fig. 3.8, are the same as those for employment.

As was true for employment, the way to interpret the service effects on conditional log quarterly earnings prior to application is to think of them as a measure of selection effects (discussed earlier). The issue here is that people with varying levels of success in the labor market prior to application might choose to receive services at varying rates. Although this is important and interesting, it is not the focus of our analysis.

Finally, as was true for employment, we need to include an error that measures the difference between the level of conditional log quarterly earnings reported in the data and that explained by the model. We construct a complex error structure that allows for correlation of errors across time, across the two labor market variables, and across service choices and labor market variables. The details of this structure are provided in Appendix 1. However, understanding the error structure is not necessary for understanding the results we will report in subsequent chapters.

3.3 Estimation

The model described in Figs. 3.4, 3.6, and 3.8 includes variables and parameters. Consider an extremely simple version of the part of the model described in Fig. 3.6 for conditional log quarterly earnings. Assume, temporarily, that conditional log quarterly earnings depend only on age and an error. We might specify this part of the model as

$$y_{it} = \boldsymbol{a} + \boldsymbol{b}x_{it} + u_{it}$$

where i indexes VR applicants, t indexes quarters for person i, y_{it} represents the conditional log quarterly earnings in quarter t for person i, x_{it} represents person i's age in quarter t, and u_{it} is the error for person i's conditional log quarterly earnings in quarter t. The parameters in the equation, $\boldsymbol{a}$ and $\boldsymbol{b}$, specify how y_{it} depends on x_{it}. This specification of the model implies that the relationship between y_{it} and x_{it} is a straight line, as illustrated in Fig. 3.9.

Given data on age and conditional log quarterly earnings, we still do not know the true values of $\boldsymbol{a}$ and $\boldsymbol{b}$, the parameters in the example above. We can use estimation methods to estimate $\boldsymbol{a}$ and $\boldsymbol{b}$.

In a broad sense, the goal in estimation is to pick values of the parameters that best explain the data. Consider, for example, Fig. 3.10. Each red dot represents an observation. Its horizontal placement is the age associated with that observation, and the vertical placement is the conditional log quarterly earnings. The goal is to find the line that best explains the data given some criterion for fit. The blue curve is the true relationship between age and log quarterly earnings, and the dotted purple line (which is almost the same as the blue line in this case) is the line that best fits the data using Ordinary Least Squares (OLS).

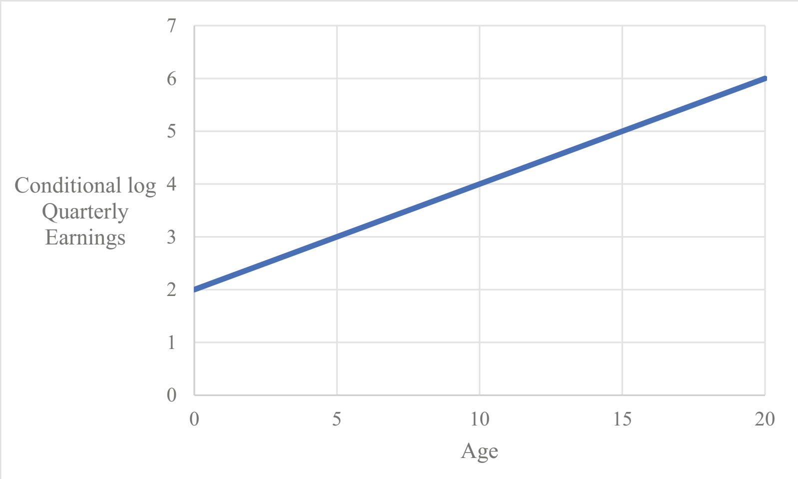

Fig. 3.9 Straight line

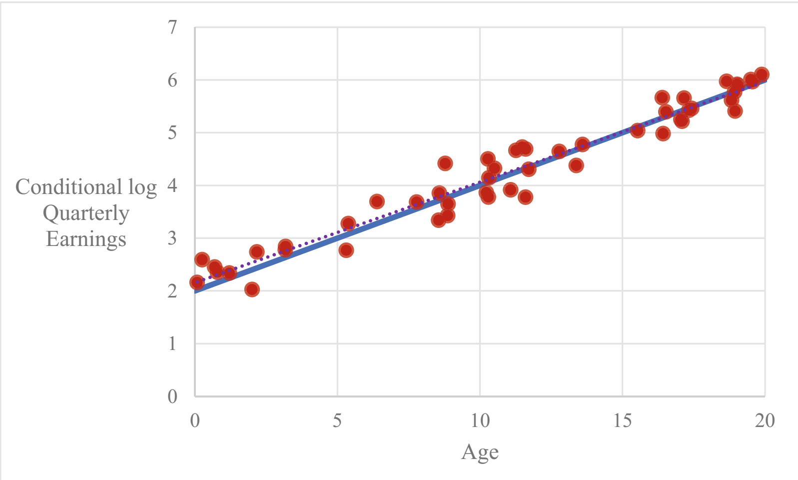

Fig. 3.10 Data

There are many different estimation methods in the econometrics literature; these include OLS, Instrumental Variables, and Maximum Likelihood Estimation (MLE). Our model has many more than two parameters, and we use a variant of MLE called Maximum Simulated Likelihood to estimate the parameters. Appendix 2 provides precise and rigorous detail for the estimation procedure. However, it is not necessary to read Appendix 2 to understand the discussion in the rest of the book.

3.3.1 Endogeneity

Before moving on to a discussion of the data used in estimation, it is useful to have an intuitive understanding of some of the econometric issues associated with any econometric exercise. These can be summarized by a discussion of endogeneity. As stated earlier, an endogenous explanatory variable in an equation of interest is one that is correlated with the error in the equation. This can happen because either (1) the endogenous explanatory variable is itself a dependent variable in another part of the model that depends on the dependent variable in the equation of interest or (2) the errors in the two relevant equations are correlated.

Consider a simple example concerning the supply and demand of bananas. Assume that the demand for bananas is a decreasing function of the price of bananas and the supply of bananas is an increasing function of the price of bananas. Let q_t^d be the demand for bananas in year t, p_t be the price of bananas in year t, and u_t^d be the error in the demand equation. Similarly, let q_t^s be the supply of bananas in year t and u_t^s be the error in the supply equation. Assume that

$$q_t^d = \boldsymbol{a} + \boldsymbol{b}p_t + u_t^d,$$

$$q_t^s = \boldsymbol{c} + \boldsymbol{g}p_t + u_t^s,$$

and that equilibrium in the bananas market requires that supply equals demand.

Figure 3.11 shows a possible downward-sloping demand curve (in purple), a possible upward-sloping supply curve (in red), and data on prices and output (supply = demand). Deviations between the equilibrium point (price = 6, output = 5.6) and the data points are caused by shifts in the demand curve, the supply curve, or both. In any case, for this model, data on price and output will never offer

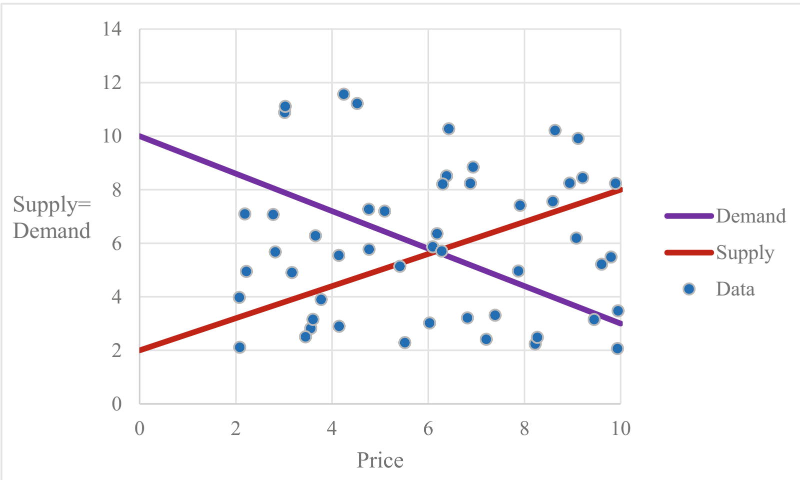

Fig. 3.11 Bananas market

information about the supply curve or the demand curve. When this happens, we say that the model is not identified.

Now, consider changing the model so that the supply curve is affected by a relevant measure of weather. In this case, shifts in the supply curve due to variation in weather enable us to learn something about the parameters of the demand curve (which stays fixed as the weather changes). As weather moves the supply curve, we can trace out the demand curve based on the prices and quantities we observe. When this happens, we say that the demand curve is identified, or the parameters of the demand curve are identified.

In the model described in Fig. 3.11 with the adjustment that adds weather to the supply equation, since we can identify the parameters of the demand equation we can estimate the demand curve's parameters, $\boldsymbol{a}$ and $\boldsymbol{b}$. However, there are still complex estimation issues. In the demand equation, the dependent variable is demand and one of the explanatory variables is price. But demand and price are jointly determined by the adjusted model. In this case, price is endogenous because it is determined inside the model. When endogenous explanatory variables exist in an equation of interest, special methods are required to estimate the parameters.

A similar endogeneity problem affects our model, described by Figs. 3.4, 3.6, and 3.8. In particular, the VR service variables are endogenous explanatory variables in the labor market outcomes part of the model described by Figs. 3.6 and 3.8. We must thus adjust our estimation methodology to deal with this problem.

3.3.2 Available Data

Even if one does not have a deep understanding of the estimation method, it is useful to have a good understanding of the data that are available for estimating the model parameters. The first source of data are the administrative records of the VR agency (agencies) being evaluated and using individuals who applied for services in State Fiscal Year (SFY) 2012, i.e., between July 1, 2011, and June 30, 2012.[15]

We observe a set of demographic variables:

- Gender (male = 1, female = 0);
- Race (white = 1, nonwhite = 0);
- Education (last grade of education completed);
- Special Education (whether the individual received a special education certificate);
- Age (measured in quarters and divided by 100);
- Marital Status (married = 1, not married = 0);
- # Dependents.

[15] Dean et al. (2015, 2017, 2018, 2019) use a SFY cohort, and Schmidt et al. (2019) use a SFY 2007 cohort.

There are also some explanatory variables concerning the disability of the individual: type of disability (e.g., mental illness, substance abuse) and a measure of the severity of the disability. The classification for severity is “not significant disability,” “significant disability,” and “most significant disability.” These severity measures are used to ration VR services during periods of order of selection.[16] Next, there are some miscellaneous variables, including whether the individual has a driver’s license or other available transportation and the amount of government benefits the individual receives.

We also observe which VR services are used by each individual. There are between 92 (Texas) and 824 (North Carolina) service categories tracked by VR agencies. This is way too many to use in modeling and estimation. Therefore, we aggregate services into seven service categories for all disability types:

- *Diagnosis & Evaluation* services include those provided at intake for assessing eligibility and developing an IPE. We deviate from standard VR practice by including medical diagnosis here rather than in a single category that combines medical diagnostics and medical treatments.
- *Training* includes on-the-job training, job readiness training, work adjustment, GED (graduate equivalency degree) expenses, and tuition and fees for vocational or business school.
- *Education* includes tuition and fees for junior and community college as well as 4-year college or university or graduate school.
- *Restoration* covers a wide variety of medical treatments including dental services, hearing/speech services, eyeglasses and contact lenses, drug and alcohol treatment, psychological services, surgical procedures, hospitalization, and prosthetic devices.
- *Maintenance* includes payments to facilitate everyday living and covers such items as transportation, clothing, motor vehicle and/or home modifications, and services to family members.
- *Placement* includes employment services, job development, and job retention.
- *Job Supports* includes job coaching and supported employment.

For individuals who are blind or vision-impaired, we add two other services:

- *Assistive Technology* includes those services the agency reports as “rehabilitation technology” to RSA. Examples include low-vision devices, assistive listening devices, and augmentative communication equipment.
- *Mobility & Orientation* includes those services the agency reports as “disability related augmentative skills training” to RSA. Examples include low-vision training, orientation and mobility training, travel training, and home training.

[16] Order of selection periods are those in which there are not enough funds to provide services to all applicants. In such periods, the order of selection for service receipt is based on severity of disability, with the most severe receiving priority.

These two are important for individuals who are blind or vision-impaired but not for those who are not, in which case they are included in *Restoration*.

We use the aggregated VR services in two ways. In the part of the model described in Fig. 3.4, VR services are the dependent variables to be explained using the available explanatory variables. In Fig. 3.6, they are explanatory variables (interacted with time as described in Fig. 3.8) used to explain employment and conditional log quarterly earnings.

The second source of data is state UI records for those individuals who applied for VR services. We observe, by quarter, the amount of labor market earnings earned by the individual. As discussed earlier, the range of quarters with earnings information is a minimum of 2.5 years prior to application to at least 6 years after application. We use the earnings data to construct two of the dependent variables in the model: employment (= 1 if and only if quarterly earnings are positive) and conditional log quarterly earnings (if and only if quarterly earnings are positive).

Some people are not included in the UI records. A reasonable rule of thumb is that those who work for the federal government and those who cross state lines to work are excluded from state UI records. Dean et al. (2015) provide evidence that there was a UI coverage gap of about 12% relative to employment reported at the federal level in Social Security earnings data in Virginia for a 2000 application cohort. This should vary across states depending on the footprint of the federal government in each state.

Since we cannot observe such individuals, we construct an adjustment for the problem. In particular, we construct data on the proportion in each county who work for the federal government using data from the Office of Personnel Management (2014) and the proportion in each county who cross state lines to work using data from the 2006–2010 American Community Survey (U.S. Census Bureau 2010). If a county has an unusually large proportion who work for the federal government or who cross state lines to work, we should expect to see a lower employment rate among VR applicants in the county.

The next source of data contains information on the employment rate in each county. This variable is constructed as the ratio of the number of people employed in the county divided by the adult population of the county. This is not the standard definition of the employment rate, which uses the labor force in the denominator. However, it is probably better for our purposes as it provides useful information about the odds of getting a job in the county the VR applicant lives in.

The final source of data contains information on fixed and variable costs, both of which are provided by the agencies. As part of their accounting system, agencies record the service type, vendor, and cost of every service provided to its clients. These purchases comprise the bulk of agencies' variable (marginal) costs.[17] Agencies report their fixed costs (comprised of administration, counseling, and

[17] We also observe services to clients provided by the Wilson Workforce and Rehabilitation Center in Virginia and the Workforce Technology Center in Maryland, both of which we have cost estimates for.

placement expenditures) annually to the Rehabilitation Services Agency via the RSA-2 form. We discuss how we apportion those fixed costs to clients in Sect. 2.3 and Chap. 5. These data are not necessary for estimating the parameters of the model. But, they are necessary for implementing return on investment or rate of return analysis (see the next section).

3.4 Return on Investment and Rate of Return

ROI and ROR analysis are related methods for measuring how beneficial VR service options are. To perform such analyses, we first need estimates of our model along with fixed and marginal cost numbers. The basic idea is to compare the benefits of services with the cost of services.

3.4.1 Discounting

An important issue must be addressed before we can compare benefits and costs. Although service costs end when the services end, potential benefits in the form of an increased probability of employment and earnings if employed could continue for years after the services end. Because money earned today is worth more than the same amount of money earned at some point in the future, we must translate those future earnings into their present-day value. This section describes that technique, which is called discounting. Equations are used throughout this section because the technique is inherently mathematical. The equations may appear to be intimidating, but the basic lesson is that the equations are used to discount future earnings gains into their present value to allow them to be compared with costs.

Before we can understand ROI and/or ROR analysis, we must have some understanding of discounting. Consider a simple example in which one has a choice between receiving \$100 today or \$100 one year from today. If one takes the \$100 today, then he can put the \$100 in a bank and earn interest at some rate r. Thus, a year from now, he will have $\$100 \times (1 + r)$, which is more than \$100. We can think about the same problem backward. The value of \$100 received in one year is $\$100/(1 + r)$. If one receives $\$100/(1 + r)$ today and puts it in the bank, in a year, he will have $(1 + r) \times \$100/(1 + r) = \$100$. Thus, we say that the present discounted value of \$100 received in a year is $\$100/(1 + r)$. By similar reasoning, one can show that the present discounted value of \$100 received in 2 years is $\$100/(1 + r)^2$, and the present discounted value of \$100 received in n years is $\$100/(1 + r)^n$. We can perform this analysis where the unit of time is a year, a quarter, a month, or any other period of time with only a small adjustment in the methodology.

Now consider a simple case for VR costs and benefits in which the cost of a particular VR service is \$100 and, conditional on receiving the service, one earns an extra \$20 per year forever starting in the year after service receipt. The net present value (NPV) of the \$100 cost at the time of service is \$100. This has an impact on the NPV of −\$100 (because it is a cost). In the first year after service receipt, one earns an extra \$20. The present value of this \$20 is $\$20/(1 + r)$ (because it is received

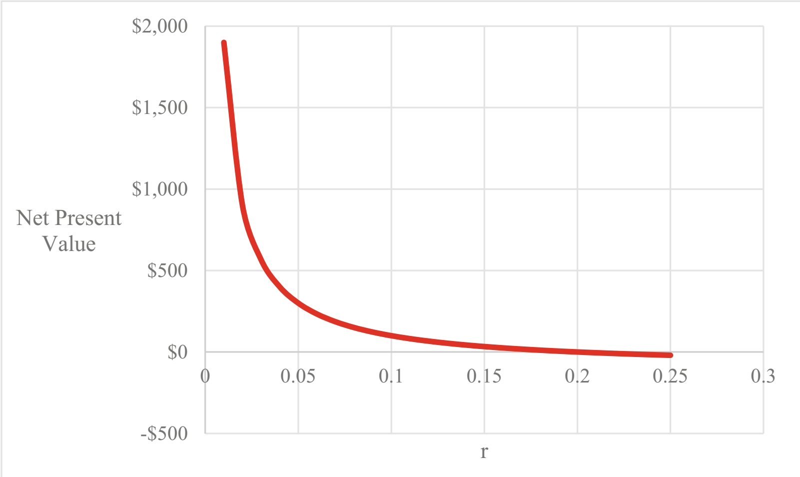

Fig. 3.12 Net present value

1 year in the future). In the second year after service receipt, one earns another extra \$20 with a present value of $\$20/(1 + r)^2$. We can do the same analysis for any year after service; e.g., the present value of the extra \$20 received in n years is $\$20/(1 + r)^n$.

Now, we are ready to compute the NPV of the VR service. It is equal to.

$$-100 + \frac{20}{(1+r) + \frac{20}{(1+r)^2 + \frac{20}{(1+r)^3 + \ldots}}}$$

With a little bit of math, we can show that this is equal to $-\$100 + (\$20/r)$. The NPV depends on the interest rate r. The smaller the interest rate, the greater the NPV. Figure 3.12 shows how the NPV changes with changes in r. When $r = 0.01$, the NPV is \$1900, while, when $r = 0.20$, the NPV is \$0. For any interest rate above 0.20, the NPV is negative.

3.4.2 Different Measures Associated with ROR

We have at least three well-known ways to evaluate the net benefit of a VR service. The first is to just report the NPV along with an assumption about r. A second way is to construct the ROI. Figure 3.13 displays the relationship between r and ROI. The ROI is equal to the present value of benefits divided by the cost. In the example above, the ROI is $(\$20/r)/\$100 = 0.2/r$. Note that both this formula and Fig. 3.13 show that ROI = 1 when $r = 0.2$ because that is when NPV = 0. This second way is also sensitive to the chosen assumption about r. As r increases, ROI decreases. Notice that, if $r = 0.01$, the ROI is 20 while the NPV is \$1900. The present value of future benefits is \$2000, which causes the NPV to be \$1900. Consistent with that, the

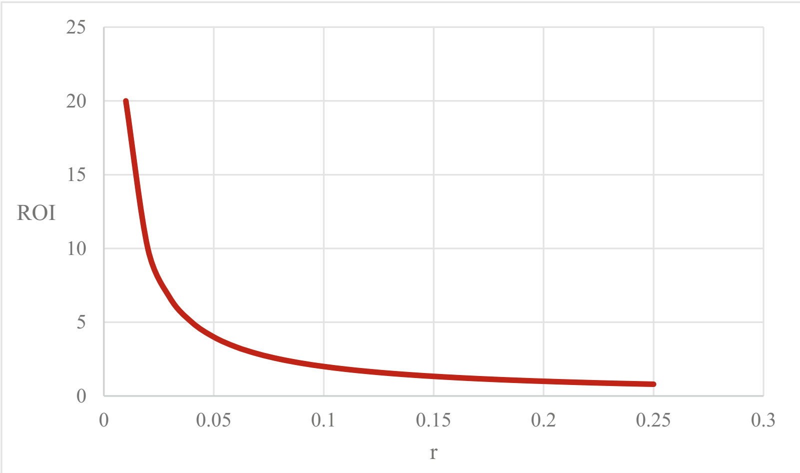

Fig. 3.13 ROI

ROI is \$2000/\$100 = 20. Also, note that, at $r = 0.20$, the present value of future benefits is \$100, which implies that the NPV is \$0 and the ROI is 1.

The last commonly used way is to use the ROR. Computing the ROR is significantly more complex than computing the other two choices. However, the ROR does not require one to make an assumption about the value of r. The ROR is the interest rate r that causes the NPV to be \$0. The general formula for the NPV is

$$\mathrm{NPV} = -\mathrm{Cost} + \frac{B_1}{1+r} + \frac{B_2}{(1+r)^2} + \frac{B_3}{(1+r)^3} + \ldots + \frac{B_T}{(1+r)^T}$$

where B_t is the benefit received in year t and T is the last period in which it is assumed benefits occur. To compute the ROR, one must evaluate the NPV equation for many values of r to find the value of r that sets NPV to \$0. By experimenting with different values of r in this formula, where cost is −\$100 and the annual benefit is \$20, we get an ROR of 0.20. In other words, the same r as shown in Fig. 3.12, where NPV is zero, and in Fig. 3.13, where ROI is one. The higher the costs, the lower is the ROR, and, the higher any benefit, the higher is the ROR. Note that as T increases, the benefit received in year T goes to \$0. Thus, there will be a value of T, T^*, such that the discounted benefit of all years beyond T^* are trivial and can be ignored. This effect works so that the value of T^* decreases with r.

There are two problems with using ROR. The first is that our data are quarterly earnings, not annual earnings. To solve this problem, we could aggregate quarterly earnings to annual earnings and compute an annual ROR. But this is an unnecessary approximation, which biases results (though not that much). A better way to solve the problem is to specify the NPV equation in terms of quarterly benefits and then compute the quarterly ROR. It is straightforward to translate a quarterly ROR into an annual ROR. The formula to do so is

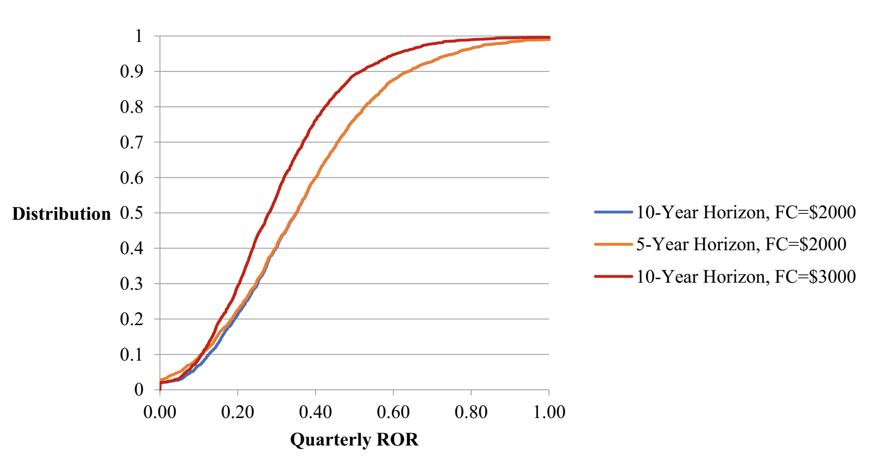

Fig. 3.14 Distribution of quarterly rates of return for physical disability. Source: Dean et al. (2018), Fig. 12

$$1 + \mathrm{ROR}_{\mathrm{annual}} = \left(1 + \mathrm{ROR}_{\mathrm{quarterly}}\right)^4$$

because there are four quarters in a year, and receiving quarterly interest four quarters in a row leads to annual interest.

The second problem is that, if the present value of the benefits is negative, there is no nonnegative value of r that satisfies the NPV equation. We address this problem in two ways, depending on the question to be answered. One way, used in Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019), is to report the proportion of individuals who have negative returns. A second way, used in Chap. 5, is to use NPV instead of ROR.

Different clients will have different estimated NPVs, ROIs, and RORs either because they chose different VR services or because they have different demographic (or other included) variables for the labor market outcomes (see Fig. 3.6). In general, we need some way to summarize the information included in these calculations for each person. Focusing on ROR for illustration, this can be done by computing a few numbers of interest or by computing the distribution of RORs. Figure 3.14 shows the distribution of quarterly ROR for the Virginia 2000 cohort for people with physical disabilities under three different assumptions. The assumptions concern how far into the future to compute quarterly benefits (5 years or 10 years) and fixed costs ($2000 or $3000). The curves in the figure report the proportion of people in the 2000 VR administrative data for people who have physical disabilities. For example, the orange curve passes through the point (0.2,0.185). This means that 18.5% of people in the sample have quarterly rates of return of no more than 20%.[18]

[18] A quarterly ROR of 20% implies an annual ROR of 107.4%.

Continuing with the orange curve, the median quarterly ROR is 35.1% because 50% of individuals have quarterly RORs no more than 35.1%.

The assumptions that provide the best distribution of ROR (to the right of alternatives) are that there is a 10-year horizon (T in the NPV equation is 10, but it is 40 when using quarterly earnings data) and fixed costs are $2000. This is the blue curve that is mostly the same as the orange curve (5-year horizon and fixed costs of $2000). The blue curve and the orange curve are pretty much the same because, for RORs greater than 20%, the benefits received after 5 years are so heavily discounted that they do not matter.[19] In effect, for large RORs, the length of the horizon has very little effect on the NPV. On the other hand, if, instead, we assume that fixed costs are $3000, the distribution of RORs moves significantly to the left. This implies that RORs are not as good. For example, for the blue and orange curves, the median quarterly ROR is 35.1%, while, for the red curve, it is 28.1%.

Note that all of the curves start at about 0.05% on the vertical axis. This means that irrespective of the assumptions, about 0.05% of applicants have an ROR that is zero or higher. The rest have a negative NPV. This happens when the sum of benefits for the applicant are less than the costs. If a large proportion of applicants have negative NPVs, we would want to look at their NPVs as well because they would provide valuable information about the people whose total discounted benefits are positive but less than the cost. Being able to observe the distribution of NPV and the proportion of NPV that is negative is valuable.

In the big picture, to perform NPV, ROI, and/or ROR analysis, the researcher must (1) construct a model that includes the effect of service provision on labor market outcomes; (2) assemble the data necessary to estimate the parameters of the model; (3) estimate the parameters of the model; and (4) perform NPV, ROI, and/or ROR analysis. The researcher cannot do step (4) unless she has successfully performed the prior steps. Many papers in the literature ignore the first three steps. Such papers obtain biased results and are difficult to interpret in terms of NPV, ROI, and/or ROR analysis.[20]

3.5 Interesting Trade-offs

3.5.1 Long-run Results vs. Short-run Results

In this section, we discuss interesting trade-offs associated with ROR and ROI analysis. The first trade-off is between getting long-run results slowly or getting short-run results faster. Long-run results are very important when one is interested in a time horizon that extends beyond the short run to compute NPV, ROI, and/or ROR.

[19] For example, if the quarterly ROR is 0.2, the discount factor for benefits received in the 21st quarter (after 5 years) is $1/[(1 + 0.2)^{21}–1] = 0.02$. This means that, for every dollar of benefits received in the 21st quarter, the discounted benefit is $0.02. In the 32nd quarter, it is $0.002.

[20] See Chap. 2 and Clapp et al. (2019) for a more detailed and extensive discussion.

They are also critical when long-run effects are quite different than short-run effects. For example, those in the 2000 cohort with mental illness (Dean et al., 2017), the short-run effects on conditional log quarterly earnings (short-run estimate minus-prior-to-application estimate) of *Training* and *Education* are -0.055 and -0.085, respectively. The long-run effects are 0.136 and 0.146. These results mean that, in the short run, conditional earnings decrease by 5.5% when *Training* is provided and by 8.5% when *Education* services are provided. On the other hand, in the long run, conditional earnings increase by 13.6% for *Training* and 14.6% for *Education*. If we had performed only a short-run analysis, we would have concluded that *Training* and *Education* are harmful services for people with mental illness even though, in the long run, they are very beneficial. It is not appropriate to assume that short-run and long-run results are similar.

However, there are some important advantages for performing short-run analysis. To perform long-run analysis requires waiting a long time for results. For example, if we want to use 7 years of data post-service receipt, we must wait at least 7 years to get any results. Frequently, results that are 7+ years old are not worth much, because the world of VR has changed too much over the 7 years to be relevant. For example, consider results from the 2007 application cohort received in 2019 (Dean et al., 2019).[21] Between 2007 and 2019, the provision of VR services changed dramatically because of the 2014 passage of the Workforce Innovation and Opportunity Act (WIOA). This changed the rules associated with VR service provision in many ways, and thus limited the value of long-run results.

If a VR agency administrator needs to report NPV, ROI, and/or ROR results to a policymaker, then, quick results are important. A policymaker is not going to be impressed by results that are a decade old. The administrator needs results as soon as possible. In this case, long-run results are not useful. For example, let's say that the VR administrator needs to report the effects of changes in VR due to the WIOA in 2020 (six years after passage). Obviously, long-run results are not possible. Short-run results are problematic for the reasons discussed above, but they are (possibly) better than having no results.

The last benefit of using short-run results is that the most available source of data is the national RSA-911 Case Service Report dataset. This provides data for every state, and it provides short-run outcome data. Thus, one of the costs of providing long-run results is that it makes it significantly more difficult to construct the data needed for analysis.

3.5.2 Complex Models vs. Simple Models

There is also a trade-off between using complicated models and estimation methods versus simple models and estimation methods. The big benefits of simple methods are that (1) it is more likely that a VR agency analyst will know how to perform the

[21] In economics, it frequently takes multiple years to do all four steps and then publish.

analysis and (2) it will require less time to construct the model and estimation method. The big disadvantage is that simpler models provide estimates that have ignored important econometric issues (e.g., endogeneity), which render the results less valuable or even useless. In Chap. 6, we investigate these issues in more detail.

The last trade-off of interest is how much heterogeneity to allow for. Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019) have shown that each type of heterogeneity is important. In particular, heterogeneity in observed explanatory variables, disability type, service effects, and time effects are all critical for any analysis. Also, it is straightforward to allow for heterogeneity. Thus, allowing for heterogeneity appears to be an obvious choice.

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National Data: What Do We Learn? 4

4.1 Introduction

The goal of this chapter is to consider some of the costs and benefits associated with using national data instead of state agency data (as was discussed in Chap. 3). First, we provide information on a list of national datasets available for research and discuss the costs and benefits of using national datasets.

Next, we discuss the implied differences in modeling between using national datasets and state agency datasets. The discussion implies that merging state RSA-911 Case Service Report (RSA-911) data with state unemployment insurance (UI) data is the best approach when examining a small set of states. If considering all states is critical, merging with UI data becomes prohibitively expensive, and the next best option is to only use the national RSA-911 data. Next, we provide a list of research papers that use national data and comment on a subset of the papers. Finally, we conclude.

4.2 National Datasets

Table 4.1 provides an incomplete list of potentially useful national datasets, along with important information about each one. For each dataset listed in the table, we indicate whether it is longitudinal (i.e., provides information on VR clients over multiple years), whether critical disability variables are available, whether variables concerning vocational rehabilitation (VR) are available, and whether information about labor market outcomes is available.

We first discuss (out of order in the table) the RSA-911 dataset. We discuss it first because it is used more often than any other dataset in papers on VR research. The dataset is a compilation of state VR administrative data, called RSA-911 data, from each public VR agency. Thus, each record's form is very similar to any individual VR agency data. However, some states include extra data in their state RSA-911 data. For example, Kentucky and North Carolina contain information about the

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Table 4.1 National datasets

Dataset	Longitudinal	Disability variables	VR variables	Income variables
American Community Survey (ACS)	N	Y	N	Y
Decennial Census	N	Y	N	Y
Current Popluation Survey (CPS)	N	Y	N	Y
Health and Retirement Study (HRS)	Y	Y	N	Y
Medical Expenditure Panel Survey (MEPS)	N	Y	N	Y
National Longitudinal Transition Study (NLTS)	Y	Y	Y	N
National Health Interview Survey (NHIS)	N	Y	N	Y
National Longitudinal Transition Study-2 (NLTS2)	Y	Y	Y	Y
National Longitudinal Survey of Youth - 1997 (NLSY97)	Y	Y	Y	Y
Panel Study of Income Dynamics (PSID)	Y	Y	N	Y
RSA-911 Case Service Reports	N	Y	Y	Y
Survey of Income and Program Participation (SIPP)	Y	Y	N	Y

Notes:
1. MEPS has a 2-year panel
2. NLTS2 has very limited VR services data

characteristics of each counselor, and North Carolina also contains information about services provided in-house.

4.2.1 RSA-911 Data

The national RSA-911 dataset is a repeated cross-section of each cohort of VR recipients requesting VR services.[1] The fact that it is a repeated cross-section means that we cannot follow VR recipients over time (because there is no key available to merge records from different years). For each fiscal year, for each public VR recipient in the United States whose case was closed during that fiscal year, there is a record that includes service receipt and expenditure, demographic variables, and

[1]It has been used, for example, by Cavenaugh (1999), Capella (2001), Capella-McDonnall (2005), Rogers et al. (2005), Dutta et al. (2008), Estrada-Hernández (2008), Lawer et al. (2009), Migliore et al. (2012), Darensbourg (2013), Roux et al. (2013), Steinman et al. (2013), Austin and Lee (2014), Chan et al. (2014), Chen et al. (2015), Cimera et al. (2015), Ipsen and Swicegood (2015, 2017), Giesen and Hierholzer (2016), Kaya et al. (2016), Alsaman and Lee (2017), Honeycutt et al. (2017), Kaya and Chan (2017), Nye-Lengerman (2017), Poppen et al. (2017), Giesen and Lang (2018), Kaya (2018), McDonnall and Cmar (2018), Ipsen and Stern (2020), and Roux et al. (2020). Also, it is merged with restricted Social Security data and used by Hyde et al. (2014).

Table 4.2 Short-run and long-run effects

	Cognitive (2015)		Mental illness (2017)		Physical (2018)	
	SR	LR	SR	LR	SR	LR
Diagnosis and Evaluation	\$334.1	\$281.7	−\$13.4	−\$320.0	\$632.4	\$328.2
Training	\$519.8	\$66.4	\$475.6	\$460.5	\$325.1	\$520.3
Education	\$101.8	\$66.4	−\$197.3	\$6.8	\$474.4	\$818.7
Restoration	−\$244.3	−\$286.3	\$128.5	\$11.9	\$736.4	\$671.0
Maintenance	−\$147.4	−\$55.6	\$658.9	\$65.2	−\$71.8	\$6.3

Notes:
1. SR = short-run effect, LR = long-run effect
2. An effect is the change in one quarter of earnings for the average person. The formula used to compute the effect is $(\Delta P^*E) + (P^*\Delta E)$, where P is the average probability of being employed, E is the average earnings conditional on being employed, ΔP is the change in the probability of being employed due to receipt of the particular service, and ΔE is the change in conditional quarterly earnings due to receipt of the particular service
3. Only five services are used, even though there are six in the papers considered. The missing service is "*Other*," and it is unclear what services are included in that category

labor market outcomes right before the individual's case was opened and soon after the individual's case was closed.

There are other attractive features of the national RSA-911 dataset. First, it is public data, so it is much easier to gain access to it than individual state data (which usually requires extensive administrative work and much data cleaning). This might be why the dataset is so popular.

Second, we can focus on a single state, multiple states, or the whole country. For each observation, a variable in the data specifies which state and county (FIPS) the observation came from. Ipsen and Swicegood (2015, 2017) and Ipsen and Stern (2020) use national RSA-911 data to analyze the use of public VR services across states, while Ipsen, Jain, and Stern (2022) disaggregate by counties across all states. Kaya et al. (2016) and Alsaman and Lee (2017) use the dataset to estimate the effects of VR service receipt on labor market outcomes. Schimmel Hyde et al. (2014), O'Neill et al. (2015), and Mann et al. (2017) merge it with restricted Social Security data.

The best features of the national RSA-911 dataset are (1) ease of use and (2) the fact that it contains almost all variables in VR public agencies and in the same form. The worst feature is that labor market data are very limited. We can observe the individual's quarterly earnings the quarter before entry into the VR agency and one and three quarters after closure. However, Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019) show that the effect of VR service varies over time. The estimated effect varies significantly between the short run (maybe three quarters after closure) and the long run, which is not available.

Table 4.2 shows short-run (SR) and long-run (LR) quarterly earnings effects. Each number in the table shows estimated effects for each disability group (cognitive impairment: Dean et al., 2015; mental illness: Dean et al., 2017; and physical

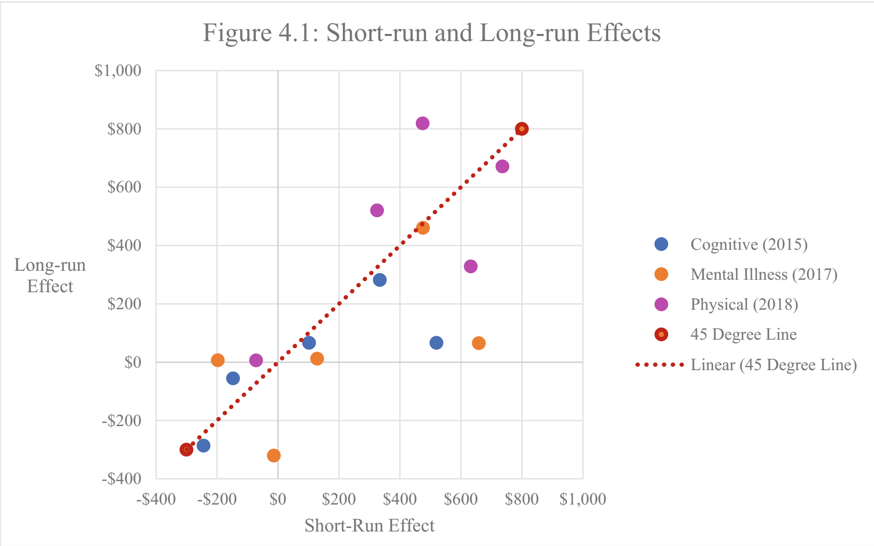

Fig. 4.1 Short-run and long-run effects

impairment: Dean et al., 2018). The effect measured is the expected change in quarterly earnings for an average quarter of an average VR recipient who used the relevant service. Because the expected earnings estimate is P*E (terms are defined in the table notes), the effect has two components: the change in the probability of being employed multiplied by conditional earnings (ΔP*E) and the probability of being employed multiplied by the change in conditional earnings (PΔE)).

For example, for VR recipients with mental illness, the short-run effect (1–8 quarters after VR application) of the receipt of *Training* services is $475.60. This means that, for each quarter between the first and eighth quarters after application, the earnings of the average VR recipient are $475.60. Similarly, for VR recipients who have a physical impairment, the long-run effect (beyond eight quarters) of *Maintenance* is $6.30, with a similar interpretation.

It is easier to identify the important patterns in Table 4.2 by looking at Fig. 4.1. In the figure, each point corresponds to a specific service for people with a specific impairment. The horizontal axis is the short-run effect listed in Table 4.2, and the vertical axis is the long-run effect listed in Table 4.2. For example, for an average VR recipient with a cognitive impairment, the short-run and long-run effects associated with the receipt of *Education* services are $101.80 and $66.40, respectively.

There is a 45° line in Fig. 4.1. All points above the line have greater long-run effects than short-run effects, and all below the line have greater short-run effects. The distance between any point and the 45° line is a measure of by how much the short- and long-run effects differ. For example, the highest point for physical impairment (at vertical effect just above $800) is far from the 45° line, which implies that long-run effects are much greater than short-run effects. Also, three of the points for cognitive impairments are very close to the 45° line, which implies that the short- and long-run effects are very similar for people with cognitive impairments (with the exception of one point that is far from the line).[2]

The important lessons associated with Fig. 4.1 are:

1. Almost all points are above \$0, which implies a positive long-run effect.
2. Many points are to the left of \$0, which implies a negative short-run effect.
3. Most services have smaller long-run effects than short-run effects (because they are below the 45° line).
4. There is no pattern in the differences between short-run and long-run effects across services and disabilities.

The fourth point implies that there is no way to infer long-run effects given only the short-run data provided in the RSA-911 dataset. This is an important flaw of the RSA-911 data.

4.2.2 Other National Datasets

As can be seen in Table 4.1, many other datasets are available from both the United States and the rest of the world. Many of these can be found in the directory of the Center for Large Data Research and Data Sharing (2022). The American Community Survey (ACS) provides annual data previously collected via long-form decennial Census questionaires. However, it is not a good dataset for research on VR because it does not include variables regarding the receipt of VR services in general, much less the receipt of specific types of VR services. Also, it is not longitudinal, so a researcher cannot measure employment and income either prior to VR service receipt or in the long run after VR service receipt.

Another national dataset that has been used to study the effects of VR on labor market outcomes is the National Longitudinal Survey of Youth—1997 (NLSY97), which Shandra and Hogan (2008) use to measure the effects of a limited set of VR programs included in the data. All other necessary variables are there, and it is longitudinal.

The Census basically contains the same variables available in the ACS. There are two important differences. The survey is administered only every 10 years, and although this requires extensive effort, Census records can be merged across Census waves, and thus it might provide longitudinal data. However, as in the ACS, there are no VR service receipt variables.

In fact, as can be seen in Table 4.1, only state and national RSA-911 Case Service Report data, the National Longitudinal Transition Study (NLTS), the NLSY97 data, and the second wave of the National Longitudinal Transition Survey (NLTS2)

[2] Hyde et al. (2014), O'Neill et al. (2015), and Mann et al. (2017) all choose not to compare short-run and long-run outcomes.

Table 4.3 Some international datasets on aging

Longitudinal and International Study of Adults (LISA)
Canadian Longitudinal Study on Aging (CLSA)
English Longitudinal Study of Ageing (ELSA)
Japanese Study of Aging and Retirement (JSTAR)
Korean Longitudinal Study of Aging (KLoSA)
Longitudinal Aging Study in India (LASI)
Survey of Health, Ageing and Retirement in Europe (SHARE)
Mexican Health and Aging Study (MHAS)
World Health Organization Study on Global Ageing and Adult Health (SAGE)

contain information on the receipt of VR services, either public or private. Also, only the NAMCS, NLSY-97, and NLTS2 are both longitudinal and contain information about the receipt of VR services.

Many randomized controlled trials, frequently performed by Mathematica, have national scope. Examples are Decker and Thornton (1996), Prero and Thornton (1991), Luecking and Wittenburg (2009), and Fraker et al. (2014). However, although many of these cover communities across the United States, they are frequently limited to a small set of communities. They tend to have a limited longitudinal span, and they are constructed to answer very specific questions. Thus, none of the datasets are used frequently by anyone other than the studies' principal investigators.

4.2.3 National Data Sources from Other Countries

The next dataset we consider is the Longitudinal and International Study of Adults (LISA). LISA is a Canadian dataset that follows 23,000 Canadians (with significant attrition). It runs biennially from 2012 to 2018, provides excellent measures of disability type and severity, and contains some information on VR services.

There is one unfortunate feature of LISA that makes it difficult to use. LISA can be used only if the researcher is Canadian (or has a Canadian coauthor). Also, research can only be published only after the Canadian government verifies that no confidential information is being made available.

Many other potentially useful datasets on disability are available across the world, some of which are listed in Table 4.3. However, none have information on receipt of VR services.[3] Thus, it appears that learning about VR service receipt requires either gathering relevant information about VR recipients in a subset of US states, as in Dean and Dolan (1991), Hollenbeck and Huang (2006), Howarth et al. (2006), Wilhelm and Robinson (2010), Dean et al. (2015, 2017, 2018, 2019), and Schmidt

[3] Part of the reason for this may be that in many countries, VR policy is more focused on incentives for employers to hire people with disabilities. Meanwhile, in the United States, it is more focused on the receipt of VR services by people with disabilities.

et al. (2019) or using the national RSA-911 data. This is a tough trade-off, because, although the national RSA-911 data are easy to collect and use, the data on labor market outcomes are mediocre. In contrast, state data, merged with state UI data, contain excellent information but are time- and labor-extensive to collect.

4.3 Differences in Modeling

In this section, we consider the differences in modeling imposed by the characteristics of individual state administrative data[4] and national RSA-911 data.

The most important advantage of state administrative data is that, once merged with UI data, we can observe quarterly earnings both before and after VR service receipt.[5] The use of such long-term data before and after VR application is necessary in order to measure both the short-run and long-run effects because it is necessary to compare labor market outcomes before VR application with labor market outcomes after VR application.[6] In Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019), earnings are observed up to 10 years after VR agency application. This allows us to estimate the short-run and long-run effects of VR service receipt on both employment probabilities and conditional quarterly earnings. This cannot be done using currently available years of national RSA-911 data, because it is prohibitively expensive[7] to match each person's VR receipt in the RSA-911 data with the appropriate UI data. This inability to estimate the effect of short-run and long-run labor market outcomes using the national RSA-911 is a major shortcoming in the RSA-911 data.

The next issue we examine is whether we can estimate selection bias, as discussed in Chap. 3. The problem here, as Clapp et al. (2019) demonstrate, is that people who apply for VR services may be different from the typical person with a disability, even after controlling for other observed explanatory variables. This possibility is called selection bias. It might be that people who apply for VR services or even choose a particular VR service might have some unobservable characteristics that affect both the value of the effect of the service on labor market outcomes and the preference for

[4] See Dean and Dolan (1991); Dean et al. (2002); Hollenbeck and Huang (2006); Howarth et al. (2006); Wilhelm and Robinson (2010); Dean et al. (2015, 2017, 2018, 2019); and Schmidt et al. (2019).

[5] Using state administrative data merged with appropriate UI data, Dean and Dolan (1991), Wilhelm and Robinson (2010), Dean et al. (2015, 2017, 2018, 2019), Hollenbeck and Huang (2006), Howarth et al. (2006), and Schmidt et al. (2019) observe quarterly earnings for approximately 3 years prior to VR application. Meanwhile, using RSA-911 data allows us to observe quarterly earnings only in the quarter prior to VR application. Unfortunately, Ashenfelter (1978) shows that labor market outcomes immediately prior to service receipt have different features than those in quarters further back in time.

[6] See Chap. 3.

[7] Payment of an annual fee is required. Although the average state fee is not that large, if multiplied by 50 it is too costly. Also, each merger requires a significant amount of high-level staff or researchers to resolve administrative and data problems.

that (or those) service(s). If so, the estimated effect of the VR service may be biased upward or downward, depending on the correlation between the service selection error and the labor market outcomes errors.

When using state RSA-911 data merged with UI data, we can control for and measure selection bias using extra variables with specific properties, which are called instruments. Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019) propose a set of instruments available in the RSA-911 state data. All of these papers use the behavior of VR counselors across all of their clients as an instrument for each client to capture a counselor's preference for particular VR services. Under the assumption that these preferences have no direct effect on labor market outcomes, these are good instruments for the effect of services on labor market outcomes. Dean et al. (2017) also use county estimates of the number of individuals with mental health problems as an instrument for the number who apply for VR services in each county.

There is reason to believe that the national RSA-911 data also contain appropriate instruments. For example, the national RSA-911 data can be merged with data on states' characteristics, and those characteristics can be used as instruments. Thus, we think that the limiting properties of the national RSA-911 data may not prevent analysis of selection problems.

As noted previously, the biggest problem with the national RSA-911 dataset is that it has very limited data on earnings before and after VR application. The biggest advantage of the national RSA-911 data is that it is straightforward to compare outcomes across states, since VR data for each state are contained in the national data. To do this with multiple state agency datasets merged with UI data would be very expensive.

4.4 Research Using National Data

As can be seen in Table 4.4, the national RSA-911 dataset is the most frequently used for research on VR, in part because it is readily available. On the other hand, the data on earnings are quite limited. Some studies, such as those by Capella-McDonnall (2005), Schimmel Hyde et al. (2014), O'Neill et al. (2015), and Mann et al. (2017) combine the national RSA-911 data with other restricted data or other data that are very expensive to collect.

With the exceptions of Aakvik et al. (2005), Dean et al. (2015, 2017, 2018, 2019), and Schmidt et al. (2019), no prior studies control for selection issues, as we explain in Chap. 3. Also, only Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019) allow for time-dependent changes in the effectiveness of VR services and for effectiveness that can vary across services and disability types. Yet, it is clear that all of these issues are empirically important.

Other variables can easily be merged with state or national RSA-911 data based on the available county (FIPS) code in the national RSA-911 data. Dean et al. (2015, 2017, 2018, 2019) merge state RSA-911 Case Service Report data for Virginia with data on local employment rates, while Schmidt et al. (2019), using data from Virgina

Table 4.4 Data and research on the effects of vocational rehabilitation

Author(s)	Year	Data source	Timing of measurement of labor market outcomes
Aakvik et al.	2005	Norwegian VR administrative data	4 years after application
Alsaman and Lee	2017	National RSA-911	Short time span
Austin and Lee	2014	National RSA-911	Short time span
Capella	2001	National RSA-911	Short time span
Capella-McDonnall	2005	National RSA-911 w/ extra longitudinal data	5 years
Cavenaugh	1999	National RSA-911	Short time span
Chan et al.	2014	National RSA-911	Short time span
Chen et al.	2015	National RSA-911	Short time span
Cimera et al.	2015	National RSA-911	Short time span
Darensbourg	2013	National RSA-911	Short time span
Dean and Dolan	1991	Virginia VR data merged w/ UI data	3 years after closure
Decker and Thornton	1996	National RCT	Short time span
Dutta et al.	2008	National RSA-911	Short time span
Estrada-Hernández	2008	National RSA-911	Short time span
Frolich et al.	2004	Riks-LS dataset	Short time span
Giesen and Hierholzer	2016	National RSA-911	Short time span
Giesen and Lang	2018	National RSA-911	Short time span
Hemenway and Rohani	1999	Florida administrative records (no UI data)	Short time span
Hollenbeck and Huang	2006	Washington VR data merged w/ UI data	Long time span
Honeycutt et al.	2017	National RSA-911	Short time span
Howarth et al.	2006	South Carolina administrative records	Short time span
Ipsen and Stern	2020	National RSA-911	None used
Ipsen and Swicegood	2015	National RSA-911	None used
Ipsen and Swicegood	2017	National RSA-911	Short time span
Kaya	2018	National RSA-911	Short time span
Kaya and Chan	2017	National RSA-911	Short time span
Kaya et al.	2016	National RSA-911	Short time span
Lawer et al.	2009	Special data from Dept of Education	Short time span
Mann et al.	2017	National RSA-911 linked w/ SSA data	Long time span

(continued)

Table 4.4 (continued)

Author(s)	Year	Data source	Timing of measurement of labor market outcomes
McDonnall and Cmar	2018	National RSA-911	Short time span
Migliore et al.	2012	National RSA-911	Short time span
Nye-Lengerman	2017	National RSA-911	Short time span
O'Neill et al.	2015	National RSA-911 data & Restricted SS data	10 years
Poppen et al.	2017	Oregon data from state agency	10 years
Rogers et al.	2005	National RSA-911 data	Short time span
Roux et al.	2013	NLTS2	Long time span
Roux et al.	2020	National RSA-911	Short time span
Schimmel Hyde et al.	2014	National RSA-911 & Restricted SS data	4 years after application
Shandra and Hogan	2008	NLSY - 1997	Long time span
Steinman et al.	2013	National RSA-911	Short time span
Wagner et al.	2005	NATCS	
Wilhelm and Robinson	2010	Utah VR data merged w/ UI data	3 years after closure

and Maryland, also use cross-state commuter patterns and federal employment. In general, the effect of local employment rates is positive for VR applicant employment probability and more ambiguous on VR applicant conditional log quarterly earnings. Schmidt et al. (2019) find that federal government employment has a large positive effect on VR applicant employment.

Part of the expected effect of federal employment on VR applicant employment is negative because federal employees are not included in the UI records.[8] The other part is due to the possibility that the federal government causes increases in private sector jobs for people with disabilities. The positive estimated effects suggest that the latter effect is large and dominates the former effect.

4.5 Conclusion

The discussion in this chapter suggests three approaches to empirical research on VR in the United States. One approach, which is by far the most popular, is to use state or national RSA-911 data and exclude labor market outcomes other than those close to the closure data for each applicant. Based on previous work (Dean et al., 2015, 2017, 2018, 2019; Schmidt et al., 2019), this approach is problematic.

[8] Dean et al. (2015) states that, in Virginia, federal employment in Virginia leads to a 12% reduction in the inclusion of UI records for VR applicants.

A second approach is to supplement RSA-911 VR data with other data that contain more longitudinal data. Schimmel Hyde et al. (2014), O'Neill et al. (2015), and Mann et al. (2017) do this by merging RSA-911 data with restricted Social Security data. While this approach yields very good data, especially on long-term SSDI and SSI receipt, it is costly and difficult to work with the Social Security Administration data.

The third and most promising approach is to merge RSA-911 data for a relatively small number of states with extended state UI data. This allows for measures of earnings both before and after VR application, extending as far into the future as desired given the year(s) of VR application. These are the data used to estimate the sophisticated models in Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019).

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5 Analysis of VR Programs in Five States

5.1 Introduction

This chapter presents results from the most comprehensive and expansive application of the structural VR-Return on Investment (VR-ROI) model to date. The model leverages longitudinal quarterly employment and earnings data to assess the impact of VR across eight distinct state agencies, four distinct disability groups, and seven to nine distinct VR services. For the first time, the VR-ROI model is employed to concurrently compare short- and long-run labor market outcomes across a broad spectrum of agencies and disability groups, which yields unparalleled insights into the diverse effects of vocational rehabilitation services. This analysis sets the stage for comprehensive understanding of VR's efficacy and provides a detailed picture that informs stakeholders about the nuanced benefits and challenges within these critical programs.

We present VR-ROI model estimates from five states, Kentucky, Maryland, North Carolina, Texas, and Virginia, and four disability types, mental illness (MI: anxiety disorders, depressive and other mood disorders, personality disorders, schizophrenia, and other mood disorders); physical impairment (PI: internal and musculoskeletal); cognitive impairment (CI: intellectual disability and learning disability); and blindness and visual impairment (BVI). We focus on applicants during State Fiscal Year 2012 (SFY2012: July 1, 2011–June 30, 2012) for the CI, MI, and PI analyses and SFYs 2007–2012 for the BVI analyses. In total, we discuss 19 separate sets of estimates: three for Kentucky (no BVI) and four for the other states.

Prior to presenting our results on the estimated economic return, in Sect. 5.2 we provide a general overview of the VR program and VR-ROI data and model (see Chaps. 1–4 for more details) and summarize the basic descriptive characteristics of the 19 VR applicant cohorts examined in our analysis. In Sect. 5.3, we discuss the VR-ROI model short- and long-run estimates of the effects of the different VR services. Then, in Sect. 5.4, we summarize the estimated present discounted value of VR services, the cost of VR services, and the net present value (i.e., benefits minus

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cost) of VR services. Although VR confers positive economic benefits for many VR clients, the costs of VR services exceed the average benefits across all applicant cohorts evaluated. Section 5.5 summarizes our results.

5.2 Cohorts and Data

The federal Rehabilitation Services Administration (RSA) provides formula grants to state VR agencies to administer VR services, supported employment services, and independent living services for individuals with disabilities in all 50 states, the District of Columbia, Puerto Rico, and four territories. Thirty-four states and territories have a single agency that serves individuals with all types of disabilities, referred to as a Combined VR agency. In addition, 22 states have two agencies: one that serves individuals who are blind or have visual impairments, referred to as a Blind VR agency, and a separate agency that serves individuals with all other types of disabilities, referred to as a General VR agency (U.S. Department of Education, 2024, page 5).

Our analysis focuses on applicants for VR services in five states. Three of these, Kentucky, Maryland, and Texas, currently have a Combined agency, and North Carolina and Virginia have both a General and a Blind agency. However, during the time individuals in our sample received services, only Maryland had a Combined agency.[1] Having a separate Blind agency may impact ROI estimates on both the cost and benefit sides. On the cost side, for example, Blind agencies have many fewer clients over which to spread fixed costs. On the benefits side, Blind agencies can develop policies and services that are more useful for people who are blind or vision-impaired compared with Combined agencies, which develop policies for a broad spectrum of disabilities.[2]

We use readily available administrative data from three sources (see Chap. 3 for more details): state VR agencies for client, case, and service provision data, state unemployment insurance (UI) offices for quarterly earnings data, and national public datasets for county-level economic data. Agencies merge their records and UI

[1] Maryland's agency is the Division of Rehabilitation Services (MD DORS) and Kentucky's is the Office of Vocational Rehabilitation (KY OVR). Virginia continues to operate two separate and autonomous agencies, the Department for Aging and Rehabilitative Services (VA DARS) and the Department for the Blind and Vision Impaired (VA DBVI). North Carolina's two agencies are now administered as separate divisions under the state's Health and Human Services: the Division of Vocational Rehabilitation Services (NC DVRS) and the Division of Services for the Blind (NC DSB). By contrast, Texas combined its two agencies, the Division for Rehabilitation Services (TX DRS) and the Division for Blind Services (TX DBS), and moved the Combined agency into the Texas Workforce Commission (TWC).

[2] Prior empirical results on the impact of agency type on the impact of VR for BVI clients is mixed. Clapp et al. (2024) find that the NPV of Blind agencies is notably lower than that of Combined agencies, while Cavenaugh (2010) and Warren-Peace (2009) find that earnings at closure of consumers who were legally blind were significantly higher in Blind agencies than in Combined agencies. However, Capella (2001) found that agency type plays no role in future earnings.

Table 5.1 Sample determination for non-BVI applicants during SFY 2012

	VA	MD	KY	NC	TX
Number of VR applicants in SFY 2012	8399	9049	9691	23,684	18,322
As a percentage of disabled population[a]	0.80%	1.33%	1.23%	1.80%	0.55%
Percentage of applicants dropped due to:					
Disability other than CI, MI, PI	16%	9%	20%	24%	24%
All other reasons	1%	5%	1%	5%	5%
Number in at least one sample	6931	7703	7644	16,854	13,088
As a percentage of applicants	83%	85%	79%	71%	71%
Number in analytic sample by disability					
Cognitive impairment	3184	3010	2372	6131	3580
Mental illness	3578	4665	4656	8085	4590
Physical impairment	2725	3414	3526	6239	7746

[a]The number of individuals with a disability in a state was estimated by multiplying the percentage of the state's population with a disability in 2023 (Institute on Disability, 2025a) by the state's population size in 2012 (U.S. Census Bureau, 2024)

records using Social Security numbers and strip identifiable personal information from the data before sharing them with us using secure means. After describing the cohorts, we discuss each of these datasets in turn.

5.2.1 Sample Frame

We restrict our analysis to individuals with a cognitive impairment (CI), mental illness (MI), physical impairment (PI), and/or blindness or visual impairment (BVI). At certain points in this chapter, we reference the CI, MI, and PI samples collectively using the term non-BVI, irrespective of whether they were served in a Combined or General agency. For non-BVI disabilities, we consider VR applicants during State Fiscal Year (SFY) 2012, i.e., the period July 1, 2011–June 30, 2012. However, except for Texas, there are too few BVI applicants in SFY 2012 to estimate our model with sufficient precision. Thus, we pool applicants from SFYs 2007–2012 for each state other than Texas in which the agency's large size enables us to use only SFY 2012 applicants.

One significant difference between Texas and other states influences interpretation of the results presented in this chapter. Data received from non-Texas agencies include all individuals who applied for VR services during the specified period. By contrast, data received from Texas agencies include only applicants who completed an individual plan for employment (IPE). We discuss this further in Sect. 5.2.2.1 regarding service provision.

Table 5.1 summarizes how we derived analytic samples for non-BVI disabilities. Row 1 identifies the sampling frame; i.e., the number of non-BVI applicants in 2012 by state. North Carolina (NC) had the most applicants, Texas (TX) the second most, and Virginia (VA) the fewest. Although the number of individuals with a disability in a state influences the number of VR applicants, Row 2 shows substantial variation

Table 5.2 Sample determination for BVI applicants during SFY 2012

	VA	MD	NC	TX
Number of BVI applicants in period	2518	2328	7448	1812
As a percentage of population with a BVI[a]	1.28%	1.98%	2.94%	0.25%
Percentage of applicants dropped due to				
Missing comorbidity information	2%	1%	1%	19%
All other reasons	2%	3%	4%	1%
Number in sample	2419	2254	7089	1446
As a percentage of applicants	96%	97%	95%	80%

[a]The number of individuals with a visual impairment in a state was estimated by multiplying the percentage of the state's population with a visual impairment in 2023 (Institute on Disability, 2025b) by the state's population size in 2012 (U.S. Census Bureau, 2024)

in the ratio of applicants to the estimated number of people with a disability in the state. NC again is the highest at 1.80%, and TX is the lowest at 0.55%.[3] VA is also under 1%, but that is likely due to a 30% drop in applications in 2012 compared with the average for the previous 5 years. Using that 5-year average, the VA ratio would have been 1.11%. Explaining the variation in this ratio, although interesting, is beyond the scope of our analysis.[4]

We exclude an individual from our analysis for several reasons; the most important is that the individual does not have a CI, MI, or PI. For example, people with hearing impairments would not be included in any of these disability groups unless they also had a CI, MI, or PI. Row 3 shows the percentage dropped for this reason, which ranges from 9% in MD (Maryland) to 24% in NC and TX. We also exclude individuals whose location is unknown or living out of state or missing key information such as education, gender, race, or age. Row 4 shows that this accounts for at most 5% of applicants.

Row 5 reports the number of individuals in at least one of the three analytic samples (CI, MI, PI), that represents between 71% and 85% of all 2012 applicants, as shown in Row 6. The final three rows provide sample sizes for each agency and each disability. These range from 2372 for CI in KY to 7746 for PI in TX -samples that are large enough to estimate the VR ROI model with a high degree of precision. Note that the samples are not mutually exclusive; i.e., an applicant may be included in more than one sample. For example, an individual with a mental illness and a physical impairment is included in both the MI and PI samples, and we control for that comorbidity in each model.

Table 5.2 presents similar information for the BVI samples. It differs from Table 5.1 in two respects. First, we were not able to collect BVI applicant information from Kentucky. Second, except for Texas, there were too few BVI applicants in

[3]The results for Texas reflect, in part, the exclusion of those who did not complete an IPE. That reduces the number of TX clients included in the data by about 35% (the average for other agencies), which implies that the comparable percentage for TX is about 0.89% (0.58/0.65).

[4]See Ipsen and Stern (2020) for more analysis.

SFY 2012 to estimate our model with sufficient precision. Thus, for VA, MD, and NC, we consider applicants from SFY 2007–2012 and focus on the first application during those years. Applications are distributed across these 6 years over a range of 14%–22%. By contrast, TX is a large agency with sufficient applicants to focus solely on SFY2012.

Row 1 lists the number of individuals who applied for BVI services during the period. Again, NC has the most (7448) and TX, with applicants who applied in a single year, has the fewest (1812). Row 2 presents these statistics as a percentage of the estimated number of individuals in the state who are blind or visually impaired. TX is comparatively low (0.25%) because this represents a single year's applicants and does not include applicants who did not complete an IPE. For those with a 6-year applicant pool, percentages range from around 1% to 3%.

Rows 3 and 4 display the percentage of individuals who were excluded from the analysis due to missing information; the largest group, TX (19%), was missing comorbidity information. Row 5 reports the size of each sample, ranging from 1446 in TX to 7089 in NC, which ranges from 80% of the original applicant pool in TX to at least 95% in each of the other states.

5.2.2 Service Provision

Individuals who apply for VR services are assigned to a counselor who records a diagnosis and determines eligibility for services. The case can be administratively closed during the application process if the individual withdraws from further consideration or if the counselor determines that the individual's disability is insufficiently severe or too severe to benefit from VR services. If the individual is determined to be eligible for VR services and does not withdraw, the counselor and the individual together develop an IPE that specifies the array of services to be provided.

5.2.2.1 Impact on Service Provision Percentages of Completing an IPE

As noted in Sect. 5.2.1, data received from all states other than Texas include individuals from all three of these categories: (1) those whose application was not successful, (2) those who were accepted but did not complete an IPE, and (3) those who were accepted and completed an IPE. Individuals in category (1) predominantly receive services to assess their eligibility. Those in category (2) are more likely to receive a broader range of services that might assist in developing an IPE in collaboration with their counselor. Finally, by implementing the IPE, those in category (3) are more likely to receive both a broader range of services and more comprehensive and/or intensive services.

By contrast, data received from Texas include only those applicants in category (3); i.e., those with a completed IPE. Since the sampling frame differs between Texas and other agencies, the results must be interpreted with that difference in mind.

As a rough gauge of the impact of restricting the sample to those with a completed IPE, we compared differences in service provision for non-Texas agencies between

Table 5.3 Examining the influence of restricting non-BVI samples to those with a completed IPE

	VA	MD	KY	NC
Percentage of applicants with a completed IPE	56%	63%	69%	67%
Percentage increase in service provision				
Category that includes assessment for eligibility	23%	10%	0%	6%
All other service categories	50%	46%	32%	23%

all applicants and those completing an IPE. Table 5.3 focuses on the non-BVI samples in states with data for all SFY 2012 VR applicants. The table reports the percentage of applicants who completed an IPE and the percentage increase in service provision when only those applicants with an IPE are included. Unsurprisingly, the percentage increases for the service category that includes services to assess eligibility are much lower than increases for other service categories. Also, those increases are smaller for the states (KY and NC) with the highest rates of IPE completion.

These disparities raise the issue of whether to define the sampling frame as including all applicants or only those with a completed IPE. For several reasons, our preference is to include all applicants when those data are available. Most importantly, our interest is in assessing the economic return on all services provided by the VR program, including those provided before an IPE has been completed.[5] The receipt of a particular service can impact subsequent labor market success irrespective of whether that service was provided to assess eligibility, develop an IPE, or implement an IPE. Services provided pre-IPE also have cost implications for VR, and therefore should be included as a source of possible benefits. In addition, applicants who did not complete an IPE often received limited VR services, and thus can serve as an important control or comparison group for our analysis. That is, they reveal the labor market outcomes of applicants with few if any services. Finally, including applicants without a completed IPE increases sample sizes and, consequently, increases the precision of our estimates.

5.2.2.2 Background and Service Provision

Services are provided (1) internally by agency personnel, (2) as a “similar benefit” purchased or provided by another governmental agency or not-for-profit organization at no charge to the VR agency, (3) as a “purchased service” through an outside vendor using agency funds, or (4) as some combination of (1), (2), and (3). Agencies record the specific type of service (which range between 92 and 824 categories, depending on the state), the dollar amount if purchased from an outside vendor, the earliest date the service could be provided, and vendor information. We do not observe services provided through similar benefits for any agency. Nor do we observe services provided internally by the Kentucky, North Carolina, or Texas agencies. Thus, for the KY, NC and TX agencies, we focus exclusively on purchased

[5] Estimating the differential impact of services provided pre-IPE versus post-IPE is an area for future research.

services. By contrast, Maryland and Virginia service records include a code that indicates whether the service was purchased or provided by the agency. Maryland's Combined agency and Virginia's General agency also fund state-operated comprehensive rehabilitation centers, the Workforce Technology Center (WTC) in Maryland and the Wilson Workforce Rehabilitation Center (WWRC) in Virginia, and we treat those services as being provided by the agency as well.

As noted, our service data track between 92 (Texas) and 824 (North Carolina) categories. Because it is neither practical nor useful to estimate a set of coefficients for so many services, we aggregate them, following Dean et al. (2015, 2017, 2018, 2019), into seven service types for non-BVI cohorts.[6] *Diagnosis & Evaluation (Diagnosis & Evaluation)*[7] services include those provided at intake to assess eligibility and develop an IPE. In contrast to standard VR practice, which aggregates medical diagnosis and treatments into a single category, we include medical diagnostics in *Diagnosis & Evaluation* and medical treatments in *Restoration*. *Training* includes vocationally oriented expenditures for on-the-job training, job readiness training, work adjustment, graduate equivalency degree (GED) expenses, and tuition and fees for vocational or business school. *Education* includes tuition and fees for community college as well as 4-year college or university or graduate school. *Restoration* covers a wide variety of medical expenditures including dental services, hearing/speech services, eyeglasses and contact lenses, drug and alcohol treatment, psychological services, surgical procedures, hospitalization, and prosthetic devices. *Maintenance* includes cash payments to facilitate everyday living and covers such items as transportation, clothing, motor vehicle and/or home modifications, and services to family members. *Placement* includes employment services, job development, and job retention. *Job Supports* includes job coaching and supported employment.

Following Clapp et al. (2019, 2024), we include two additional service types for BVI cohorts. *Assistive Technology (AT)* includes those services the agency reports as "rehabilitation technology" to RSA. Examples include low-vision devices, assistive listening devices, and augmentative communication equipment. *Orientation & Mobility (O&M)* includes those services the agency reports as "disability-related augmentative skills training" to RSA. Examples include low-vision training, orientation and mobility training, travel training, and home training. Because these two types of services are seldom provided to non-BVI cohorts, we include them in *Restoration* for non-BVI cohorts.

Tables 5.4, 5.5, and 5.6 report the percentage of individuals receiving services by service category, agency, and disability type. For simplicity and because there is more variation in service provision across agencies than across non-BVI disability

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[6]These aggregation rules change over cohorts. For example, the 2012 cohort data used for this analysis predate the 2014 Workforce Innovation and Opportunity Act (WIOA), which affects categorization for more current cohorts.

[7]We use *italics* for service types to avoid confusion.

Table 5.4 Percentage of individuals receiving purchased services by agency and disability type

Service type	VA		MD		KY	NC		TX	
	Non-BVI	BVI	Non-BVI	BVI	Non-BVI	Non-BVI	BVI	Non-BVI	BVI
D&E	35%	34%	62%	49%	73%	46%	65%	81%	94%
Training	18%	18%	19%	24%	13%	16%	4%	28%	20%
Education	2%	9%	7%	11%	17%	9%	6%	10%	7%
Restoration	18%	44%	8%	15%	22%	56%	56%	41%	55%
Maintenance	39%	45%	32%	61%	29%	40%	20%	47%	64%
Placement		6%	34%	15%	20%	2%	0%	15%	2%
Job supports	27%	9%	18%	8%	8%	26%	2%	16%	8%
AT		24%		59%			33%		66%
O&M		7%		19%			0%		34%

Notes: (1) Virginia's general agency does not purchase *Placement* services. (2) *AT* and *O&M* are included in *Restoration* for non-BVI samples. (3) We did not collect BVI data for Kentucky

Table 5.5 Percentage of Virginia applicants receiving services by service source and disability type

Service type	Non-BVI				BVI			
	Neither	PS only	Agency only	Both	Neither	PS only	Agency only	Both
D&E	49%	24%	16%	11%	60%	27%	6%	7%
Training	76%	15%	6%	3%	82%	18%	0.1%	0.2%
Education	98%	2%			91%	9%		
Restoration	75%	16%	7%	2%	56%	44%		
Maintenance	56%	33%	5%	6%	54%	45%	0.0%	1%
Placement	79%		21%		86%	5%	8%	0.5%
Job supports	73%	27%			91%	9%		
AT					68%	8%	8%	16%
O&M					71%	2%	22%	5%

types, the CI, MI, and PI samples are combined into non-BVI for these tables.[8] Table 5.4 focuses on services purchased from an external vendor and reveals considerable variation in purchased services across agency, disability type, and service type. As noted in Sect. 5.2.1, the percentages reported for Texas are not directly comparable to the other states because they are based on applicants who completed an IPE, while percentages for other states are based on all applicants. As expected, the Texas percentages are generally higher than those for other states and often much higher.[9] This cannot be said for the other four states. No state

[8]Tables for all states and all disability types are provided in Appendix Tables A.2–A.6.

[9]Nevertheless, the Texas percentages are more interpretable for VR administrators because administrators generally think in terms of their "consumers," i.e., applicants who completed an IPE. For the same reason, percentages for other states likely appear to be low to VR administrators

Table 5.6 Percentage of Maryland applicants receiving services by service source and disability type

	Non-BVI				BVI			
Service type	Neither	PS only	Agency only	Both	Neither	PS only	Agency only	Both
D&E	34%	44%	4%	18%	30%	21%	21%	27%
Training	76%	16%	4%	3%	72%	18%	5%	6%
Education	93%	7%			89%	11%		
Restoration	92%	8%			85%	15%		
Maintenance	67%	31%	0.4%	1%	38%	59%	1%	3%
Placement	66%	34%			81%	14%	3%	1%
Job supports	82%	18%			92%	8%		
AT					34%	25%	6%	35%
O&M					66%	10%	15%	9%

consistently provides more or less of a service than the other three. Nevertheless, there is considerable variation in service provision across agencies, disability types, and service types.

Although there are some notable differences between service allocations for non-BVI and BVI clients across states, the differences are not systematic. The largest disparities occur when shares for BVI disabilities exceed those for non-BVI: 61% vs. 32% for *Maintenance* in Maryland and 44% vs. 18% for *Restoration* in Virginia. North Carolina has the two largest differences whereby non-BVI exceed BVI shares: 26% vs. 2% for *Job Supports* and 40% vs. 20% for *Maintenance*. Finally, with respect to service type, *Education* is the least purchased service across agencies and disabilities. By contrast, *Diagnosis & Evaluation* is one of the top two purchased services in most cases. At 81% of non-BVI and 94% of BVI individuals, Texas stands out for its purchase of *Diagnosis & Evaluation*.

Table 5.5 for Virginia and Table 5.6 for Maryland provide additional details on service provision: specifically, a breakdown by source for the only two states for which we observe agency-provided services. Services can be provided by purchase only, through the agency only, by both, or not at all.[10]

Table 5.5 focuses on Virginia and reports the percentage of individuals who were provided purchased services by source, service category, and disability type.

because they include all applicants. For purposes of comparison with Texas, Appendix Table A.7 restricts its focus to applicants who have completed an IPE and reports the proportion receiving purchased services by service type.

[10] Although counselor services are not recorded by either state, we do observe services that are provided by state-operated comprehensive rehabilitation centers (SOCRCs) and funded by the VR agency. These centers are important sources of services provided in-house. The SOCRC for Virginia's General agency is the WWRC, which operates independently; for Maryland's Combined agency it is the WTC, which is housed in the same facility as the VR agency. By contrast, Virginia's Blind agency operates its own center, the Virginia Rehabilitation Center for the Blind and Vision Impaired (VRCBI).

Beginning with non-BVI disabilities, the table shows that, at 51% (= 1 − 0.49), *Diagnosis & Evaluation* is the most commonly provided service: 24% receive it only through purchase, 16% only from the WWRC, and 11% from both sources. The second most common is *Maintenance* at 44%, with no other service provided to more than 27% of applicants. Least common is *Education*, which only 2% of applicants receive. Although the agency does not purchase *Placement* services, the WWRC does provide it to 21% of applicants. Given that very few applicants receive *Assistive Technology* or *Orientation & Mobility*, those are included in *Restoration*.

In contrast to the non-BVI portion of the table, in which it is first, the BVI portion shows that *Diagnosis & Evaluation* is the third most common service at 40%. The two most prevalent are *Maintenance* (46%) and *Restoration* (44%). Of the first seven service categories, purchased services dominate all but *Placement*. The agency plays its most significant roles in providing *Orientation & Mobility* (22% alone and 5% in combination with purchased services) and *Assistive Technology* services (8% alone and 16% in combination). Unlike Virginia's General agency, Virginia's Blind agency operates its own in-house training center, for which we have incomplete data. Consequently, agency services are higher than reported in this table and, in particular, for *Assistive Technology* and *Orientation & Mobility*.

Table 5.6 provides the same type of breakdown for Maryland. *Diagnosis & Evaluation* is the most commonly provided service at 66% of non-BVI applicants (44% only through purchase, 4% only by the WTC, and 18% by both) and 70% of BVI applicants. For non-BVI clients, service provision drops off substantially after that to 34% for *Placement* and 33% for *Maintenance*. Other than for *Diagnosis & Evaluation* and *Training*, the WTC plays a relatively minor role in service provision for non-BVI applicants. For BVI clients, over 60% of clients receive *Maintenance* and *Assistive Technology,* and the WTC provides notable *Diagnosis & Evaluation, Training, Maintenance, Placement, and Assistive Technology and Orientation & Mobility* services.

5.2.3 Employment and Earnings

We estimate service impacts with respect to two labor market measures: quarterly employment and quarterly earnings (if employed).[11] Each state's VR agency coordinates with its unemployment insurance agency to provide us with quarterly earnings for at least nine quarters prior to the application quarter and at least 20 quarters after. Depending on the state, we have between 33 and 60 separate quarterly observations for each individual regarding employment status (defined as having quarterly earnings above zero) and nominal earnings (if employed). In the VR-ROI model, we separate quarterly observations into four periods (see Chap. 3):

[11] More precisely, we estimate the impact of service receipt on the probability of employment in a quarter and the log of earnings conditional on being employed in the quarter. See Chap. 3 or an introductory college math book for help with understanding "log."

1. Two or more quarters prior to the application quarter. We use this period as a baseline against which we measure service impacts.
2. The quarter immediately preceding the application quarter. Employment and earnings are known to drop in the periods before individuals apply to workforce development programs. To account for this decline, referred to as the Ashenfelter (1978) dip, we explicitly allow service effects to vary in this quarter so that it does not bias the remaining pre-service quarterly labor market variables.
3. The first eight quarters after application (the "short" run), when many individuals are receiving VR services.
4. More than eight quarters post-application (the "long" run), when most participants' cases have been closed.

This subsection describes observed trends in the labor market variables over these periods. We caution against overinterpreting these trends because they do not control for anything other than disability type and agency. Many other factors influence such trends, some of which are specific to individuals and are described in Chap. 3.[12] Others are specific to the economic health of the United States and local economies, which varied significantly across these four periods. The pre-application period encompasses the last two quarters of 2008 through the first two quarters of 2011, a period strongly affected by the 2008 financial crisis and ensuing Great Recession. The unemployment rate peaked at 9.9% in the fourth quarter of 2009 and fell gradually after that. The effects of these trends can be observed in the average U.S. unemployment rates of 8.9% for SFY 2009–2011 (roughly the pre-application period); 8.5% for SFY 2012; 7.3% for SFY 2013–2014 (roughly the short run); and 5.1% for SFY 2015–2017 (roughly the long run) (Bureau of Labor Statistics, 2023). As discussed in Chap. 3, the VR-ROI model explicitly accounts for local economy effects using data on the local employment rate (i.e., employed/working-age population).

Table 5.7 reports employment rates and mean quarterly earnings (if employed) for North Carolina's General agency by disability type and period. Although the table is specific to North Carolina, general patterns are similar across agencies.[13] There is considerable heterogeneity across disability type in both levels and trends. With respect to employment rates, the Ashenfelter dip between the pre-application period (2 or more quarters before application) and the quarter before application is evident for the MI and PI cohorts but not for the CI cohort, in which the employment rate remained constant at 19% between these two periods. Post-application rates rose for the CI and MI cohorts and were highest in the long run. In stark contrast, the PI employment rate fell from the pre-application period to both post-application periods. Thus, the PI employment rate was the highest across disabilities

[12] Appendix Tables A.8–A.12 provide descriptive statistics as well as impacts on employment and earnings (if any) of the other factors. A separate table is provided for each state and each disability type.

[13] Results for the full set of states are provided in Appendix Tables A.13–A.17.

Table 5.7 Employment rates and mean nominal quarterly earnings (if employed) for North Carolina's general agency by disability type and period

	Employment			Mean quarterly earnings if employed		
Descriptive statistic	CI	MI	PI	CI	MI	PI
# of applicants in cohort	6131	8085	6239	6131	8085	6239
% employed						
2 or more Qtrs before application	19%	25%	33%	$1867	$2306	$3408
1 quarter before application	19%	22%	27%	$1569	$1664	$2574
First 8 Qtrs after application (short run)	30%	29%	28%	$1671	$1800	$2459
More than 8 Qtrs after app. (long run)	41%	32%	29%	$2155	$2037	$2698

Note: The table summarizes quarterly observations. Thus, for example, the number of observations for the row, "First 8 Qtrs After Application," is eight times the number of applicants

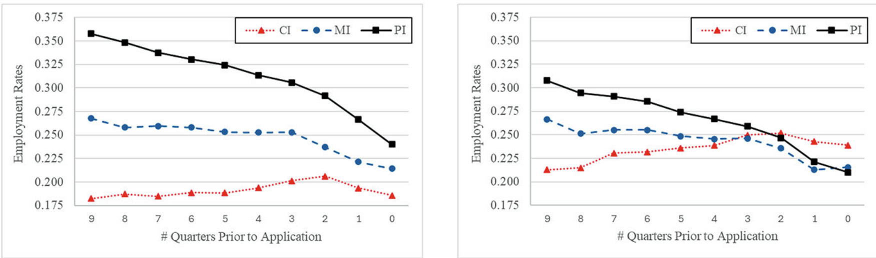
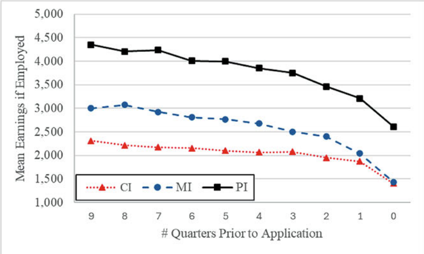


Fig. 5.1 Quarterly employment rates prior to application by Disability Cohort for North Carolina (left panel) and Maryland (right panel)

pre-application but lowest in the long run. In contrast, for the CI cohort the employment rate was the lowest pre-application but highest in the long run. With a few exceptions, these patterns hold across all agencies.

The second set of columns in Table 5.7 report mean quarterly earnings for applicants who were employed in the quarter. The Ashenfelter dip is evident for all disability cohorts, ranging from about $300–$800 in North Carolina and $250–$1100 across the other four states. In contrast to employment rates, mean earnings (if employed) were lower in the short run when compared with the pre-application period for all disability types and all states. Although they were lower in the long run for the MI and PI cohorts in all states, they were higher in all states for the CI cohort. For all states, mean earnings were highest for the PI cohort in all periods, but the gap between it and the other two disability types narrowed over time. Pre-application earnings were higher for the MI than the CI cohort in all states, but the gap narrowed considerably over time for all states and reversed itself for North Carolina and Virginia by the long-run period.

Although pre-application employment and earnings play an important role in estimating the labor force impacts of VR service provision, we have no direct knowledge of the individual dates of disability onset. Plausibly, the date differs across disability types. Figures 5.1 and 5.2 explore that possibility by charting



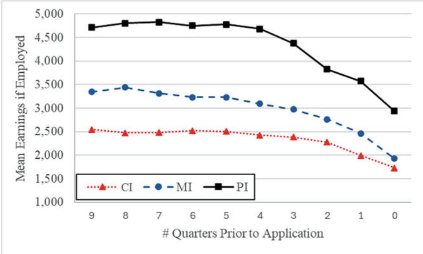


Fig. 5.2 Mean nominal quarterly earnings (if employed) prior to application by Disability Cohort for North Carolina (left panel) and Maryland (right panel)

quarterly employment rates and mean nominal earnings (if employed) for nine quarters leading up to application, separately by disability cohort and agency.

Figure 5.1 displays quarterly employment rates prior to application for the CI, MI, and PI disability groups in North Carolina (left panel) and Maryland (right panel).[14] Employment rates for both states and each disability type drop in the quarters prior to VR services application. In both states, employment rates for the PI group fall throughout this period but, consistent with an Ashenfelter dip, fall more quickly beginning in the second quarter before application. In contrast, employment rates for the CI and MI are much more stable or even increasing until the third or second pre-application quarter, when they begin to decline. At the beginning of the period, employment rates in both states are highest for the PI cohort, second highest for MI, and lowest for CI. Although the gaps between disability types decline, the rank order holds throughout the pre-application period in North Carolina. In Maryland, however, employment rates are similar by the second pre-application quarter. Beyond that quarter, the employment rate falls more slowly for the CI cohort, which exhibits higher employment rates for the final two quarters.

Figure 5.2 displays mean quarterly earnings (if employed) prior to application for the CI, MI, and PI disability groups in North Carolina (left panel) and Maryland (right panel). Three observations with respect to Fig. 5.1's employment rates are also true for Fig. 5.2's mean quarterly earnings: (1) The pre-application dip is noticeable for all disability types in each state. (2) The PI disability group has the highest mean quarterly earnings, followed by MI and CI. (3) Mean quarterly earnings during the pre-application period dip fall faster for the PI group than for the CI and MI groups. However, there are two noteworthy differences between the employment and earnings charts for Maryland. (1) While employment rates fell throughout the pre-application period for the PI group, their mean quarterly earnings remained stable until the fourth quarter prior to application before declining quickly. (2) The employment rate for the PI group fell from first to last place during the

[14] Results for the full set of states are provided in Appendix Fig. A.1. Although there are some notable differences across the states (see the employment trends in Kentucky, which rise rather than fall in the quarter prior to VR application), the patterns are mostly similar (Figs. A.2–A.11).

pre-application period, but their mean earnings ranked first in all pre-application quarters.

Although many factors affect trends in employment and earnings, we reiterate that the data in these descriptive tables and charts account only for an applicant's disability and the state agency. For that reason, the main lessons at this stage are that there are significant differences across agencies and disability types in terms of pre-service employment and earnings. To account for these differences, any model that estimates the labor market impacts of service provision should include controls for pre-service employment, pre-service earnings if employed, agency, and disability.

5.3 Estimated Short- and Long-Run Effects of VR Services

For each agency and disability type, we estimate separate labor market impacts for the seven to nine service types; the available sources (purchased services for agencies other than Maryland and Virginia, for which we observe three sources: purchased services only, agency provided only, or both); and each of the four periods.[15] The VR-ROI model, as described in Chap. 3, controls for many observable (individual and labor market) characteristics in an attempt to ensure that our estimated changes result from provision of the service rather than extraneous factors that are correlated with provision of the service.

In Sect. 5.3.1, we summarize estimated short- and long-run service effects for each cohort.[16] To illustrate the basic findings, Figs. 5.3 and 5.4 display the estimated long-run (panel A) and short-run (panel B) effects of the seven vocational rehabilitation services on employment and earnings for employed individuals with different types of disabilities (CI, MI, PI) in North Carolina and Texas. A full set of results for all states and disabilities are provided in Appendix Tables A.13–A.36. Overall, we find substantial heterogeneity across states, service types, disability types, and the short versus long run—yet there are also some commonalities.

In Sect. 5.3.2, we discuss how to interpret and understand some of the key differences between the results of this analysis, which uses data from 2012 VR applicants in five states, and those found in Dean et al.'s (2015, 2017, 2018, 2019) analyses, which use data from 2000 VR applicants in Virginia. In particular, the estimated effects of the different VR services from our analysis of the 2012 program are often negative, while those from the 2000 program are nearly all positive.

[15] Distinguishing by service source, when possible, differs from our earlier work (Dean et al., 2015, 2017, 2018) and provides more nuance regarding service impacts. In total, we estimate 828 service impacts for employment propensity and another 828 for nominal earnings (if employed).

[16] Short-run effects are found by differencing the estimated effect in the first eight quarters post-application from the effect two or more quarters prior to application. Long-run effects are found by differencing the estimated effect from nine quarters or more post-application from the effect two or more quarters prior to application.

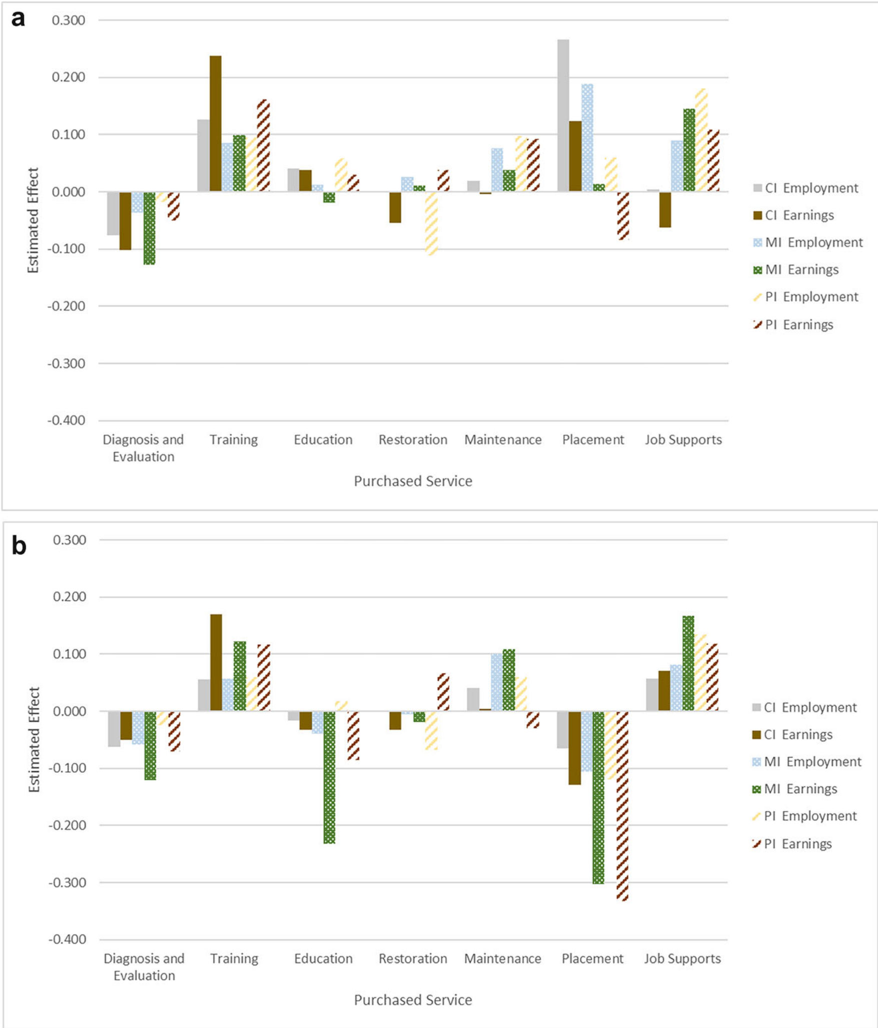


Fig. 5.3 (**a**) Long-run return on VR services in North Carolina. (**b**) Short-run return on VR services in North Carolina

5.3.1 Estimated Effects of VR Services

In the following, we focus on North Carolina and Texas and summarize estimated long-run effects by service type (Figs. 5.3a and 5.4a):

- ***Diagnosis and Evaluation*** (mixed effects)**:** In North Carolina, the estimated effects are negative across CI, MI, and PI for both employment and earnings. In contrast, in Texas there is a slight positive effect on employment for CI, MI, and

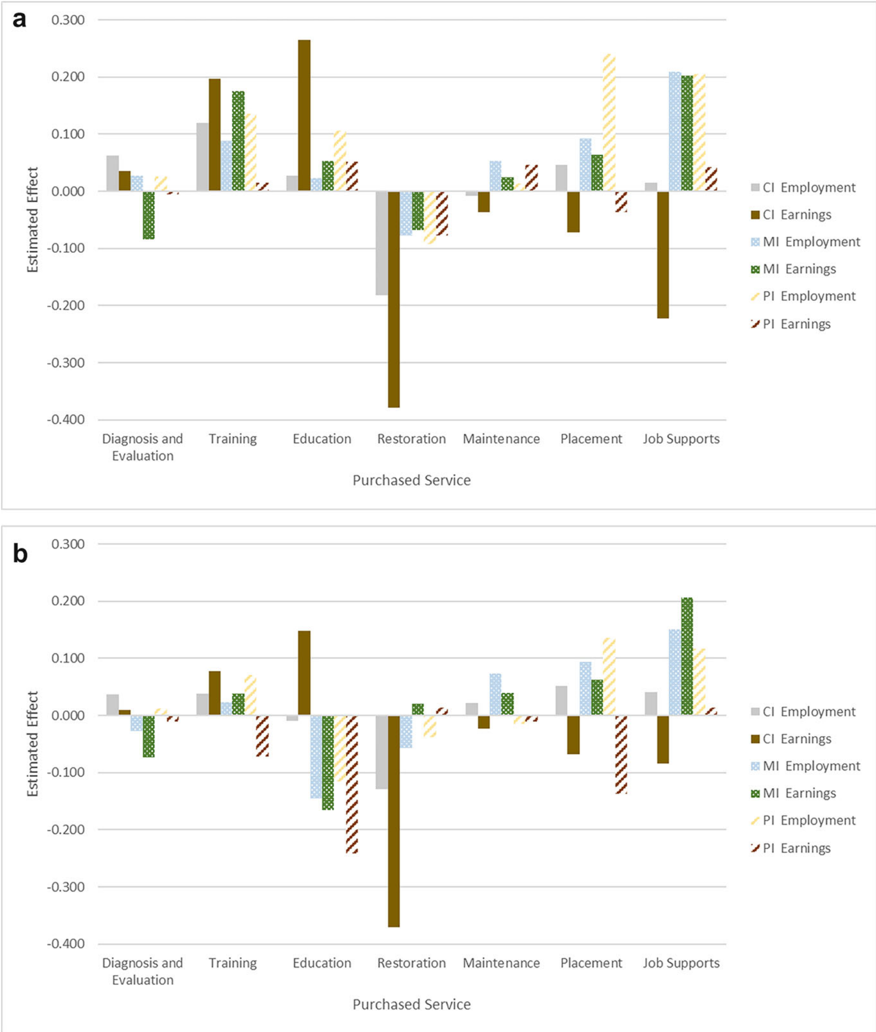


Fig. 5.4 (**a**) Long-run return on VR services in Texas. (**b**) Short-run return on VR services in Texas

PI and a positive effect on earnings for CI. For BVI (not shown in the figures), the estimated effects for employment and earnings are positive in Texas and North Carolina.

- ***Training*** (positive effects)**:** The estimated effects are positive across all disability groups and states for both employment and earnings. Likewise, the estimated long-run effects of *Training* are also positive in Virginia and Maryland (see Appendix Tables A.13–A.14). The exception is Kentucky, where the estimated effects are negative for clients with PI.

- ***Education*** (generally positive effects)**:** With one exception (MI earnings in NC), estimated effects for employment and earnings are positive, with somewhat larger results for Texas than those for NC. In particular, the effect on earnings for CI is notably high in Texas. Long-run effects are also estimated to be positive in Kentucky and Virginia. In Maryland, however, the effects are negative for clients with CI.
- ***Restoration*** (mixed effects): Estimated effects are negative and substantial across all disability categories in Texas. This is in stark contrast to North Carolina, where the estimated effects are relatively small and of mixed sign.
- ***Maintenance*** (positive effects for MI and PI, but mixed effects for CI)**:** Estimates are small and even negative for employment and earnings for CI, but positive for MI and PI. Effects in North Carolina and Texas exhibit the same basic pattern, but the North Carolina estimates are somewhat larger than those for Texas. In Kentucky, the estimated effects are positive for all three disability groups, while in Virginia, the estimated effects are generally negative.
- ***Placement*** (positive effects for employment but mixed effects for earnings): Estimated effects are large and positive on employment for CI, MI, and PI; however, *Placement* has a negative effect on earnings for PI in both states and CI in Texas. In Kentucky, estimated effects are positive for employment and earnings.
- ***Job Support*** (mixed effects)**:** For CI, there is a large negative effect on earnings, but the estimated effect on employment is negligible. There is a strong positive effect for MI and PI on employment and earnings. For those with BVI (not shown in the figure), estimated earnings effects are very positive for clients in North Carolina but negative for those in Texas.
- ***Assistive Technology* and *Orientation & Mobility*** (positive for *Assistive Technology* and mixed for *Orientation & Mobility*): For the BVI subgroups (not shown in the figures), the effect of *Assistive Technology* on employment and earnings is positive. The effect of *Orientation & Mobility* on earnings is negative, and the effect on employment is mixed; positive in North Carolina but negative in Texas.

These estimates reveal some consistent themes regarding the impact of VR services across states and disability, but also some notable differences. *Training* and *Placement*, for example, have strong positive impacts on employment for nearly all disability groups, while the impact on earnings is more variable for *Placement* services. The effect of services such as *Job Support* is much more variable; services are particularly beneficial for BVI in terms of earnings but have a substantial negative effect on earnings for those with CI. Clearly, not all services are beneficial across the board; for example, *Diagnosis & Evaluation* is estimated to have a generally negative effect on earnings for the CI, MI, and PI groups in North Carolina but a positive effect in Texas. *Restoration* has a large negative effect on employment and earnings in Texas but mixed effects in North Carolina.

Figures 5.3b and 5.4b display estimated short-run effects in North Carolina and Texas, respectively. In some cases, short- and long-run estimates are similar (e.g., *Job Support*). In many cases, however, the estimates differ notably in magnitude and even sign. Perhaps the most striking differences are found in the estimated effects of *Education*, which are generally positive in the long run but negative in the short run. For example, in Texas, *Education* is estimated to decrease the employment probability of clients with MI by 0.145 in the short run but increase the employment probability by 0.105 in the long run. Similarly, for *Placement* services in Virginia, the effects are generally large and positive in the long run but large and negative in the short run.

5.3.2 Interpreting Negative Estimated Effects of Some VR Services

How can we reconcile these results with the analogous results in Dean et al. (2015, 2017, 2018, 2019) which are almost all positive? Although these sets of studies use similar data and methods, two key differences are important to highlight. First, Dean et al. examine the Virginia VR program in SFY 2000, while the results in this chapter examine VR programs in SFY 2012. Second, the data used by Dean et al. (2015, 2017, 2018, 2019) include employment and earnings for up to 36 quarters post-service receipt, while the results from this analysis include labor market measures 20 quarters post-service receipt.

The environment in which VR operated in 2012 was much more challenging than in 2000. The Rehabilitation Service Administration's report for Federal Fiscal Years (FFY) 2014–2015 depicts a difficult period beginning around 2009 (U.S. Department of Education, 2018, pp. 24–26). The total number of employment outcomes achieved by participants receiving VR services dropped by about 12% in FFY 2009 compared with the previous 3 years (from an average of 205,412 to 180,539) and another 5% in FFY 2010 (to 171,964). The report linked this decline to several factors:

- "RSA policies that encouraged VR agencies to serve individuals with significant disabilities, especially those with the most significant disabilities, and focused efforts on assisting these individuals to achieve high-quality employment outcomes that are consistent with their aspirations and informed choices";
- "VR agencies' implementation of an order of selection policy. In PY 2010, of the 80 State VR agencies, 35 reported that they could not serve all eligible individuals and implemented an order of selection. Agencies operating under an order of selection policy must give priority to serving individuals with the most significant disabilities";
- "Increases in cost of services, such as tuition costs, which reduced the availability of resources for individuals with disabilities for other services that lead to employment outcomes"; and
- "Employment outcomes that began increasing each year starting in 2011 but remained below the FY 2008 level (employment outcomes for FYs 2014 and 2015 totaled 183,452 and 186,234, respectively)."

The implementation of an order of selection (OOS) policy merits additional discussion. We have worked most extensively with Virginia's General agency (VA DARS) and were provided with their record of OOS category closings and openings between 2004 and 2016. Most importantly, although all categories were open in 2000, all categories were closed for the first 6 months of SFY 2012. At that point the most significant disability (MSD) category was opened for those who applied before the second month of SFY 2012. The cutoff month was gradually extended throughout the remainder of SFY 2012. All categories were closed again in November 2012. In short, only the MSD category was open to SFY 2012 applicants, none were able to receive services for at least the first 6 months, and then were available only to those applying earlier in the year. This contrasts with SFY 2000, during which a mix of applicants in the most significant, significant, and non-significant categories received services throughout the year.

Such a strict OOS policy is likely to have subtle but important effects within our model. Specifically, VR counselors are trained to do everything possible to assist their clients in gaining employment. We have been told by staff in several agencies that this could include directing clients to services outside the agency. In our model, individuals who received services outside the VR agency would be part of the comparison group, and thus might reduce the estimated impact of services provided by the VR agency. To the extent that this occurs, it could have important effects on the estimates.

Beyond the factors identified by the RSA, there are other possible explanations for the negative results reported in this chapter:

- First, there may be important programmatic and environmental changes from 2000 to 2012 that explain why some estimates changed from positive to negative over this period. Some examples are:
  - Non-VR programs, including those that provide on-the-job training, may have improved to the point that they are now more effective than VR services, even if use of them did not change;
  - The same fiscal challenges that led to the implementation of OOS might have increased the size of VR caseloads and decreased expenditures per case, and resulted in services that are not as effective.

  We underscore that there have been significant changes to the VR program since 2012, including WIOA (see Chap. 1), that may have notably impacted the efficacy of VR. The effects of the VR program in 2024 are not likely to be the same as the effects in 2012 or 2000. We are currently developing and applying new VR-ROI models to evaluate the post-WIOA VR program in North Carolina (see Chap. 7 for further details). Although the VR-ROI model can be applied in this setting, estimating the long-run effects of VR, which we have found to be critical to fully understanding the efficacy of the program, takes time.
- Second, a 5-year post-service window may not be sufficient for estimating the "true" long-run effects of VR services. With more data further into the future, long-run estimated benefits might become positive. This is consistent with results found in Dean et al. (2015, 2017, 2018, 2019).

- Finally, the model may be mis-specified or there may be other data-related problems that cause incorrect negative estimates, especially for the 2012 cohorts. For example, we do not observe and thus do not model agency-provided VR services. If these VR services are highly effective, it could cause us to incorrectly conclude that VR services are ineffective. Likewise, the receipt of disability insurance and arguably nonpecuniary benefits of VR service receipt (e.g., improved independent-living skills) are not included in the model (or observed in our data). This may result in negative estimated effects of VR services if individuals and their counselors are making service choices that optimize over these outcomes and those choices deviate from what is optimal with respect to the labor market alone (see Chap. 2, footnote 37, for an example).

We do not know how much any of these are causing the differences between the earlier studies and the negative results found based on data on the 2012 cohorts. They each require serious thought and careful future research. In Chap. 7, challenges and directions for future research are discussed in more detail.

5.4 Discounted Returns, Cost, and Net Present Value of VR

Although the results summarized in Sect. 5.3 provide details on the short- and long-run effects of different VR services, these estimates do not directly uncover economic returns on the VR program. In this section, we delve into the economic implications of state VR programs across the five states: Virginia, Maryland, Kentucky, North Carolina, and Texas. For each state and each of the four disability groups (CI, MI, PI, and BVI), we provide summary measures of the labor market benefits and costs associated with VR programs.

To do this, we must account for the fact that clients receive different service bundles, and the benefits of those services may accrue over many years. Thus, as discussed in Chap. 3, to assess the net present value (NPV) of the VR program we take the following three steps. We (1) use the estimated effects discussed in Sect. 5.3 to assess the present discounted value of the actual VR services provided to each client, (2) determine the cost of VR services for each client, and (3) compute the NPV by subtracting the cost from the present discounted value of the benefit.

We begin in Sect. 5.4.1 by examining the estimated aggregated labor market returns for participants in VR programs and quantify the average monetary labor market benefits accrued by VR clients with different types of disabilities in each state. To determine the discounted value of VR benefits, we first use the estimated short- and long-run effects of the different VR services (see Sect. 5.3) to compute long-run labor market earnings with and without the VR services provided to the client.[17] Long-run earnings are extrapolated 20 years into the future, with the

[17] The short run includes the first eight quarters after application, at least part of which includes service provision for those with an IPE. Thus, to the extent that there is an opportunity cost to the

difference in earnings in each quarter discounted using an annual rate of 0.974 to determine the present value of the VR program.[18]

Then, in Sect. 5.4.2, we discuss the cost of providing these VR programs and break costs into fixed and variable (or purchased services) components. By understanding the cost structure, we can assess the programs' efficiency across different states.

Finally, in Sect. 5.4.3, we integrate benefits and cost by calculating the distribution of the NPV of each state's VR program. The VR-ROI model enables us to compute the NPV for each client. NPVs vary across clients within a particular disability group in a state because (1) they choose different combinations of services and (2) they have different demographic variables that interact with service provision; see Chap. 6 for more discussion of this point. In this chapter, we report summary measures, which include the mean, median, and outer percentiles of the distribution of NPV.

The NPV shows the long-run economic value of investing in VR for an individual with disabilities and provides a critical lens through which policymakers and stakeholders can evaluate and compare the effectiveness of labor market training programs and VR services. By using this financial lens, we aim to offer nuanced understanding of the labor market value VR programs provide to individuals with disabilities (Sect. 5.4.1) and the broader society (Sect. 5.4.3). This, in turn, will enable informed decision-making for strategic investments in the future of VR. Importantly, however, this analysis only accounts for the labor-market benefits of VR.

5.4.1 Present Discounted Value of Labor Market Benefits

Figure 5.5 displays the mean present discounted value (PV) of the benefits of VR services for individuals with CI, MI, and PI for each of the five states: Virginia, Maryland, Kentucky, North Carolina, and Texas. In addition to the mean, Table 5.8 reports median and outer percentiles (75%, 90%, and 95%) and reports estimates of the PV of benefits for those with BVI. Without subtracting costs, these PV estimates yield insights into the labor market benefits of VR services for VR clients.

Table 5.8 shows generally positive but substantial variation in mean benefits across states and disability groups. In particular, the figure shows:

- **Virginia (VA)**: positive mean PV for all cohorts, with the lowest for clients with MI ($292) and the highest for those with BVI. Excluding those with BVI, the overall average PV is $787. Across all disabilities, the average is $1128.

client while receiving services, those are accounted for in calculating the present value of the VR program for the individual.

[18] This rate is consistent with the U.S. Office of Management and Budget's (OMB) recommendation to use a 2.6% annual discount rate for the evaluation of public programs (OMB, 2018).

Fig. 5.5 Mean Present value of benefits by disability and state agency (excluding BVI)

- **Maryland (MD)**: positive mean returns for all cohorts, with the lowest for clients with BVI ($226) and the highest for those with PI ($1931). Excluding those with BVI, the overall average PV is $1068. Across all disabilities, the average is $925.
- **Kentucky (KY)**: negative average returns for the CI and PI cohorts, but a positive average return for clients with MI. The average PV across all disabilities is negative (−$1367).
- **North Carolina (NC)**: negative average returns for the CI cohort (−$7020) but positive average returns for the other three disability groups. Excluding those with BVI, the overall average PV is −$86; across all disabilities, the average is $2857.
- **Texas (TX)**: positive average returns for all cohorts except for those with PI. Excluding those with BVI, the overall average PV is $989; across all disabilities, the average is $1423.

For clients with BVI, we estimate positive average benefits in all four states: Virginia ($1128), Maryland ($226), North Carolina ($11,348), and Texas ($6207). Recall that Maryland has a Combined agency, while the other three states had a separate Blind agency at the time.

Overall, these results suggest that VR generally improves labor market outcomes. Of the 19 applicant cohorts examined in our analysis, 15 are estimated to have positive mean benefits from VR services. Mean benefits for clients with MI and BVI are estimated to be positive in every state, which suggests that VR services are particularly beneficial for these groups. For clients with CI and PI, the benefits are positive in three states but negative in two states. For example, for clients with CI, the average present value of VR is estimated to be positive in Virginia, Maryland, and Texas but negative in Kentucky and North Carolina.

Table 5.8 Present discounted value of benefits, by disability and state agency

State and disability		Present value of benefits						
		% Pos.	Mean	Std Dev	Median	75%	90%	95%
VA	CI	51.0	$1030	$13,640	$142	$5993	$15,034	$22,067
VA	MI	50.4	$292	$11,568	$72	$4725	$11,735	$17,991
VA	PI	51.8	$1152	$19,701	$384	$8284	$22,126	$34,559
VA	BVI	51.3	$2468	$103,915	$604	$24,455	$75,108	$115,115
VA	Non-BVI	51.0	$787	$14,981	$172	$5944	$15,510	$24,036
VA	All	51.0	$1128	$48,705	$201	$7328	$22,305	$40,694
MD	CI	51.2	$452	$13,782	$182	$5391	$13,799	$22,283
MD	MI	51.3	$833	$14,434	$227	$5032	$13,448	$22,138
MD	PI	51.1	$1931	$26,853	$197	$7487	$22,834	$36,592
MD	BVI	51.0	$226	$14,623	$79	$4444	$13,306	$21,511
MD	Non-BVI	51.2	$1068	$19,010	$197	$5699	$16,159	$26,480
MD	All	51.2	$925	$18,345	$174	$5539	$15,808	$25,565
KY	CI	35.3	−$9410	$28,714	−$5654	$4087	$16,117	$27,073
KY	MI	59.5	$3877	$18,679	$2459	$11,087	$23,297	$34,500
KY	PI	41.8	−$2880	$22,636	−$2080	$6632	$19,400	$29,440
KY	Non-BVI	48.1	−$1367	$23,186	−$441	$8506	$20,900	$32,404
NC	CI	55.9	−$7020	$27,920	$1812	$8248	$14,440	$19,691
NC	MI	67.5	$4949	$11,587	$2540	$9478	$18,792	$25,790
NC	PI	42.6	$203	$6255	$0	$3422	$7536	$10,725
NC	BVI	63.3	$11,348	$26,538	$2535	$16,275	$44,915	$67,860
NC	Non-BVI	56.4	−$86	$17,971	$1107	$6943	$14,133	$20,314
NC	All	58.2	$2857	$21,118	$1433	$8230	$19,510	$30,730
TX	CI	60.3	$3897	$22,525	$3529	$14,156	$27,648	$39,750
TX	MI	63.6	$4029	$19,606	$3972	$12,725	$24,368	$32,952
TX	PI	48.5	−$2156	$25,067	−$434	$9033	$22,792	$34,110
TX	BVI	61.3	$6207	$27,405	$3065	$15,573	$34,885	$50,463
TX	Non-BVI	55.5	$989	$23,236	$1717	$11,796	$24,584	$35,186
TX	All	56.0	$1423	$23,655	$1834	$12,013	$25,153	$36,530

Note: Using an annual 2.6% discount rate

5.4.2 Costs: Fixed and Purchased Services

The present value of benefits is not sufficient to assess the economic return of VR; we must also account for costs. For example, if the benefit is $1000 and the cost is $5000, the economic return is negative. Our next step is to present details on the cost of providing VR services by state and disability group. There are two types of costs to consider: (1) fixed costs, such as facilities, administration, and staffing which, for the purposes of the NPV analysis, are divided equally among all clients; and (2) variable or marginal costs associated with expenses related to providing a specific

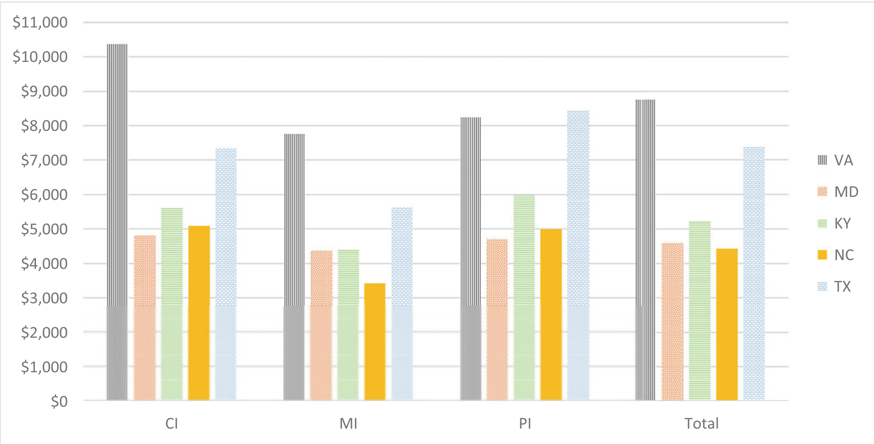


Fig. 5.6 Total average cost (FC + VC) per client by disability and state agency (excluding BVI)

service to a particular client.[19] Figure 5.6 presents a bar graph that depicts the total average costs (combining fixed and marginal) per client incurred by state VR programs for individuals with different types of disabilities across the five states. The figure does not include cost estimates for clients with BVI, because the cost of providing services is notably higher than for those with CI, MI, and PI (see Table 5.9). Each disability category has a group of five bars, with each bar representing a state's average total cost for that disability. Each state is represented by a unique pattern or color in the bars.

A quick visual inspection of Fig. 5.6 highlights two points. First, average total costs in TX and VA are notably higher than those in MD, KY, and NC. Depending on the disability group, the average cost in TX and VA varies from $5600 (MI in TX) to slightly more than $10,000 (CI in VA). For the other three states, average total costs are generally between $4000 and $5000 per client. Second, average total costs for clients with MI are slightly less than for those with CI or PI.

Table 5.9 presents a more detailed breakdown of per-client average fixed costs (FC) and variable costs (VC) (i.e., purchased services) by disability type for different state agencies and the four disability groups. In general, for clients with CI, MI, and PI, fixed and variable costs are of similar magnitudes. Notably, however, average fixed costs for clients with BVI are substantially higher than variable costs in the three states that have Blind and General agencies: VA, NC, and TX. For example, average fixed costs for BVI clients are $31,818 in VA, $14,413 in NC, and $18,961

[19]The variable cost for a client receiving a service is assigned the median cost for the service by disability type and agency. Fixed costs are estimated by multiplying the total cost of purchased services for this disability and agency by the agency's ratio of fixed costs (which mainly consists of administration, counseling, and placement) to total purchased services, as reported in the RSA-2 filed annually. That estimate of fixed costs is then spread evenly across all clients with that disability.

Table 5.9 Fixed and variable costs in $1000s by disability and state agency

State and disability		Average cost per client		Purchased service median costs								
		FC	VC	D	T	E	R	M	P	S	A	O
VA	CI	3.862	6.503	0.515	0.579		0.410	0.170		4.704		
VA	MI	3.103	4.651	0.152	0.550	2.024	0.444	0.161	0.750	3.508		
VA	PI	3.320	4.918	0.100	0.497	4.911	0.428	0.180	0.750	4.197		
VA	BVI	31.818	5.542	0.200	0.767	10.29	0.854	0.213	1.268	34.342	0.935	1.101
VA	Non-BVI	3.417	5.338									
VA	All	8.801	5.377									
MD	CI	2.385	2.434	0.489	1.846	1.706	0.462	0.212	1.300	1.084		
MD	MI	2.207	2.165	0.329	1.500	1.076	0.321	0.220	1.300	1.558		
MD	PI	2.501	2.200	0.329	1.736	1.164	0.908	0.261	1.300	1.059		
MD	BVI	4.943	4.772	0.294	3.580	2.180	0.259	0.404	0.857	1.800	1.806	0.925
MD	Non-BVI	2.344	2.249									
MD	All	2.807	2.698									
KY	CI	3.490	2.148	0.650	0.747	2.724	0.624	0.151	1.600	4.400		
KY	MI	2.784	1.641	0.344	0.850	2.919	0.649	0.177	1.800	4.400		
KY	PI	4.011	1.970	0.330	0.814	3.750	1.257	0.202	1.800	4.400		
KY	Non-BVI	3.354	1.865									
NC	CI	2.980	2.117	0.120	0.610	0.961	0.350	0.322	3.200	3.849		
NC	MI	2.099	1.327	0.030	0.023	0.748	0.350	0.426	2.200	3.406		
NC	PI	3.824	1.171	0.045	0.024	1.218	0.644	0.451	2.200	3.445		
NC	BVI	14.413	2.341	0.166	0.800	5.643	2.809	1.006		2.980	0.111	7.304
NC	Non-BVI	2.899	1.532									
NC	All	5.870	1.741									
TX	CI	3.920	3.437	0.296	1.414	1.583	0.292	0.195	2.650	4.819		
TX	MI	3.508	2.119	0.314	1.308	1.208	0.402	0.233	2.050	3.000		
TX	PI	6.183	2.246	0.470	1.485	1.425	1.500	0.250	2.050	2.100		

(continued)

Table 5.9 (continued)

State and disability		Average cost per client		Purchased service median costs								
		FC	VC	D	T	E	R	M	P	S	A	O
TX	BVI	18.961	4.000	1.412	0.750	1.219	0.839	0.473		0.999	1.617	1.502
TX	Non-BVI	4.903	2.475									
TX	All	6.110	2.606									

Notes: *FC*, fixed costs; *VC*, variable costs; *D*, diagnosis & evaluation; *T*, training; *E*, education; *R*, restoration; M, maintenance; *P*, placement; *S*, job supports; *A*, assistive technology; *O*, orientation & mobility in TX. In these states, average FC is between five and six times that of average VC. In contrast, average fixed costs for clients in MD, which has a Combined agency, are just under $5000 and that is about the same as the average variable cost.

Table 5.9 also shows median purchased service costs for the nine service categories. *Education* and *Job Supports* services are generally the costliest. The median variable costs of other services vary across states and disabilities.

5.4.3 NPV: Present Value of Benefits Minus Costs

Table 5.10 and Figure 5.7 show the average NPV for VR programs for individuals with CI, MI, and PI as well as total NPV across these three disability groups for each of the five states. Table 5.10 also reports the median and outer percentiles (75%, 90%

Table 5.10 Net present discounted value of benefits, by disability and state agency

State and disability		NPV of benefits						
		% Pos.	Mean	Std Dev	Median	75%	90%	95%
VA	CI	27.6%	−$5471	$14,760	−$4842	$933	$9922	$16,952
VA	MI	22.7%	−$9262	$15,450	−$6066	−$608	$5509	$12,069
VA	PI	32.6%	−$4405	$20,082	−$4654	$3176	$17,297	$28,946
VA	BVI	21.1%	−$29,779	$104,108	−$32,334	−$7166	$4078	$87,669
VA	Non-BVI	27.2%	−$6594	$16,832	−$5197	$738	$10,025	$18,508
VA	All	26.0%	−$11,305	$50,138	−$6863	$431	$12,200	$25,726
MD	CI	26.8%	−$4422	$13,979	−$4497	$572	$9460	$17,658
MD	MI	26.7%	−$4229	$14,781	−$4463	$484	$9186	$17,679
MD	PI	32.2%	−$2904	$26,935	−$4298	$2709	$18,327	$31,715
MD	BVI	14.7%	−$9942	$15,355	−$9765	−$4454	$3823	$13,015
MD	Non-BVI	28.4%	−$3874	$19,198	−$4441	$1064	$11,617	$22,002
MD	All	26.1%	−$4899	$18,742	−$5241	$412	$10,359	$20,844
KY	CI	21.9%	−$14,719	$28,592	−$10,812	−$1222	$10,355	$20,457
KY	MI	43.1%	−$646	$18,083	−$1822	$6046	$17,669	$28,521
KY	PI	24.8%	−$9055	$22,336	−$8332	−$75	$12,051	$22,488
KY	Non-BVI	32.3%	−$6618	$22,954	−$5361	$3141	$14,714	$26,084
NC	CI	36.4%	−$8883	$35,612	−$3292	$4193	$16,574	$29,464
NC	MI	47.6%	$1293	$21,121	−$500	$7215	$20,272	$31,994
NC	PI	32.4%	−$4761	$17,606	−$4403	$2705	$12,869	$20,064
NC	BVI	23.1%	−$6100	$30,408	−$13,470	−$1480	$28,740	$56,330
NC	Non-BVI	39.6%	−$3603	$25,863	−$2540	$5100	$17,006	$28,253
NC	All	35.4%	−$4246	$27,126	−$4140	$4394	$18,763	$33,279
TX	CI	43.3%	−$2784	$22,946	−$3193	$8325	$21,777	$34,193
TX	MI	45.6%	−$1007	$19,208	−$1114	$7461	$18,383	$27,067
TX	PI	27.2%	−$10,017	$25,122	−$8239	$1280	$15,132	$26,086
TX	BVI	17.5%	−$16,764	$27,596	−$19,882	−$7295	$11,805	$27,927
TX	Non-BVI	36.1%	−$5792	$23,432	−$4838	$5007	$17,789	$28,314
TX	All	34.6%	−$6706	$23,998	−$5606	$4528	$17,571	$28,314

Note: Using an annual 2.6% discount rate

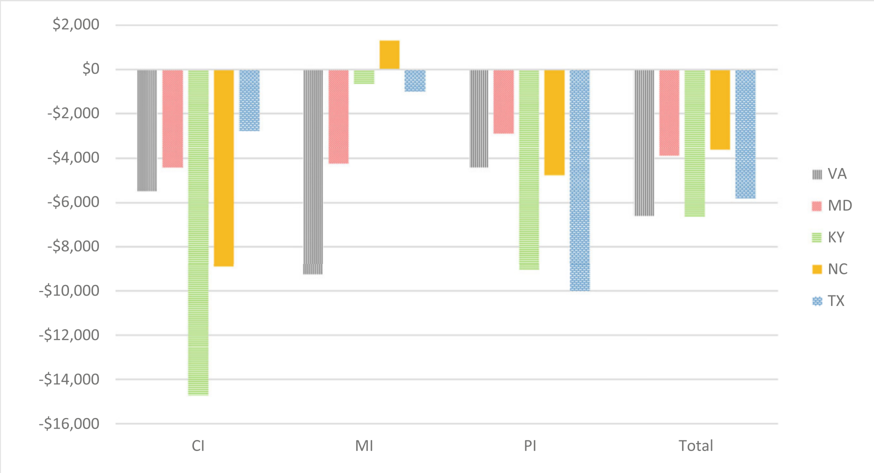


Fig. 5.7 Mean Net present value of benefits by disability and state agency (excluding BVI)

and 95%) and NPV estimates for those with BVI. These NPV estimates show the long-run economic value of investing in VR.

A striking—and sobering—finding is that the estimated mean and median NPVs are negative across all agencies and all disability types except for NC MI, which has a positive mean but negative median value. The costs of VR programs exceed the financial benefits, with the magnitude of this difference varying by state and type of disability. Average agency-wide NPV estimates are less than −$4000 per client in all five states. The rank order of average NPV estimates for non-BVI cohorts varies across states. Average NPV is lowest for clients with MI in VA; for clients with CI in MD (but MI is close), KY, and NC; and for clients with PI in TX.

Except for NC, NPV statistics for BVI are notably lower (i.e., more negative) than for non-BVI. This is mildly surprising, because Table 5.8 reveals that the present discounted value (PDV) of BVI benefits are larger in VA, NC, and TX than that of non-BVI categories. The reason is the substantially higher fixed costs for BVI, and especially for VA ($31,818 for BVI vs. $3417 for non-BVI), as shown and discussed in Sect. 5.4.2.

Table 5.10 also shows the distribution of NPV across clients by state and disability type. While mean and median NPV estimates are consistently negative, the net return for some clients may be positive. In fact, we find that the 75th percentile of the NPV distribution is generally positive, while the 90th and 95th percentiles are uniformly positive. For example, in NC, the mean total NPV is

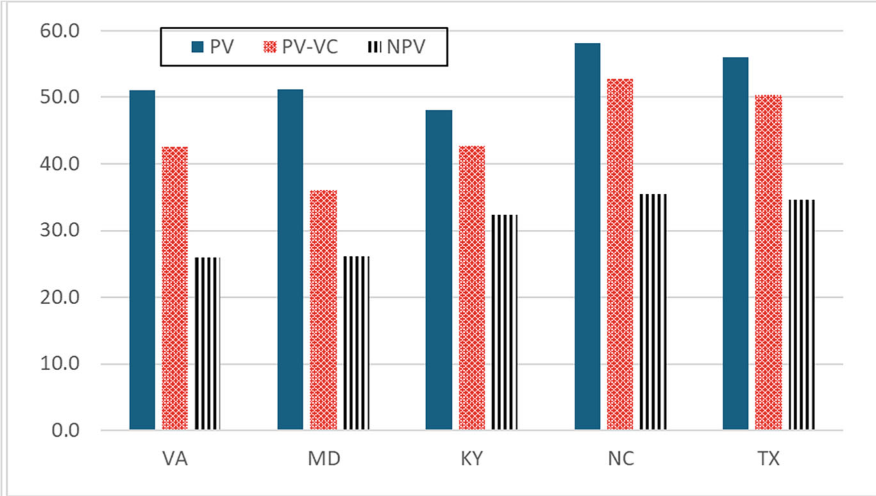


Fig. 5.8 Percentage of positive PV, PV-VC, and NPV for all clients by state

−$4246, but the 75th, 90th, and 95th percentiles of the NPV distribution are $4394, $18,763, and 33,279, respectively. Thus, 10% of VR clients in NC had NPV estimates of more than $18,763.

In summary, two factors contribute to the negative net returns of VR. First, labor market returns are generally positive but often small (see Table 5.8 and Fig. 5.5). Second, the costs of VR services are substantial relative to the benefits (see Table 5.9 and Fig. 5.6).

To provide additional insight, Fig. 5.8 shows the fraction of all clients with a positive (1) present discounted value (PV) of returns, (2) PV minus variable costs, and (c) NPV (PV minus fixed and variable costs) by state and disability group. Consider first the PV of benefits: At least 48% of VR clients in each of the five states have positive returns, ranging from 48% in KY to 58% in NC. After subtracting variable costs, these decrease to between 36% (MD) and 53% (NC). Finally, after additionally subtracting fixed costs, the percentage of clients with a positive NPV decrease to about 26% in VA and MD, 32% in KY, and about 35% in NC and TX. Thus, even for many clients with positive gross returns, the net return is negative.

Finally, we highlight an important limitation of the method used to generate the NPV estimates. In order to create the estimates reported in Tables 5.8 and 5.10, we used simulated, rather than individual-level, data on VR clients. After generating basic descriptive statistics on VR clients and estimating the effects of VR services on short- and long-run labor market outcomes (see Sect. 5.3), we lost access to individual-level data from Virginia, Maryland, Texas, and Kentucky. Unlike in previous work (Dean et al., 2015, 2017, 2018, 2019; Schmidt et al., 2019), without the individual-level data, we could not estimate NPV. Instead, using summary statistics on the means and standard deviations of each explanatory variable and estimated short- and long-run labor market effects discussed in Sect. 5.3, we used simulation methods to estimate the benefits of VR for each client. The basic idea is to use the mean and standard deviation to simulate pseudo observations that have the same average characteristics as the actual individuals in our sample. Then, given this simulated data, we estimated the benefits of VR. This simulation approach can be employed without having actual individual-level data. However, it is not the same because the joint distribution of the actual data may not be completely characterized by the means and standard deviations. Even so, it is a commonly used and validated approach in situations in which true data are not available.

For North Carolina, we still had access to individual-level data and thus were able to compare results using both approaches. Table 5.11 reports differences between feasible simulation methods and the approach that relies on individual-level data for North Carolina. In certain cases, the mean or medians are close, but there are also some significant differences (e.g., MI estimates). However, the signs are almost all consistent. Still, it should be clear that the precise numbers reported in Tables 5.8 and 5.10 depend on modeling choices.[20]

[20] There are three important differences between the simulation and data-based approaches. The first is that for the simulation method, we observe only the mean and standard deviations of the explanatory variables; in contrast, we observe the complete joint distribution of the explanatory variables in the data-based method. This causes possible bias when using the simulation method which cannot be addressed. The second is that in data-based methods, all errors are set to zero; in the simulation method, errors are simulated. The simulation of errors is probably better in theory, but it creates outlier observations that have a large effect on reported moments in the two tables. The third is that there are several variables in the data-based model that cannot be simulated. We do not know how much of a problem this causes. We evaluated the simulation model based on how well it performed in matching the medians in North Carolina, and it did pretty well. However, because of outliers caused by the simulation of errors, the method did not do as well regarding the means and standard deviations of outcome variables.

Table 5.11 Comparison of PV and NPV for North Carolina using actual vs. simulated data, by disability

	Disab	% Pos	Mean	Std dev	Median	75%	90%	95%
Present discounted value of benefits								
Based on actual values	CI	35.1	$188	3968	−$561	$1651	$5982	$7885
	MI	63.7	$1764	4228	$464	$3763	$7139	$10,103
	PI	49.3	$1137	5399	−$62	$3420	$7997	$11,559
	BVI	55.2	$6207	27,826	$980	$7276	$14,161	$23,059
Based on simulated values	CI	55.9	−$7020	$27,920	$1812	$8248	$14,440	$19,691
	MI	67.5	$4949	$11,587	$2540	$9478	$18,792	$25,790
	PI	42.6	$203	$6255	$0	$3422	$7536	$10,725
	BVI	63.3	$11,348	$26,538	$2535	$16,275	$44,915	$67,860
Net present value (NPV) of benefits								
Based on actual values	CI	12.3	−$4909	$4208	−$4822	−$3601	$990	$3351
	MI	24.9	−$1662	$3506	−$2203	−$20	$2533	$5022
	PI	15.5	−$3858	$4726	−$4870	−$2161	$1874	$4969
	BVI	8.2	−$10,547	$27,855	−$16,141	−$8297	−$1659	$6350
Based on simulated values	CI	36.4	−$8883	$35,612	−$3292	$4193	$16,574	36.4%
	MI	47.6	$1293	$21,121	−$500	$7215	$20,272	47.6%
	PI	32.4	−$4761	$17,606	−$4403	$2705	$12,869	32.4%
	BVI	23.1	−$2699	$33,135	−$11,990	$2040	$35,820	23.1%

5.5 Conclusion

This chapter presents the most complete and thorough examination to date of vocational rehabilitation (VR) programs and offers an unprecedented, detailed analysis across five states and four disability groups. Using our rigorous return on investment (VR-ROI) model and rich longitudinal data, this study accounts for the complexity inherent in VR programs in assessing the efficacy of VR across various jurisdictions and disability categories.

Our findings reveal a complex and variable picture of the efficacy of VR, in which the estimated effects of VR services vary between short- and long-term horizons, across state VR programs, across different VR services, and among disability groups. This nuance underscores the essential point that VR's influence is not homogeneous; it varies across state programs, disability groups, VR services, labor market outcomes, and the short and long run. There is no simple, punch-line conclusion about how VR affects employment and earnings. This variability highlights the importance of tailoring VR services to the unique needs of each client and demonstrates that a one-size-fits all approach does not suffice (see Clapp et al., 2024).

Overall, we find that many clients realize positive labor market returns from VR services, which affirms these programs' potential to facilitate gainful employment and enhance earnings. In general, the majority of clients in most programs experience these positive outcomes, but many do not. This prompts us to reflect on the cause of such effects and on the comprehensive value of VR services.

In further assessing the cost-benefit calculus, our analysis yields a sobering revelation: The economic returns of VR are negative; that is, the costs exceed the benefits. This raises concerns about the program's financial efficacy and invites further examination of the underlying reasons for this shortfall. We do not have a clear understanding of this finding.

Nevertheless, there are several reasons to interpret this finding with caution and humility. First, these findings stand in contrast to earlier studies, including our own, which have demonstrated substantial positive returns from VR services (Dean et al., 2015, 2017, 2018, 2019). Such discrepancies hint at potential heterogeneity over time or variations in program execution that may influence VR's effectiveness. Second, we are skeptical regarding what it means for a program to have a negative benefit (see our discussion of possible reasons at the end of Sect. 5.3). Most likely, some important aspect of the service or of the people who use the service has changed relative to 2000 or is not well modeled. We do not know what that consists of, but we are very cautious about assuming that service benefits are negative. However, the overall estimates are not encouraging, and it is pretty clear that the benefits are less than the costs. Thus, it would be worthwhile for agencies to identify how to reduce fixed costs, which have risen significantly from the cost measures in 2000 used by Dean et al. (2015, 2017, 2018, 2019).

Third, VR programs may confer additional social and individual benefits that elude direct financial quantification. However, this is beyond the scope of our economic assessment. Thus, while our analysis focuses on employment and earnings, the success of VR programs should not be evaluated solely based on labor market outcomes. VR services may have other important non-labor market benefits that improve the quality of life of VR clients and should be considered in an ROI evaluation (Fleming et al., 2013; van Nispen et al., 2020). For example, VR services are thought to be instrumental in cultivating improved independent living skills, which are critical for the overall well-being and autonomy of individuals with disabilities. Others include improvements in quality of life, self-esteem, self-confidence, and health. Such intangible gains, though not encapsulated in our current economic analysis, are vital for understanding the overall benefits of VR. Thus, one might view our estimates of the PV of VR benefits as a lower bound.

Although our findings might cast a shadow on the fiscal merits of VR, they also suggest the need for broader dialogue on the overall value of VR services and emphasize the necessity for further exploration of the nuanced interplay of the economic, social, and individual factors that define the success of vocational rehabilitation programs. Future research on the impact of VR should thus consider other outcomes.

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6 Simplifying the Model

6.1 Introduction

Although the VR-ROI model provides a state-of-the-art approach for return on investment (ROI) analysis of Vocational Rehabilitation (VR) programs, the model's complexity can render interpreting and assessing the benefits of VR challenging. Moreover, the models are difficult to estimate and require advanced computational methods, statistical knowledge, programming skills, and computing resources. This complexity can make it prohibitive for VR agency staff to estimate and use such models to evaluate the ROI of VR programs in other states and time periods.

Given these practical concerns, a critical issue is determining whether a simplified model and estimator can provide credible agency-specific ROI estimates. Focusing on the North Carolina program as a case study, we estimate the benefits and net present value (NPV) of VR from simpler models that are relatively easy to understand and can be estimated using standard statistical software packages on a laptop computer. The primary specification we estimate is a *difference-in-differences* (DID) model.[1] This DID research design is a central feature of the VR-ROI model (see Chap. 3) and has been used to evaluate the impact of VR programs in Washington State (Hollenbeck & Huang, 2006), Minnesota (Maryns & Robertson, 2015), Massachusetts (Uvin et al., 2004) and Utah (Wilhelm and Robinson, 2013). See Chap. 2 for further details.

With data before and after service receipt, the DID design allows us to effectively account for unobserved but stable confounding variables, such as a different work ethic and/or motivation, undiagnosed disabilities, and other factors that are difficult or impractical to measure. With panel (longitudinal) data, we net out these individual factors that are fixed over time by differencing labor market outcomes before and

[1] Details of the DID model along with codes for estimating the model using standard statistical software packages such as Excel are included in Appendix 6. However, this detail is not necessary for understanding the results in this chapter.

C. M. Clapp et al., *Evaluation of Vocational Rehabilitation Services*, Diversity and Inclusion Research, https://doi.org/10.1007/978-3-031-86942-6_6

after service provision. Similarly, calculating a second difference in the time trends between treatment and comparison groups nets out the effects of time-varying confounders (e.g., economy-wide changes such as a recession).

Using standard DID models and data from the North Carolina agency, we differentiate between short- and long-run effects, different VR services, and the detailed set of control variables described in Chap. 3. However, we do not use the complex error structure and multiple equations for service receipt, the counselor instrumental variables, or the nonlinear models that are all central features of the structural VR-ROI model.

6.2 Difference-in-Differences Models: Two Illustrations

Our objective is to compare results from simpler DID models with those from the VR-ROI model. To do this, we use the data to estimate DID models using the short- and long-run effects for the seven VR services across the three groups of clients with different types of impairments. However, we begin by illustrating the DID design in two simpler settings in which there is only a single VR service variable. First, we estimate the effect of *Training* for clients diagnosed with cognitive impairments (CI) by comparing employment rates for VR clients 1 year before and 3 years after the application period in state fiscal year (SFY) 2012. Second, using the full sample, we present a series of estimates in which there is a single treatment variable that indicates whether a client received any purchased service besides *Diagnosis & Evaluation (D)*. This model distinguishes between short- (SR) and long-run (LR) effects and the underlying disability type. We also estimate the model with and without the rich covariates described in Chap. 3.

6.2.1 A 2 × 2 Analysis of the Effect of *Training* on Clients with CI

Table 6.1 reports quarterly employment rates 1 year before and 3 years after applying for VR services in SFY 2012 (i.e., between July 1, 2011, and June 30, 2012) for clients with CI who received purchased *Training* services and those who did not

Table 6.1 Quarterly employment rates by application quarter and treatment status, SFY 2012 North Carolina VR clients with cognitive impairments group[a,b]

Period[c]	Untreated	Treated
Pre-application	0.140	0.035
Post-application	0.404	0.339

Notes
[a]The treated group received purchased *Training* services. The untreated group did not
[b]Those with prior VR spells are dropped
[c]The periods are four quarters before (pre) and 12 quarters after (post) the date the VR clients applied for services in SFY 2012

receive purchased *Training* services. We refer to these two groups as the treated and untreated, respectively.

Three years after applying for VR services, a comparison of treated and untreated groups shows that the employment rate for those who received *Training* services is -0.065 ($=0.339 - 0.404$) percentage points less than those that who did not receive services, which suggests that *Training* decreases employment probability by 6.5 percentage points. This conclusion is correct if the treated and untreated would have had the same employment propensities and faced the same labor market environments, except for the fact that the treated received VR *Training* services. Yet, in the pre-application period, employment rates between the treated and untreated differ by -0.105 ($=0.035 - 0.140$). If the decision to receive VR *Training* services is influenced by a client's propensity to find employment, the observed contemporaneous association is spurious: treated clients would have higher or lower employment rates regardless, depending on whether the selection bias is positive or negative.

Another way to look at the data is to compare changes in employment rates before and after VR clients receive *Training* services. In particular, the employment rate increased from 0.035 to 0.339, which suggests a substantial positive effect of *Training* services on employment. However, this "before-after" analysis is correct only if no other determinants of employment, including health status or the local labor market, changed between pre- and post-application periods except for the receipt of *Training*. This condition does not hold for the untreated, whose employment rate increased from 0.140 1 year prior to the application quarter to 0.404 3 years after the application quarter. Since the untreated group did not receive substantive VR services, something else changed over this time.

Suppose that, in the absence of VR services, the treated and untreated would have experienced the same change in employment rates. That is, the employment rate in the absence of the treatment would have increased by 0.264 ($=0.404 - 0.140$). The employment rate for those who received *Training* services increased by 0.304 ($=0.339 - 0.035$). The DID model compares the time-series changes in employment rates for the treated and untreated and yield an estimate of 0.040 ($=0.304 - 0.264$). This suggests that VR increases employment probability by 4.0 percentage points. The key assumption is that observed trends in employment rates for untreated clients equal the counterfactual trends that would have been realized for treated clients had they not received *Training* services.

6.2.2 Short- and Long-Run Effects of a Single VR Treatment

We now distinguish between short- and long-run effects and the underlying disability type. We also estimate this model with and without the rich covariates described in Chap. 3. For this illustration, a key simplification is that, instead of allowing for seven treatment variables, we use a single treatment variable that indicates whether a client received any purchased service besides *Diagnosis & Evaluation (D)*. Clients who did not receive purchased services or received only *D* services are considered

Table 6.2 Short- and long-run effects of VR purchased service receipt on employment and log earnings by disability[a,b,c,d]

	Employment				Log earnings			
	No covariates		Covariates		No covariates		Covariates	
	SR	LR	SR	LR	SR	LR	SR	LR
CI	**0.047**	**0.081**	**0.047**	**0.081**	**−0.383**	**−0.322**	**−0.335**	**−0.265**
MI	**0.067**	**0.103**	**0.067**	**0.103**	−0.015	0.043	−0.014	0.053
PI	0.009	**0.014**	0.009	**0.014**	−0.043	**0.144**	−0.046	**0.138**
ALL	**0.072**	**0.123**	**0.073**	**0.123**	−0.037	**0.071**	0.0002	**0.128**

Notes:
[a]Clients with prior VR spells are not included
[b]VR service receipt includes all services except D
[c]Bolded estimates are statistically significant at the 5% significance level
[d]The number of dependents is not included as a covariate

untreated. In this framework, 71% of clients received VR treatment, and 29% did not.

The results are displayed in Table 6.2. For each disability group and all the groups together, we estimate the short- and long-run DID model with and without covariates. The first set of results are estimates for employment, and the second are estimates for log earnings. DID estimates in bold font are statistically significant at the 5% level.

The estimated employment effects are all positive, and most are statistically significant at the 5% level. Estimated short-run log earnings effects are all negative, although mostly insignificant, while long-run effects are mostly positive and significant. The one notable exception is the estimated log earnings effect for clients with CI, where VR services are estimated to have a large negative effect on earnings.

There are several other key takeaways from the DID estimates that are replicated in the more elaborate DID models considered in Sect. 6.2:

Short- vs. long-run effects: There are notable differences between estimated short- and long-run effects. This is especially true for earnings where the estimated short-run effects are often negative, although insignificant, and the estimated long-run effects are positive (except for those with CI).

Disability types: There are notable differences between estimated effects across the different disability types. For example, estimated long-run employment effects for clients with PI are 0.014, while estimated long-run effects for clients with CI and MI are around 0.10.

Covariates: Finally, the estimates do not notably change when covariates are included in the model.

6.3 DID Model Estimates of the Effects of VR on Employment and Earnings

In this section, we estimate several DID models of the effects of VR services on employment and log earnings and compare the results with those from the structural VR-ROI model described in Chap. 3. As in the structural model, we distinguish between short- (0–2 years) and long-run (greater than 2 years) effects; clients with cognitive impairments (CI), mental illness (MI), or physical impairments (PI); and seven service types: *Diagnosis & Evaluation* (D), *Training* (T), *Education* (E), *Restoration* (R), *Maintenance* (M), *Placement* (P), and *Supported Employment* (S). In our primary DID specification, the sample includes clients who applied for VR services for the first time in 2012; all clients with prior VR service spells (i.e., receipt of VR services) are dropped.

6.3.1 Model 1A: Baseline Model

For our baseline model, Model 1A, we estimate a separate DID model for each of the seven service types and do not include clients with prior VR spells. We first provide a detailed analysis of the effects of VR services for clients with cognitive impairment and then evaluate the effects for those with mental illness or physical impairment.

6.3.1.1 Cognitive Impairments

Table 6.3 displays short- and long-run estimates for clients with cognitive impairments. The first two columns show the short- and long-run estimates from the structural VR-ROI model, the next two from DID models without covariates, and the final two from DID models with covariates. The first panel of results are the estimates for employment, and the second panel are the estimates for log earnings. DID estimates in bold font are statistically significant at the 5% level, and DID estimates in italicized font have a different sign than the structural VR-ROI model estimates.

The estimated employment effects are mostly but not always positive. For example, *Education* services are estimated to increase the employment rate by 0.055 in the short run and 0.084 in the long run. One notable exception is the estimated effects for *D*, which are negative, statistically significant (at the 5% level), and substantial. The effects of VR services on log earnings are varied, with some positive and some negative. *Education*, for example, is estimated to have a positive effect in both the short and long run, *Restoration* to have negative effects, and *Training* to have a negative effect in the short run and a positive effect in the long run.

The estimates also reflect important differences across periods and services. Short- and long-run estimates substantially differ in many cases (e.g., see the estimated effect of *Training*). There are also notable differences in the estimated effects of the different services. For example, *Restoration* services have a large

Table 6.3 Estimated effect of VR services for clients with CI[a,b,c,d]

	Employment					
			DID			
	Structural model[e]		Without covariates		With covariates	
	SR	LR	SR	LR	SR	LR
D	−0.063	−0.076	**−0.054**	**−0.030**	**−0.054**	**−0.031**
T	0.056	0.126	***−0.054***	**0.083**	***−0.054***	**0.083**
E	−0.016	0.040	***0.055***	**0.084**	***0.053***	**0.080**
R	−0.000	0.001	***0.026***	*−0.001*	***0.026***	*−0.001*
M	0.040	0.019	**0.040**	0.009	**0.040**	0.009
P	−0.065	0.267	**−0.063**	**0.222**	**−0.062**	**0.223**
S	0.057	0.004	**0.078**	**0.096**	**0.078**	**0.097**
	Log earnings for the employed					
			DID			
	Structural model		Without covariates		With covariates	
	SR	LR	SR	LR	SR	LR
D	−0.051	−0.102	**−0.228**	**−0.31**	**−0.193**	**−0.224**
T	0.170	0.237	***−0.287***	0.031	***−0.220***	**0.111**
E	−0.032	0.038	***0.164***	**0.278**	*0.126*	**0.258**
R	−0.032	−0.054	−0.096	**−0.189**	−0.097	**−0.172**
M	0.005	−0.004	0.030	*0.040*	0.027	*0.053*
P	−0.129	0.123	**−0.398**	*−0.178*	**−0.499**	***−0.254***
S	0.070	−0.062	***−0.103***	**−0.155**	***−0.117***	**−0.152**

Notes:
[a]Clients with prior VR spells are not included
[b]Bolded estimates are statistically significant at the 5% significance level
[c]The number of dependents is not included as a covariate
[d]Italicized estimates have a different sign than structural model estimates
[e]Structural model employment estimates are multiplied by 0.4 to account for the nonlinearity of the model

negative effect on long-run log earnings, while *Education* services have a large positive effect.

While there is variation in estimates across services and periods, estimates with and without covariates are generally similar. **Thus, given our objective to consider simpler models, we focus on DID models without covariates.** Although not necessary, dropping the covariates serves to further simplify the model. However, in the nonlinear VR-ROI model, covariates have an important impact on the estimated ROI even if they only modestly affect the estimated short- and long-run effects of VR.

A key objective is to compare the estimated effects from DID models with those using the structural VR-ROI model discussed in Chap. 3. As mentioned previously, DID estimates in italicized font have different signs than the structural estimates. More often than not, structural and DID model estimates have the same sign, but not always: 8 of the 28 estimates differ for employment and 10 of the 28 for log earnings. For employment, except for the long-run effect of *Supported Employment (S)*, estimates with the same sign are within five percentage points of each other. For example, estimates of the short-run effects of *Maintenance (M)* on employment propensity are identical, at 0.04, in the structural and DID models, and estimates of the long-run effects differ by 0.010 (0.019 versus 0.009). The one exception is for the long-run effect of *Supported Employment*, where the structural model estimate of 0.004 is notably smaller than the DID estimate of 0.096.

In contrast, magnitudes for log earnings are often substantially different even when the signs are the same. For example, the long-run effect of *Education* on log earnings is estimated to equal 0.038 in the structural model and 0.278 in the DID model.

Overall, for VR clients with CI, DID model estimates are often close approximations to structural model estimates for the employment outcome but not for the log earnings outcome. In particular, in the model without covariates, 79% of the employment estimates are within five points of each other, while for log earnings only 1 of the 14 DID estimates is within five points of the analogous structural model estimate. Still, the qualitative conclusions are mostly similar across models and outcomes.

6.3.1.2 Mental Illness and Physical Impairments

Table 6.4 presents analogous estimates from the structural and DID models for clients with mental illness and physical impairments in which the DID models are estimated without covariates. With a few exceptions, employment estimates (25 out of 28) have the same signs but, in some cases, differ by more than ten points. For example, for clients with physical impairments, the long-run effect of *Placement* is 0.059 when using the structural model and 0.267 when using the DID model. Overall, the DID model for employment provides a somewhat closer match to the structural model for the PI and CI subsamples than the MI subsample; for the PI and CI subsamples, three estimates differ by more than five points, while 6 estimates differ by more than five points in the MI subsample.

For earnings, most estimates from the two models have the same signs (20 of 28 estimates). For those with the same signs, about half are within five percentage points of each other. However, eight of the estimates with the same signs differ by more than ten points. For example, for clients with physical impairments, the long-run effect of *Placement* is −0.084 when using the structural model and −0.637 when using the DID model. In contrast to estimates for employment, the log earnings DID model provides a somewhat closer match to the structural model for the MI subsample than for those with PI and CI; for MI subsample, 7 of the 14 estimates differ by more than five points; for the PI and CI subsamples, 11 and 12 estimates differ more than five points, respectively.

Finally, as we found with the structural models, these results confirm substantial differences in the effects of VR services for clients with different impairments. For example, the long-run effect of *Restoration* on earnings is estimated to be positive

Table 6.4 Estimated effects of VR services for clients with MI & PI, without covariates[a,b,c]

	MI							
	Employed				Log earnings			
	Structural model[d]		DID		Structural model		DID	
	SR	LR	SR	LR	SR	LR	SR	LR
D	−0.058	−0.037	**−0.038**	**−0.020**	−0.121	−0.127	**−0.145**	**−0.123**
T	0.057	0.086	**0.035**	**0.106**	0.122	0.099	0.001	*−0.082*
E	−0.040	0.012	*0.017*	**0.072**	−0.232	−0.018	**−0.363**	−0.068
R	−0.006	0.026	***0.024***	**0.051**	−0.019	0.011	−0.006	**0.052**
M	0.101	0.076	**0.075**	**0.068**	0.108	0.039	*−0.035*	0.023
P	−0.105	0.189	−0.001	**0.276**	−0.302	0.013	**−0.394**	*−0.153*
S	0.082	0.089	**0.136**	**0.147**	0.167	0.146	**0.125**	**0.189**
	PI							
	Employed				Log earnings			
	Structural model		DID		Structural model		DID	
	SR	LR	SR	LR	SR	LR	SR	LR
D	−0.025	−0.018	**−0.029**	**−0.028**	−0.071	−0.050	−0.026	*0.031*
T	0.060	0.095	**0.069**	**0.104**	0.117	0.162	*−0.089*	*−0.031*
E	0.018	0.058	**0.051**	**0.134**	−0.085	0.031	−0.053	**0.207**
R	−0.068	−0.112	**−0.05**	**−0.066**	0.067	0.038	**0.123**	**0.158**
M	0.060	0.098	**0.064**	**0.091**	−0.030	0.092	**−0.122**	**0.110**
P	−0.119	0.059	*0.017*	**0.267**	−0.332	−0.084	**−0.876**	**−0.637**
S	0.134	0.180	**0.155**	**0.209**	0.118	0.109	***−0.117***	*−0.026*

Notes:
[a]Clients with prior VR spells are not included
[b]Bolded estimates are statistically significant at the 5% significance level
[c]Italicized estimates have a different sign than the structural model estimates
[d]Structural model employment estimates are multiplied by 0.4 to account for the nonlinearity of the model

and substantial for clients with PI, positive but much more modest for clients with MI, and negative and substantial for clients with CI.

6.3.2 Models 1B and 1C: Modified Baseline Model

In this section, we estimate related DID models that more closely aligned with features of the VR-ROI model specification. First, in Model 1B, we include data on clients with prior spells. Second, in Model 1C, we jointly estimate the effects of the different VR services rather than estimating each separately. While these two models are substantially easier to estimate and evaluate than the VR-ROI model, they are somewhat more complex than Model 1A. In addition, there are valid statistical reasons to exclude clients with prior service periods (Dean et al., 2015).

6.3.2.1 Model 1B: Including Clients with Prior Spells

There are important tradeoffs when deciding on whether to include clients with prior spells in the sample used to estimate the model. On the one hand, including these clients increases the sample size and, as a result, reduces the variance of the estimators. On the other hand, clients with prior spells are arguably different, and including them may bias the estimates (Dean et al., 2015). The structural models are estimated using data on clients with and without prior spells and include a statistical control to account for those with prior spells.

Table 6.5 reports DID estimates when the sample includes clients with prior VR spells. We refer to this as the unrestricted sample. For clients with CI, the restricted sample includes information on 4301 VR clients, and the unrestricted sample

Table 6.5 Estimated effects of VR services, including clients with prior spells, without covariates[a,b]

	CI			
	Employment		Log earnings	
	SR	LR	SR	LR
D	**−0.081**	**−0.087**	**−0.136**	**−0.203**
T	***−0.031***	**0.079**	***−0.074***	0.030
E	**−0.021**	***−0.022***	−0.059	0.003
R	*0.007*	***−0.031***	−0.007	**−0.098**
M	***−0.029***	***−0.084***	*−0.055*	**−0.075**
P	**−0.087**	**0.201**	**−0.342**	*−0.095*
S	***−0.014***	***−0.043***	*−0.022*	**−0.092**
	MI			
	Employment		Log earnings	
	SR	LR	SR	LR
D	**−0.053**	**−0.041**	**−0.122**	**−0.120**
T	**0.023**	**0.069**	0.028	*−0.014*
E	**−0.020**	*0.000*	**−0.231**	−0.030
R	−0.006	**0.016**	−0.044	*−0.012*
M	**0.023**	**0.016**	***−0.056***	*−0.012*
P	**−0.038**	**0.206**	**−0.338**	***−0.163***
S	**0.047**	**0.055**	**0.068**	**0.106**
	PI			
	Employment		Log earnings	
	SR	LR	SR	LR
D	**−0.029**	**−0.030**	−0.002	*0.045*
T	**0.045**	**0.076**	0.067	**0.108**
E	**0.022**	**0.063**	−0.057	**0.123**
R	**−0.049**	**−0.069**	**0.089**	**0.072**
M	**0.026**	**0.050**	**−0.075**	**0.102**
P	−0.033	**0.189**	**−0.588**	**−0.280**
S	**0.069**	**0.094**	*−0.007*	0.022

Notes:
[a]Bolded estimates are statistically significant at the 5% significance level
[b]Italicized estimates have a different sign than the structural model estimates

includes another 1930 clients. For the MI and PI subsamples, sample sizes are 5581 and 4527, respectively in the restricted sample and 8085 and 6239, respectively, in the unrestricted sample.

Although the general patterns are similar to those using the restricted sample, there are some notable differences between particular estimates, across outcomes, and across impairment subsamples. DID estimates using the unrestricted sample are generally more consistent with structural model results than those using the restricted sample (see Tables 6.3 and 6.4). For the CI subsample, estimates from the unrestricted sample are further from structural employment results but are notably closer to earnings estimates. For example, using the unrestricted sample to estimate the effect of VR on employment, 8 of the 14 estimates have signs that differ from structural model estimates, while only four have different signs using the restricted sample. In contrast, for the earnings model, eight of the estimates either have different signs or are more than five points apart using the unrestricted sample. However, in the restricted sample, 13 of the 14 estimates fall into this grouping. For clients with MI, earnings estimates from the unrestricted sample are generally closer to VR-ROI estimates, and for those with PI, employment estimates from the unrestricted sample are generally closer.

Overall, while there is variation across different services and disability types, DID estimates from the unrestricted sample are somewhat closer to structural model estimates than those from the restricted sample. This is not surprising given that the structural VR-ROI model is estimated using the unrestricted sample.

For simpler DID models, the restricted samples have sufficient numbers of observations to precisely estimate the effects of VR on labor market outcomes. For the structural model, the demands of the data are much more extensive than those for the DID models. To obtain precise structural estimates, we elected to use the larger unrestricted sample, which contains about 30% more clients than the restricted sample. The primary disadvantage is that clients with prior service periods may differ from new clients in ways that can lead to biases (Dean et al., 2015). While our structural model includes a control variable for these biases, this correction may not fully address the issue. In general, when it is possible to get precise estimates, using the restricted sample is the best choice.

6.3.2.2 Model 1C: Estimating Service Effects Jointly

In the structural VR-ROI model, service effects are estimated jointly. Table 6.6 reports estimated coefficients from the DID model, in which the service effects are estimated jointly, rather than separately, as was done to generate results in baseline Model 1A. In general, the estimates are similar to those obtained when estimating the effects of services separately (see Tables 6.3 and 6.4). Most, but not all, of the estimates have the same sign as the structural model estimates. In fact, for the MI and PI models, all employment estimates except for the short-run effect of *Restoration* have the same signs. Moreover, for the employment models, estimates are generally within five percentage points of each other. For the log earnings models, several of the estimates have different signs, and those with the same signs often differ by more than five points.

Table 6.6 Joint estimation of service effects by disability type[a,b,c]

	CI			
	Employment		Log earnings	
	SR	LR	SR	LR
D	**−0.060**	**−0.077**	**−0.180**	**−0.258**
T	***−0.039***	**0.077**	***−0.206***	**0.127**
E	***0.056***	**0.129**	*0.062*	**0.190**
R	*0.010*	**0.016**	**−0.156**	**−0.198**
M	0.013	***−0.024***	0.075	***0.087***
P	**−0.080**	**0.187**	−0.258	*−0.054*
S	**0.098**	**0.088**	*−0.072*	**−0.101**
	MI			
	Employment		Log earnings	
	SR	LR	SR	LR
D	**−0.049**	**−0.044**	**−0.126**	**−0.103**
T	**0.029**	**0.087**	0.066	*−0.048*
E	−0.009	**0.055**	**−0.382**	−0.054
R	*0.010*	**0.037**	*0.015*	**0.053**
M	**0.053**	**0.020**	0.037	0.009
P	**−0.087**	**0.183**	**−0.377**	*−0.185*
S	**0.125**	**0.124**	**0.112**	**0.214**
	PI			
	Employment		Log earnings	
	SR	LR	SR	LR
D	**−0.016**	**−0.012**	−0.065	−0.008
T	**0.055**	**0.070**	*−0.054*	*−0.045*
E	0.019	**0.100**	−0.009	**0.132**
R	**−0.053**	**−0.072**	**0.147**	**0.151**
M	**0.046**	**0.047**	**−0.085**	**0.094**
P	**−0.122**	**0.098**	***0.837***	**−0.608**
S	**0.150**	**0.181**	−0.022	0.026

Notes:
[a]Clients with prior VR spells are not included
[b]Bolded estimates are statistically significant at the 5% significance level
[c]Italicized estimates have a different sign than the structural model estimates

Overall, we do not see any systematic differences between the results when VR services are estimated jointly or separately. In some cases, joint estimates are closer to the structural model estimates (see, for example, the long-run employment effects of *Placement* for those with PI and MI). In other cases, the joint estimates are further apart (see, for example, the long-run employment effects of *Placement* for those with CI); in many cases, the estimates are similar.

Table 6.7 Estimated effects of VR services, all disabilities without covariates[a,b]

	Employment		Log earnings	
	SR	LR	SR	LR
D	**−0.083**	**−0.099**	**−0.175**	**−0.226**
T	**0.046**	**0.182**	**−0.162**	−0.018
E	**0.021**	**0.065**	−0.071	**0.216**
R	**0.021**	**0.340**	**0.072**	**0.110**
M	**0.061**	**0.055**	**0.044**	**0.143**
P	0.007	**0.331**	**−0.454**	**−0.207**
S	**0.154**	**0.214**	**0.149**	**0.204**

Notes:
[a]Clients with prior VR spells are not included
[b]Bolded estimates are statistically significant at the 5% significance level

Table 6.8 Short-run (<9 quarters) estimated effects of VR services, all disabilities without covariates[a,b,c]

	Employed				Log earnings			
	CI	MI	PI	All	CI	MI	PI	All
D	**−0.054**	**−0.038**	**−0.029**	**−0.083**	**−0.228**	**−0.145**	−0.026	**−0.175**
T	***−0.054***	**0.035**	**0.069**	**0.046**	***−0.287***	0.001	*−0.089*	**−0.162**
E	***0.055***	*0.017*	**0.051**	**0.021**	***0.164***	**−0.363**	−0.053	−0.071
R	***0.026***	***0.023***	**−0.050**	**0.020**	−0.096	−0.006	**0.123**	**0.072**
M	**0.040**	**0.075**	**0.064**	**0.061**	0.030	*−0.035*	**−0.122**	**0.044**
P	**−0.063**	−0.001	*0.017*	0.007	**−0.398**	**−0.394**	**−0.876**	**−0.454**
S	**0.078**	**0.136**	**0.155**	**0.154**	***−0.103***	**0.125**	***−0.117***	**0.149**

Notes:
[a]Clients with prior VR spells are not included
[b]Bolded estimates are statistically significant at the 5% significance level
[c]Italicized estimates have a different sign than the structural model estimates

6.3.3 Other Even Simpler Models

In this section, we report estimates from three DID models that further simplify various aspects of the VR-ROI Model. In Table 6.7, we report estimates from models for all VR clients together, rather than separating results by disability type. In Table 6.8, we report estimates for short-run effects only, both by disability type (CI, MI, and PI) and for all VR clients.

6.3.3.1 Model 2: All VR Clients

Table 6.7 displays estimates from the DID model without accounting for impairment; that is, all VR clients regardless of disability type are included in this sample. In this model, with one exception, employment estimates are positive, while earnings estimates vary with some being positive and some negative. For example, *Training* is estimated to have negative short- and long-run effects on earnings, while *Restoration* is estimated to have a positive effect on short- and long-run earnings. Given that this model does not allow for the effects of VR to vary by impairment, it is not surprising that the estimates often differ from structural model estimates that vary by CI, MI, and PI. The signs of the estimates, however, are generally consistent. For example, over 70% of structural model employment estimates have the same sign as those from this DID model. Likewise, over 70% of PI and MI structural model estimates for earnings have the same sign as this DID model, and for CI, half of the estimates have the same sign. Thus, the qualitative findings from this simple DID model are similar to those from the structural model.

6.3.3.2 Models 3 and 4: Short Run by Disability Type (Model 3) and All VR Clients (Model 4)

Table 6.8 reports estimates from a DID model that uses short-run (<9 quarters) labor market outcomes. Focusing on short-run effects has three advantages: (1) the model is simpler to estimate, since there is only one effect per service rather than 2; (2) the evaluator does not need to wait more than 2 years to collect data on the long run; and (3) similar labor market data are available in the administrative RSA-911 data accessible to VR agencies. See Chap. 3 for a more in-depth discussion of the trade-offs of estimating short- versus long-run models.

Overall, the short-run estimates are similar to those found using baseline DID Model 1A, as shown in Tables 6.3 and 6.4. The estimated short-run effects on employment are mostly positive and generally consistent with those found using the VR-ROI model. When disability types are not considered, the only negative estimate on employment is for *Diagnosis and Evaluation (D).* Estimated short-run effects on log earnings vary in sign and magnitude and are often substantially different (more than five points) from those from the VR-ROI model. Fifteen of the 21 estimates differ in sign and/or by at least five points.

6.4 Present Value of Benefits and NPV from DID Models

In this section, we report the estimated mean present value of benefits without including cost and NPV estimates that include costs using DID Models 1–4. Cost estimates are identical to those used to derive ROI using the structural VR-ROI models. Table 6.9 displays estimates for the CI, MI, and PI subgroups. The first row displays results from the VR-ROI model. As discussed in Chap. 5, the mean present value of the benefits of VR in North Carolina are estimated to be positive for clients in all three disability groups. However, after costs are subtracted, the NPV estimates are all negative. That is, on average, cost exceed benefits.

The other rows display results from five DID models in which estimates in italicized font have different signs from those estimated using the VR-ROI model. In general, NPV estimates from the DID models are similar to those found using the VR-ROI model. Recall that the baseline Model 1A estimates short- and long-run effects for each of the seven services and for each disability type. When using results from this DID model without covariates, estimated mean benefits are positive for

Table 6.9 Estimated present value of benefits and NPV using different models[a,b]

	Mean PV of benefits			Mean NPV		
Model	CI	MI	PI	CI	MI	PI
VR ROI	188	1764	1137	−4909	−1662	−3858
Baseline model						
1.a: Without covariates	*−1057*	3085	2376	−5928	−61	−2393
1.c: With covariates	*−893*	*−2145*	*−115*	−5765	−5291	−4883
Other simpler models 2–4						
Model 2: All VR clients	7846	4077	842	*3146*	−59	−2986
Model 3: Short run only	*−273*	356	*−157*	−5144	−2791	−4925
Model 4: Short run & all VR clients	944	463	*−171*	−3755	−3673	−3999

Notes:
[a]Clients with prior VR spells are not included
[b]Italicized estimates have a different sign than the structural model estimates

clients with MI and PI but negative for CI, while the mean NPV is estimated to be negative for all subgroups. When the effects across different services are estimated jointly and covariates are included in this baseline DID model (Model 1c), the estimated present value of benefits and NPV are all negative. On average, the costs of the VR program are estimated to exceed the benefits by around $5000. Except for the MI subgroup, these DID estimates of NPV have a magnitude similar to those found using the VR-ROI model. For example, the mean NPV for clients with CI is estimated to be -$4909 using the VR-ROI model, −$5928 for Model 1A without covariates, and -$5765 using Model 1C with covariates.

The last three rows of Table 6.9 report mean NPV estimates for three models that further simplify baseline Model 1. First, in Model 2, we estimate the short- and long-run effects of VR for all clients without allowing effects to vary by impairment type (see Table 6.7). Using this simplified model, estimates from the PI subsample are similar to those using the VR-ROI model, while estimates for the CI and MI subsamples are substantially larger. For example, for the CI subsample, Model 2 estimates of the PV of benefits and NPV are nearly $8000 more than those found using the VR-ROI model. Moreover, the mean NPV is positive using Model 2 but negative using the VR-ROI model.

The last two rows display NPV estimates based on short-run effects, where Model 3 allows effects to vary by disability type and Model 4 restricts estimated short-run effects to be the same across all clients (see Table 6.8). Surprisingly, Model 3 NPV estimates all have the same sign and magnitudes that are somewhat similar to results using the VR-ROI model across all three disability groups. They all are negative, which suggests that the average cost exceeds the average benefits, with the estimates differing by \$235 = (\$5144—\$4909) for clients with CI, \$1129 for clients with MI, and \$1067 for clients with PI. Likewise, using Model 4, NPV estimates are all negative and of magnitude similar to those using the VR-ROI model.

6.5 Conclusion

DID models provide a straightforward approach to assessing the effects of VR services that can be estimated using standard statistical software packages. However, whether the simpler DID model can provide estimates similar to those using the more elaborate VR-ROI model described in Chap. 3 is an open question. The VR-ROI model accounts for important economic, statistical, and methodological issues that are not addressed in the DID model.

In this chapter, we take a first step in identifying whether a simple DID model may be "good enough" in many practical settings program administrators and policy evaluators face. In general, we find that the estimates from a series of DID models seem sensible and are often consistent in sign, if not magnitude, with analogous estimates from the structural model. Perhaps most striking are estimates of the mean NPV. Even in restrictive Models 3 and 4, which only allow for short-run effects, NPV estimates are generally, but not always, similar in sign and magnitude to NPV estimates using the VR-ROI model. Still, these results are not uniformly positive; in some cases, estimates from the DID model differ substantially from the VR-ROI model.

We stress that these results should be viewed as preliminary and uncertain, rather final and conclusive evidence for whether these types of simple DID models should be widely used in practice across different agencies and time periods. Certainly, without investing the time and resources required to evaluate the structural VR-ROI model, researchers can estimate DID models to get a first take on the effects of VR and resulting NPV estimates. However, results from such an exercise should be interpreted with caution. When feasible, a structural model that accounts for simultaneous endogenous relationships of different VR services and labor market outcomes is preferable and will result in more credible inferences. In contrast, DID models cannot account for numerous factors in the structural model and, as such, may lead to biased inferences. Moreover, the simplest DID models (e.g., Models 2, 3, and 4) cannot provide valuable information on how the effects of VR vary in the short- and long-run, by disability groups, and by service types. The structural model provides more information than the DID models, and this extra information can be valuable. Finally, we find variation across states that suggests a one-size-fits all approach is not effective.

Are there settings in which DID models more closely approximate results using the VR-ROI model? Under what circumstances might estimates from the simplest DID models provide valid (qualitative) NPV estimates, even if they lack some important and useful details found in the VR-ROI model? In future work, we aim to further assess DID models and provide more guidance on the benefits and limitations of these models in different settings.

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Conclusions and Next Steps

7

7.1 Introduction

State Vocational Rehabilitation (VR) agencies provide services to individuals with disabilities to help them prepare for, secure, retain, or regain employment. The programs are designed to provide tailored services that address the unique needs of each client, enabling people with disabilities to achieve their employment goals and increase their independence.

In the context of VR, the evaluation of economic returns is essential for understanding the value and effectiveness of these programs. By assessing the return on investment (ROI), we can determine the financial benefits of VR services relative to their costs, providing valuable insights for policymakers, administrators, and other stakeholders.

Over the past two decades, our team has been developing and refining the VR-ROI model to provide a rigorous framework for estimating the employment and earnings impacts of VR service provision across types of service, types of disability, time periods, and agencies. The models have been developed and refined with input from VR agency senior leadership, program management, direct service staff, and consumers to learn details of the VR program and service provision, including VR decision-making processes. Relevant stakeholders have been consulted throughout with the aim of ensuring the models and results are accurate, appropriate, valued, and useful.

The resulting ROI evaluations have been positively reviewed by academic economists and vocational researchers and have been published in major academic economics journals (Dean et al., 2015, 2017, 2018, 2019), the Journal of Rehabilitation Administration (e.g., Clapp et al., 2019; Schmidt et al., 2019; Stern et al., 2019), and the Journal of Visual Impairment and Blindness (Clapp et al., 2020). This rigorous model is summarized in Chap. 3, providing a detailed explanation of its development and application, while Chap. 2 reviews the publications and the broader literature on ROI evaluations.

C. M. Clapp et al., *Evaluation of Vocational Rehabilitation Services*, Diversity and Inclusion Research, https://doi.org/10.1007/978-3-031-86942-6_7

This book synthesizes the existing ROI literature, the methodological approaches used in our VR-ROI analyses, and new empirical findings based on data from five states: Virginia, Maryland, Kentucky, North Carolina, and Texas. This is the most current, comprehensive and expansive application of the structural VR-Return on Investment (VR-ROI) model to date.

While this model and the new results provide valuable data on the economic returns of VR services, it is important to recognize that these results should be interpreted with caution. The model is an analytical tool that can help guide decision-making, but it does not capture all the variables that might influence client outcomes, such as personal preferences, specific goals, and non-financial benefits like quality of life and social integration. Moreover, the results reported in this book are based on the VR program in 2012, not the present. The counselors, clients, and administrators should consider the broader context of the current environment and each individual's unique needs and circumstances when considering the efficacy of VR and when determining the most appropriate services.

Importantly, the VR-ROI model is a work in progress, continually evolving to better capture the complexities and impacts of VR programs. The model will continue to evolve in order to reflect changes in the VR-program, differences across programs, disabilities, and state programs, and limitations of the data and model. In this concluding chapter, we briefly summarize some of the key themes from this book and then discuss directions for future research and refinements of the model.

7.2 Overview of the VR-ROI Model and Key Findings (Chaps. 3 and 5)

The VR-ROI model is a sophisticated tool designed to evaluate the labor market and financial outcomes of VR programs. This model integrates detailed longitudinal data on VR services and employment and earnings, allowing us to estimate the labor market returns of VR services. Applying the VR-ROI model to this rich dataset has allowed us to provide a nuanced understanding of the economic impact of VR programs across time periods, different states agencies, and different disability groups.

Throughout this book, we emphasize the importance of state-specific and disability-specific analyses in understanding the broader impacts of VR programs. By tailoring our evaluation to the unique contexts of different states and the varied needs of clients, we provide insights that are both comprehensive and relevant for improving VR services.

In Chap. 5, we present results on the impact of VR across eight distinct state agencies, four distinct disability groups, and seven to nine distinct VR services. The findings show the diverse impact of VR services, highlighting both the successes and challenges of the programs. While many clients experience positive labor market returns, indicating the potential of VR programs to enhance employment and earnings, the overall economic return remains negative. This suggests that the costs of VR services often exceed the financial benefits for most clients, prompting the need for a critical evaluation of VR program efficacy. These findings underscore the importance of continuous improvement and adaptation of VR programs to ensure they effectively support individuals with disabilities in achieving their employment and independence goals. These findings also suggest the need to reexamine the validity of the VR-ROI model and reconcile the contrasting results from the 2000 cohorts in Virginia where we generally find VR has a positive ROI versus the negative ROI estimates for 2012 cohorts.

7.3 Drawbacks of the VR-ROI Model

While the VR-ROI model offers valuable insights into the economic returns of VR programs, it is not without limitations. Several key drawbacks should be considered when interpreting the results and applying the model in decision-making:

1. **Complicated to develop, estimate, and understand**: The VR-ROI model is complex and requires advanced statistical knowledge, programming skills, and substantial computing resources. This complexity makes it challenging for VR agency staff to estimate and use the model effectively. The need for specialized expertise limits the model's accessibility and practicality for everyday use by VR professionals.
2. **Missing important outcomes**: The VR-ROI model focuses on labor market outcomes, namely employment and earnings. While these are important indicators of VR program effectiveness, the model does not account for important non-pecuniary benefits. These include improvements in independent living skills, overall well-being, and quality of life, which are crucial for a holistic understanding of VR program impacts.
3. **Reliance on binary measures**: The VR-ROI model's measures of service receipt use a yes/no variable that indicates whether a client received a particular service or not. This approach does not capture the variability in service intensity across different clients. The intensity of services, such as the amount of time or dollars spent on a particular service for a particular client may significantly influence outcomes. Ignoring this variability can affect the accuracy and usefulness of the model's estimates.
4. **Non-purchased services**: The model does not account for non-purchased VR services, which are services provided by VR agencies that do not involve direct financial transactions. These services, such as counseling, job coaching, and other support activities, are critical components of the VR process. The absence of these non-purchased services from the model means that the full scope of VR service provision and its impacts on clients are not fully captured. This is probably a critical problem for BVI services.

The VR-ROI model uses appropriate data and state-of-the-art methods that have been vetted by a rigorous peer-reviewed process aimed at improving the credibility and reliability of published research in economics. Still, users of the model should understand that these limitations may bias the resulting estimates. By acknowledging and eventually addressing these limitations, we can ensure that the VR-ROI model remains a valuable resource for understanding and improving VR services, ultimately supporting better outcomes for individuals with disabilities.

7.4 Challenges for the Future of the VR-ROI Model

These drawbacks highlight the need for continuous refinement and improvement of the VR-ROI model. Although the model provides a robust framework for evaluating the economic returns of VR programs, addressing these and other limitations can enhance its credibility, accuracy, accessibility, and comprehensiveness. In this section, we describe several ongoing projects as well as ideas for future analyses designed to address some of the key limitations of the existing VR-ROI model.

First, we have a number of projects supported by generous funding from the National Institute on Disability, Independent Living, and Rehabilitation Research's (NIDILRR) Fiscal Year 2022 Field Initiated Projects (FIP)–Development grant (#90IFDV0028) to update and improve the VR-ROI model. Using data from the North Carolina Division of Vocational Rehabilitation Services, this initiative has three primary objectives:

- **Updated data:** Our previous VR-ROI evaluations, including those discussed in Chaps. 5 and 6, were estimated prior to the 2014 Workforce Innovative and Opportunity Act (WIOA). Our updated model will be developed using agency data for individuals who applied for VR services between 2017 and 2021, i.e., after revised WIOA common performance measures (CPM) were integrated into program data collections.[1] When combined with unemployment insurance earnings data collected at least through 2023, these updated data will provide an opportunity to investigate the relationship of CPM data elements to long-term employment outcomes, as well as COVID-impacted service provision and labor market outcomes.

  Using these data, we will offer a timely summary of the impacts of VR post-WIOA and during the COVID-19 pandemic and resulting recession. Nearly every stakeholder we consulted stressed the importance of providing timely ROI information, especially given changes due to WIOA and the COVID-19 pandemic. We will also provide descriptive measures of the relationship between WIOA-

[1] WIOA outlined six primary performance indicators that require longitudinal data to measure post-VR employment and earnings (in addition to educational and employer-relevant outcomes). To ensure accountability via data-driven, evidence-based evaluation techniques, the indicators are to be adjusted based on economic conditions and participant characteristics via statistical modeling. In response to WIOA, the NIDILRR 2018–2023 Long-Range Plan (NIDILRR, 2018) highlighted a "need for valid models of return on investment that are usable by state VR agencies" (p. 15) and "[lay] the groundwork for the development and use of evidence-based VR practices" (p. 16).

mandated common performance data elements and long-term employment outcomes.

- **Measures of intensity:** Further research is needed to measure the intensity of VR services in terms of expenditure and time spent with clients. This will help understanding the relationship between service intensity and client outcomes. By capturing detailed information on how much time and resources are devoted to each client, we can better assess the effectiveness of different service delivery models and identify areas for improvement. In particular, this will enable administrators and policymakers to address critical allocation questions. For example, we expect that a person who receives \$5000 of service will benefit much more than one who receives \$100 of service. But the question remains, "how much more?" Was the extra \$4900 of service worth the benefit? It might be that a client needs some minimum level of service to gain any benefit, or it might be that, after receiving some critical amount, extra service yields diminishing returns. Also, \$1000 of one service may provide different benefits than \$1000 of a different service. Ultimately, efficient allocation of resources will allow VR agencies to serve more consumers and achieve better outcomes within constrained budgets.
- **Accessibility of the VR-ROI model**: There is a critical need for a simplified estimator that can be used to provide agency-specific ROI estimates on a timely basis by in-house staff. Further research will aim to render the VR-ROI methodology more accessible and understandable for various stakeholders, including policymakers, VR administrators, and counselors. Simplifying the model and providing training and resources can help VR professionals use the model more effectively. Making the methodology more user-friendly will enhance its practical application and ensure that it can be widely adopted to inform decision-making. Our aim is to provide critical information about the minimum-sufficient data analysis techniques necessary to draw credible inferences. This will better position VR agencies to perform high-quality reliable evaluations of their own programs to enhance services for their clients.

  In Chap. 6, we present some preliminary work on simplifying the model by assessing whether a simple difference-in-differences (DID) model may be "good enough" in many practical settings that program administrators and policy evaluators encounter. In general, we find that the estimates from a series of DID regressions seem sensible and are often consistent in sign, if not magnitude, with analogous estimates from the structural model. Still, in some cases, estimates from the DID model substantially differ from the VR-ROI model.

  We stress that the results in Chap. 6 should be viewed as preliminary and uncertain, and thus interpreted with caution. When feasible, a structural model that accounts for the simultaneous endogenous relationships of different VR services and labor market outcomes is preferable and will yield more credible inferences; DID models cannot account for the numerous factors in the structural model and, as such, may lead to biased inferences. Moreover, the simplest DID models (e.g., Models 2, 3, and 4) cannot provide valuable information on how the effects of VR vary by the short and long run, by disability groups, and by service types. The structural model provides more information than the DID models, and this extra information is valuable. Moreover, we have found variation across states that suggests a one-size-fits-all approach may not be credible.

Are there particular settings in which DID models more closely approximate results obtained with the VR-ROI model? Under what circumstances might estimates from the simplest DID models provide valid (qualitative) net present value (NPV) estimates even if they lack some important and useful details found in the VR-ROI model? In future work, we aim to further assess the DID models and provide more guidance on the benefits and limitations of these models in different settings.

To develop an accessible model, our future work will analyze the importance of each aspect of our complex model so that we can assess the feasibility of simplifying it to the point that it can be estimated by agency staff. In doing so, we will implement a testing procedure to measure the precision of the simplified models relative to the more complex model and look for ways to correct for biases found in the simplified model. For example, we will investigate whether our nonlinear model specification is necessary, whether the use of instrumental variables is necessary, and how useful it is to use data on labor market outcomes further into the future than typically required by RSA.

We recognize that even a simplified version of our model may be difficult for some state VR agencies to estimate without guidance. As proposed, we will provide training to interested staff and other stakeholders in the use of the simplified VR-ROI model.

To guide our model development and knowledge translation efforts, we have designed and conducted a survey of current Policy and Program Evaluation staff at all state VR agencies. The information collected from this survey will identify the available resources and in-house data analysis capabilities at each agency and facilitate model development and knowledge translation. This will help us determine what data management and statistical analysis techniques can feasibly be conducted by most state VR agencies and enable us to tailor the associated knowledge translation components related to this goal to the benefit of state agencies. Our aim is to publish the results of this survey in a VR journal.

Second, in addition to this new work funded by the NIDILRR grant, we have two research papers that are currently under submission at leading journals. The first applies our modeling framework to estimate the ROI for clients who are blind or vision-impaired (Clapp et al., 2024). Using data on clients who applied for services from VR agencies in Virginia, Maryland, and Oklahoma in State Fiscal Year (SFY) 2007, we mostly find that the large administrative costs lead to negative rates of return. When published, this will be the first paper in the economics literature to consider the impact of VR on people with vision impairments and one of the few to evaluate labor market outcomes for people with vision impairments. The second paper, which has been submitted to a VR journal, examines the differences between the economic and taxpayer return on investment of VR programs and demonstrates that the economic return is generally the more relevant measure of the efficacy of a

VR program. To do this, we use the VR-ROI model, which finds a substantial estimated impact of VR on earnings but translates into only a small impact on the taxpayer return. That is, the cost of VR is large relative to the lifetime changes in tax receipts. For example, while the estimated median annualized rate of return for clients with physical impairments is 174% (Dean et al., 2018), we estimate that less than half of VR recipients with physical impairments have a positive taxpayer return. We view this as strong evidence that taxpayer rates of return are not appropriate measures of program value.

Finally, a number of other avenues for future research may extend the model in important ways. These include:

- **Non-labor market outcomes**: Evaluating whether VR impacts non-labor market outcomes, such as improvements in the quality of life, independence, and overall well-being of VR clients. These outcomes are crucial for comprehensive understanding of the impact of VR programs. In fact, as noted in Chap. 2, NIDILRR's long-run plan for sponsored-research priorities includes community living and participation and health and function; distinctly nonpecuniary benefits. Their importance in the lives of individuals with disabilities has been demonstrated by the broader VR literature outside the public VR agency context (Saunders et al., 2000; Fleming et al., 2013). Most telling, Balcazar et al., (2023) surveyed VR counselors about what they view as a successful outcome for their clients. The survey was open-ended, so counselors could list multiple outcomes. While the majority reported that the client's employment defined success, 33% also considered independent living to be a positive outcome for their clients. Educational placements were viewed similarly by 30% of counselors. Unsurprisingly given these views, 18% of counselors provided services with an independent living goal in mind, and 10% with an educational goal in mind. By developing measures and methods to assess these nonfinancial benefits, we can provide a more complete picture of the value of VR services.
- **Interactions with transportation**: Investigating how transportation access and services interact with VR outcomes will be valuable, since transportation can be a significant barrier to employment for individuals with disabilities. Understanding the role of transportation in facilitating or hindering access to VR services and employment opportunities can inform the development of more effective support strategies.
- **Evaluation of Pre-ETS (Pre-Employment Transition Services)**: Assessing the effectiveness of Pre-ETS in preparing students with disabilities for employment, while considering both short- and long-term outcomes, will enable us to refine early interventions and ensure that they provide the necessary support for successful transitions from school to work.
- **Non-purchased services:** By examining the usage, cost, and impact of non-purchased services within VR programs, we can better assess their contribution to client outcomes and identify ways to optimize service delivery.
- **Changes over time**: Given the qualitative differences between results from the 2000 and 2012 cohorts (see Chap. 5), an analysis of important programmatic and

environmental changes would be constructive. This would be helpful for understanding both the differences and important issues with respect to future ROI analyses.

- **Long-run effects:** Likewise, it would be useful to understand how many years of data are necessary to accurately measure the long-run effects of VR. As noted above, a critical difference between the 2000 and 2012 analyses is that we had access to 5 years of post-service data in 2012 and 10 years in 2000. With more data further into the future, will the long-run estimated benefits for 2012 become positive? This is consistent with results found by Dean et al. (2015, 2017, 2018, 2019).

7.5 Using Results from the VR-ROI Model

The utility of the VR-ROI model varies depending on the stakeholder using the results:

- **Counselors** may use the model as a supplementary tool when working with clients to develop individualized plans for employment and help them understand the potential financial outcomes of different services. However, counselors should also take the personal goals of the client and nonfinancial benefits of VR services into account and tailor services to meet each client's unique needs.
- **For administrators**, the VR-ROI model can serve as a broader resource for evaluating the overall effectiveness of programs and help identify which services tend to provide the most value across client populations. These data can be instrumental in shaping budgetary decisions and optimizing service provision to meet agency goals. At the same time, administrators should also consider other objectives of the VR program.
- **Policymakers** may view the VR-ROI model as a tool for justifying funding decisions or making programmatic adjustments based on financial returns. However, it is crucial that they recognize the model's limitations and understand that not all services can be evaluated solely based on economic outcomes. The nonfinancial impacts of VR services such as improving independence, quality of life, and social integration are also important in evaluating the success of these programs.

Each of these stakeholders uses the VR-ROI model for different purposes, and therefore the results must be interpreted and applied within the appropriate context. Understanding the model's strengths and limitations ensures that the data support better decision-making across various levels of the vocational rehabilitation process.

For instance, a negative ROI estimate does not necessarily mean that a client should not receive a particular service. Rather, it highlights the importance of balancing the specific needs and abilities of the client with the knowledge that some evidence suggests that the service may not be financially beneficial. In addition, counselors and clients should consider the broader context, including nonfinancial benefits such as improvements in quality of life, independence, and social integration, and remember that results regarding the efficacy of VR in 2012 may not reflect subsequent changes to the program. The VR-ROI model might be used as one of many tools in the decision-making process. Counselors should work collaboratively with clients to make informed decisions that account for the individual's unique circumstances and goals. This approach ensures that services are tailored to meet the specific needs of each client rather than relying solely on generalized financial metrics.

As we continue to refine the VR-ROI model, it is essential to remember that data-driven insights are just one aspect of a complex decision-making process. The ultimate goal is to support individuals with disabilities in achieving meaningful employment and independence. This requires that counselors and administrators interpret VR-ROI results responsibly and ensure that service decisions reflect both the financial data and each client's circumstances. Balanced application of the VR-ROI model reinforces the commitment of VR professionals to provide high-quality, client-centered services that support the diverse needs of individuals with disabilities.

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Appendices

Appendices of Chap. 3

Appendix 1: Mathematical Model for VR Service Receipt and Labor Market Outcomes

The model described here is very similar to those in Dean et al. (2015, 2017, 2018, 2019) and Schmidt et al. (2019). Most of the notation in this model is the same as in Dean et al. (2015). Our model consists of an equation for VR service receipt, one for employment, and one for log quarterly earnings conditional on employment.

The first model is a description of the value of using each service or any combination of services. Let y_{ij}^{*} be the latent value for person i of using service $j = 1, 2, \ldots, J$. We specify the value as

$$\begin{aligned} y_{ij}^{*} &= x_i^{y}\beta_j + u_{ij}^{y} + \varepsilon_{ij} \\ \varepsilon_{ij} &\sim iid\text{Logistic} \end{aligned} \tag{A.1}$$

where x_i^{y} is a vector of exogenous explanatory variables and u_{ij}^{y} is an error whose structure is specified below. As is usually the case, it is assumed that $y_{ij} = 1\left(y_{ij}^{*} > 0\right)$. Each person i can choose as many services as are available. We allow for multiple choices because we observe multiple choices throughout the data. For example, Table A.1 shows the frequency of multiple service choices in the data used in Dean et al. (2017).

Next, let z_{it}^{*} be the latent value to person i of working in quarter t. We specify the value as

$$z_{it}^{*} = x_{it}^{z}\gamma^{z} + \sum_{k=1}^{K} d_{ik} \sum_{j=1}^{J} \alpha_{jk}^{z} y_{ij} + u_{it}^{z} + \varphi_{it}^{z} \tag{A.2}$$

where x_{it}^{z} is a vector of (possibly) time-varying exogenous explanatory variables, d_{ik} is a dummy variable equal to 1 iff $\tau_k < t - t_a \leq \tau_{k+1}$, where t_a is the quarter of application, and u_{it}^{z} and φ_{it}^{z} are errors whose structure is specified below. We divide

C. M. Clapp et al., *Evaluation of Vocational Rehabilitation Services*, Diversity and Inclusion Research, https://doi.org/10.1007/978-3-031-86942-6

Table A.1 Frequency of service combinations

Rank	Combination	Frequency	Rank	Combination	Frequency
1	d	232	16	dtr	24
2	dr	119	17	dtermo	24
3	r	73	18	to	24
4	dt	63	19	dm	22
5	t	52	20	dterm	22
6	tm	45	21	drm	19
7	dtm	43	22	do	15
8	dtrmo	40	23	dro	14
9	m	36	24	rm	14
10	dtmo	31	25	e	13
11	tmo	30	26	dmo	13
12	dtrmo	29	27	dter	11
13	dtro	28	28	trm	10
14	dto	25	29	te	10
15	o	24	30	drmo	10

Initials: d=diagnosis & evaluation, t=training, e=education, r=restoration, m=maintenance, o=other

time into four periods and have α^z_{jk}, the effect of each service on the value of employment, depend on k in order to distinguish between quarters prior to, after in the short run, and after in the long run. We set the first period from $-\infty$ to -2 and the second period to -1. These are the quarters prior to application, and the second period allows for the Ashenfelter (1978) dip. The other two periods are from 1 to 8 and 9 to ∞; these are called the short run (1 to 8) and the long run (>8).

Finally, let w_{it} be the log wage for person i in quarter t conditional on i working in quarter t. We model this as

$$w_{it} = x^w_{it}\gamma^w + \sum_{k=1}^{K} d_{ik} \sum_{j=1}^{J} \alpha^w_{jk} y_{ij} + u^w_{it} + \varphi^w_{it} \tag{A.3}$$

where x^w_{it} is a vector of (possibly) time-varying exogenous explanatory variables, d_{ik} is a dummy variable defined the same as in Eq. (A.2), and u^w_{it} and φ^w_{it} are errors whose structure is specified below.[1]

The error structure of the model is

[1] Dean et al. (2017) adds an extra equation similar in structure to Eqs. (A.3) and (A.4) because it had data on Social Security (SS) benefits allowing for modelling how VR services affect receipt of SS benefits. Dean et al. (2018) adds another equation to allow for estimation of when a disability began. This was used again in Clapp et al. (2024) for people with vision impairments. Schmidt et al. (2019) changed the set of VR services because the appropriate way to define services had changed between 2000 and 2007. Clapp et al. (2024) used two extra services appropriate for people with vision impairments.

$$
\begin{aligned}
u_{ij}^{y} &= \lambda_{j1}^{y} e_{i1} + \lambda_{j2}^{y} e_{i2}, \\
u_{it}^{z} &= \lambda_{1}^{z} e_{i1} + \lambda_{2}^{z} e_{i2} + \eta_{it}^{z}, \\
u_{it}^{w} &= \lambda_{1}^{w} e_{i1} + \lambda_{2}^{w} e_{i2} + \eta_{it}^{w}, \\
\eta_{it}^{z} &= \rho_{\eta} \eta_{it-1}^{z} + \zeta_{it}^{z}, \\
\eta_{it}^{w} &= \rho_{\eta} \eta_{it-1}^{w} + \zeta_{it}^{w}, \\
\begin{pmatrix} \zeta_{it}^{z} \\ \zeta_{it}^{w} \end{pmatrix} &\sim iidN\left[0,\ \sigma_{\zeta}^{2} \begin{pmatrix} 1 & \rho_{\zeta} \\ \rho_{\zeta} & 1 \end{pmatrix}\right], \\
\begin{pmatrix} e_{i1} \\ e_{i2} \end{pmatrix} &\sim iidN[0, I], \\
\zeta_{it}^{z} &\sim iidN[0, 1], \\
\zeta_{it}^{w} &\sim iidN\left[0, \sigma_{w}^{2}\right].
\end{aligned}
\tag{A.4}
$$

We include (e_{i1}, e_{i2}) to allow for two common factors with factor loadings $\left(\lambda_{jk}^{y}, \lambda_{k}^{z}, \lambda_{k}^{w}\right)$. These common factors naturally cause correlations across the three equations, (A.1)–(A.3). We allow for serial correlation ρ_{η} because it is well known that individual employment and earnings are serially correlated. We allow for correlation between employment and earnings ρ_{ζ} because we think that such correlation might be important.

Define the vector of errors as $u_i' = \left(u_{i1}^{y}, u_{i2}^{y}, .., u_{iJ}^{y}, u_{i1}^{z}, u_{i1}^{w}, .., u_{iT}^{z}, u_{iT}^{w}\right)$. The covariance matrix of u_i is

$$
\Omega = \begin{pmatrix} A & B' \\ B & C+D \end{pmatrix}
$$

where

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

$$
C = H \bigotimes \begin{pmatrix}
1 & 1 & \cdots & 1 \\
1 & 1 & \cdots & 1 \\
\vdots & \vdots & \ddots & \vdots \\
1 & 1 & \cdots & 1
\end{pmatrix},
$$

$$H = \begin{pmatrix} \sum_k \left(\lambda_k^z\right)^2 & \sum_k \lambda_k^z \lambda_k^w \\ \sum_k \lambda_k^z \lambda_k^w & \sum_k \left(\lambda_k^z\right)^2 \end{pmatrix},$$

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

$$B = \begin{pmatrix} \sum_k \lambda_{1k}^y \lambda_{1k}^z & \sum_k \lambda_{2k}^y \lambda_{1k}^z & \cdots & \sum_k \lambda_{Jk}^y \lambda_{1k}^z \\ \sum_k \lambda_{1k}^y \lambda_{1k}^w & \sum_k \lambda_{2k}^y \lambda_{1k}^w & \cdots & \sum_k \lambda_{Jk}^y \lambda_{1k}^w \\ \vdots & \vdots & \ddots & \vdots \\ \sum_k \lambda_{1k}^y \lambda_{1k}^z & \sum_k \lambda_{2k}^y \lambda_{1k}^z & \cdots & \sum_k \lambda_{Jk}^y \lambda_{1k}^z \\ \sum_k \lambda_{1k}^y \lambda_{1k}^w & \sum_k \lambda_{2k}^y \lambda_{1k}^w & \cdots & \sum_k \lambda_{Jk}^y \lambda_{1k}^w \end{pmatrix}.$$

Appendix 2: Estimation

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

$$\theta_y = \left(\beta_j, \lambda_{j1}^y, \lambda_{j2}^y\right)_{j=1}^J,$$

$$\theta_z = \left(\gamma, \lambda_1^z, \lambda_2^z, \rho_\eta, \sigma_\zeta^2, \rho_\zeta, \left[\alpha_{jk}^z\right]_{j=1}^J\right),$$

$$\theta_w = \left(\delta, \lambda_1^w, \lambda_2^w, \rho_\eta, \sigma_\zeta^2, \rho_\zeta, \left[\alpha_{jk}^w\right]_{j=1}^J\right).$$

We estimate the model using Maximum Simulated Likelihood (MSL) estimation. The likelihood contribution for observation i is

$$L_i = \int L_i(u_i) d\Psi(u_i|\Omega)$$

where $\Psi(u_i|\Omega)$ is the joint distribution of u_i,

$$L_i(u_i) = L_i^y(u_i^y)\prod_{t=1}^{T} L_{it}^{zw}\left(u_{it}^z, u_{it}^w\right),$$

$$L_i^y(u_i^y) = \prod_{j=1}^{J} \frac{\exp\left\{x_i^y\beta_j + u_{ij}^y\right\}^{y_{ij}}}{1 + \exp\left\{x_i^y\beta_j + u_{ij}^y\right\}}$$

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

$$L_{it}^0\left(u_{it}^z, u_{it}^w\right) = 1 - \Phi\left(x_{it}^z\gamma^z + \sum_{k=1}^{K} d_{ik}\sum_{j=1}^{J}\alpha_{jk}^z y_{ij} + u_{ijt}^z\right),$$

$$L_{it}^1\left(u_{it}^z, u_{it}^w\right) = \frac{1}{\sigma_w}\phi\left(\frac{w_{it} - x_{it}^w\gamma^w - \sum_{k=1}^{K} d_{ik}\sum_{j=1}^{J}\alpha_{jk}^w y_{ij} - u_{ijt}^w}{\sigma_w}\right).$$

$$\Phi\left(x_{it}^z\gamma^z + \sum_{k=1}^{K} d_{ik}\sum_{j=1}^{J}\alpha_{jk}^z y_{ij} + u_{ijt}^z\right).$$

In general, it is difficult to evaluate the integral for L_i. But it can be done quickly using simulation of the errors of the model. We do so also using antithetic acceleration (Geweke, 1988). Using antithetic acceleration significantly reduces simulation error enough so that the asymptotics of the estimation procedure depends only on the sample size.

Appendix of Chap. 5

Appendix 3: Supplementary Tables

Table A.2 Proportion of Virginia applicants receiving services by service source and disability type

	Virginia CI				Virginia MI			
Service	Neither	PS only	Ag only	Both	Neither	PS only	Ag only	Both
Diagnosis and evaluation	0.449	0.201	0.222	0.128	0.500	0.275	0.118	0.107
Training	0.724	0.141	0.093	0.042	0.749	0.175	0.047	0.028
Education	1.000	0.000	0.000	0.000	0.978	0.022	0.000	0.000
Restoration	0.711	0.162	0.095	0.032	0.718	0.177	0.067	0.039
Maintenance	0.569	0.274	0.074	0.083	0.526	0.392	0.030	0.052
Placement	0.778	0.000	0.222	0.000	0.787	0.002	0.211	0.000
Supported employment	0.661	0.339	0.000	0.000	0.718	0.282	0.000	0.000
Assistive technology	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mobility and orientation	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	Virginia PI				Virginia BVI			
Service	Neither	PS only	Ag only	Both	Neither	PS only	Ag only	Both
Diagnosis and evaluation	0.412	0.299	0.143	0.145	0.602	0.269	0.060	0.069
Training	0.735	0.167	0.061	0.038	0.820	0.177	0.001	0.002
Education	0.971	0.029	0.000	0.000	0.913	0.087	0.000	0.000
Restoration	0.655	0.165	0.117	0.063	0.558	0.441	0.000	0.000
Maintenance	0.526	0.355	0.048	0.071	0.545	0.448	0.000	0.007
Placement	0.781	0.002	0.217	0.000	0.857	0.055	0.084	0.004
Supported employment	0.761	0.239	0.000	0.000	0.914	0.086	0.000	0.000
Assistive technology	0.000	0.000	0.000	0.000	0.676	0.085	0.083	0.155
Mobility and orientation	0.000	0.000	0.000	0.000	0.711	0.023	0.220	0.046

Table A.3 Proportion of Maryland applicants receiving services by service source and disability type

Service	Maryland CI				Maryland MI			
	Neither	PS Only	Ag Only	Both	Neither	PS Only	Ag Only	Both
Diagnosis and evaluation	0.286	0.461	0.044	0.209	0.329	0.460	0.035	0.176
Training	0.683	0.211	0.059	0.047	0.768	0.162	0.038	0.032
Education	0.945	0.055	0.000	0.000	0.927	0.072	0.001	0.000
Restoration	0.901	0.061	0.018	0.020	0.878	0.093	0.013	0.015
Maintenance	0.663	0.320	0.006	0.010	0.646	0.341	0.003	0.009
Placement	0.659	0.341	0.000	0.000	0.605	0.395	0.000	0.000
Supported employment	0.798	0.202	0.000	0.000	0.782	0.218	0.000	0.000
Assistive technology	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mobility and orientation	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	Maryland PI				Maryland BVI			
Service	Neither	PS only	Ag only	Both	Neither	PS only	Ag only	Both
Diagnosis and evaluation	0.305	0.439	0.040	0.216	0.298	0.215	0.214	0.273
Training	0.757	0.173	0.036	0.033	0.717	0.181	0.045	0.057
Education	0.909	0.090	0.000	0.000	0.893	0.107	0.000	0.000
Restoration	0.804	0.136	0.021	0.038	0.851	0.149	0.000	0.000
Maintenance	0.619	0.356	0.006	0.019	0.380	0.588	0.006	0.026
Placement	0.689	0.310	0.000	0.000	0.813	0.143	0.032	0.011
Supported employment	0.846	0.154	0.000	0.000	0.916	0.084	0.000	0.000
Assistive technology	0.000	0.000	0.000	0.000	0.343	0.246	0.062	0.349
Mobility and orientation	0.000	0.000	0.000	0.000	0.663	0.098	0.146	0.094

Table A.4 Proportion of Kentucky applicants receiving purchased services by disability type

Service	Kentucky CI		Kentucky MI		Kentucky PI	
	No PS	PS	No PS	PS	No PS	PS
Diagnosis and evaluation	0.191	0.809	0.246	0.754	0.275	0.725
Training	0.871	0.129	0.858	0.142	0.858	0.142
Education	0.882	0.118	0.844	0.155	0.805	0.195
Restoration	0.770	0.230	0.719	0.281	0.658	0.342
Maintenance	0.739	0.261	0.670	0.330	0.693	0.307
Placement	0.677	0.323	0.807	0.193	0.828	0.172
Supported employment	0.857	0.143	0.913	0.087	0.932	0.068
Assistive technology	0.000	0.000	0.000	0.000	0.000	0.000
Mobility and orientation	0.000	0.000	0.000	0.000	0.000	0.000

Table A.5 Proportion of North Carolina applicants receiving purchased services by disability type

Service	North Carolina CI No PS	North Carolina CI PS	North Carolina MI No PS	North Carolina MI PS	North Carolina PI No PS	North Carolina PI PS	North Carolina BVI No PS	North Carolina BVI PS
Diagnosis and evaluation	0.688	0.313	0.542	0.458	0.335	0.665	0.347	0.653
Training	0.736	0.264	0.876	0.124	0.908	0.092	0.957	0.043
Education	0.936	0.064	0.894	0.106	0.882	0.118	0.939	0.061
Restoration	0.298	0.702	0.459	0.541	0.427	0.573	0.440	0.560
Maintenance	0.599	0.401	0.533	0.467	0.617	0.383	0.796	0.204
Placement	0.954	0.046	0.981	0.019	0.988	0.012	0.998	0.002
Supported employment	0.579	0.421	0.729	0.271	0.840	0.159	0.984	0.016
Assistive technology	0.000	0.000	0.000	0.000	0.000	0.000	0.674	0.326
Mobility and orientation	0.000	0.000	0.000	0.000	0.000	0.000	0.995	0.005

Table A.6 Proportion of Texas applicants receiving purchased services by disability type

Service	Texas CI No PS	Texas CI PS	Texas MI No PS	Texas MI PS	Texas PI No PS	Texas PI PS	Texas BVI No PS	Texas BVI PS
Diagnosis and evaluation	0.229	0.771	0.159	0.841	0.178	0.822	0.060	0.940
Training	0.640	0.359	0.639	0.361	0.789	0.211	0.800	0.200
Education	0.881	0.119	0.902	0.098	0.913	0.086	0.925	0.075
Restoration	0.799	0.201	0.576	0.424	0.461	0.539	0.449	0.551
Maintenance	0.556	0.444	0.448	0.552	0.543	0.457	0.360	0.640
Placement	0.811	0.189	0.815	0.184	0.884	0.116	0.981	0.019
Supported employment	0.642	0.358	0.825	0.175	0.910	0.090	0.918	0.082
Assistive technology	0.000	0.000	0.000	0.000	0.000	0.000	0.337	0.663
Mobility and orientation	0.000	0.000	0.000	0.000	0.000	0.000	0.664	0.336

Table A.7 Percentage of individuals with IPE receiving purchased services by disability type

Service	Virginia		Maryland		Kentucky	North Carolina		Texas	
	Non-BVI	BVI	Non-BVI	BVI	Non-BVI	Non-BVI	BVI	Non-BVI	BVI
Diagnosis and evaluation	42	46	67	52	73	49	66	81	91
Training	28	26	27	29	17	20	6	28	20
Education	3	13	9	14	22	12	8	10	7
Restoration	23	65	11	18	29	62	74	41	55
Maintenance	55	66	46	78	37	50	25	47	64
Placement	0	8	52	21	27	4	0	15	2
Supported employment	42	13	27	10	11	34	2	16	8
Assistive technology		36		75			43		66
Mobility and orientation		10		26			1		34

Notes: (1) Virginia's general agency does not purchase *placement* services. (2) *AT* and *O&M* are included in *restoration* for non-BVI samples. (3) We did not collect BVI data for Kentucky

Table A.8 Employment rates mean nominal quarterly earnings (if employed), Virginia

Virginia	% employment			Mean earning if employed		
	CI	MI	PI	CI	MI	PI
# of applicants in cohort	3184	3578	2725	3184	3578	2725
2 or more Qtrs before application	17%	31%	33%	$2141	$2622	$3612
1 quarter before application	22%	24%	24%	$1707	$1784	$2466
First 8 Qtrs after application (short run)	32%	31%	28%	$1813	$1923	$2365
More than 8 Qtrs after app. (long run)	44%	33%	31%	$2489	$2153	$2547

Table A.9 Employment rates mean nominal quarterly earnings (if employed), Maryland

Maryland	% employment			Mean earning if employed		
	CI	MI	PI	CI	MI	PI
# of applicants in cohort	3010	4665	3414	3010	4665	3414
2 or more Qtrs before application	22%	30%	35%	$1917	$2422	$3423
1 quarter before application	24%	21%	22%	$1533	$1745	$2406
First 8 Qtrs after application (short run)	34%	28%	26%	$1614	$1809	$2427
More than 8 Qtrs after app. (long run)	42%	31%	27%	$2060	$2060	$2602

Table A.10 Employment rates mean nominal quarterly earnings (if employed), Kentucky

	% employment			Mean earning if employed		
Kentucky	CI	MI	PI	CI	MI	PI
# of applicants in cohort	2372	4656	3526	2372	4656	3526
2 or more Qtrs before application	20%	31%	35%	$2275	$2919	$4094
1 quarter before application	26%	34%	34%	$2032	$2397	$3463
First 8 Qtrs after application (short run)	33%	33%	31%	$2010	$2406	$3042
More than 8 Qtrs after app. (long run)	39%	33%	31%	$2789	$2849	$3316

Table A.11 Employment rates mean nominal quarterly earnings (if employed), North Carolina

	% employment			Mean earning if employed		
North Carolina	CI	MI	PI	CI	MI	PI
# of applicants in cohort	6131	8085	6239	6131	8085	6239
2 or more Qtrs before application	19%	25%	33%	$1867	$2306	$3408
1 quarter before application	19%	22%	27%	$1569	$1664	$2574
First 8 Qtrs after application (short run)	30%	29%	28%	$1671	$1800	$2459
More than 8 Qtrs after app. (long run)	41%	32%	29%	$2155	$2037	$2698

Table A.12 Employment rates mean nominal quarterly earnings (if employed), Texas

	% employment			Mean earning if employed		
Texas	CI	MI	PI	CI	MI	PI
# of applicants in cohort	3580	4590	7746	3580	4590	7746
2 or more Qtrs before application	23%	33%	47%	$1994	$3035	$4798
1 quarter before application	26%	28%	41%	$1535	$2321	$3965
First 8 Qtrs after application (short run)	46%	41%	46%	$1804	$2378	$3834
More than 8 Qtrs after app. (long run)	56%	43%	45%	$2471	$2685	$4041

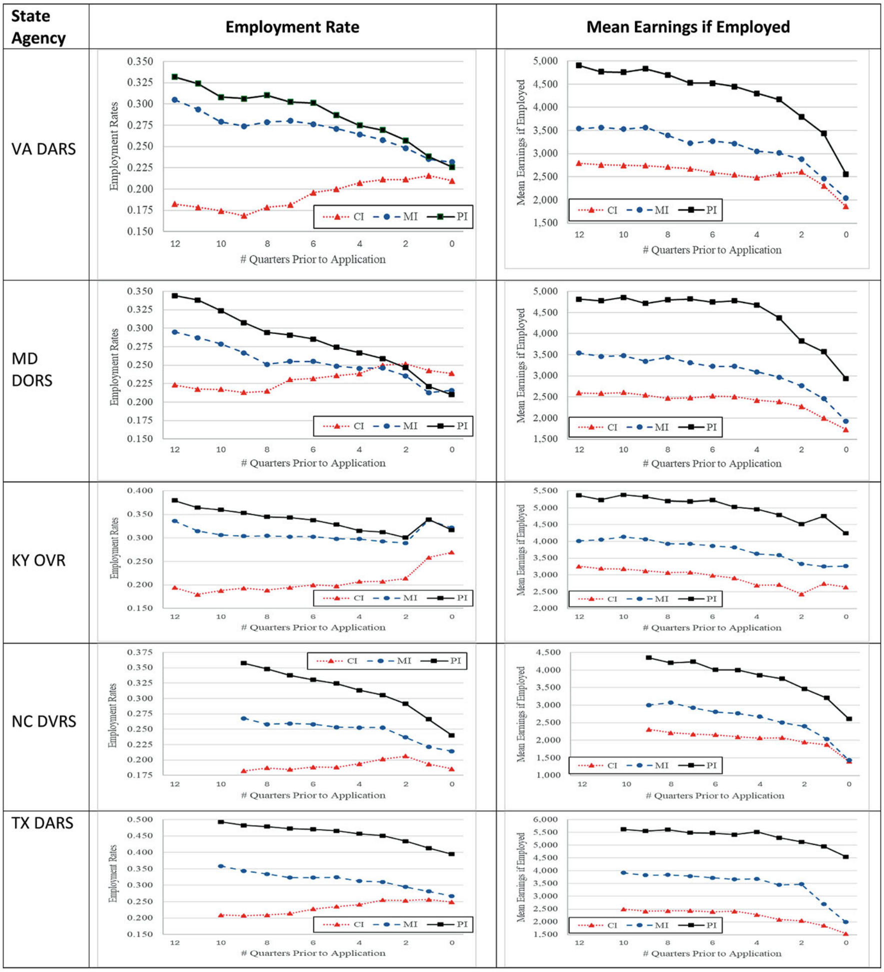

Fig. A.1 Quarterly employment rates prior to application by disability cohort

Table A.13 Short- and long-run effects of VR Services, Virginia

	CI		MI		PI		BVI	
Service type	Employment	Earnings	Employment	Earnings	Employment	Earnings	Employment	Earnings
	Long run							
Diagnosis and evaluation	−0.102	−0.251	−0.024	−0.049	0.024	0.000	0.064	0.246
Training	0.011	0.023	0.002	−0.060	0.074	0.052	0.134	0.088
Education			0.058	0.251	0.015	0.246	0.058	−0.178
Restoration	−0.086	−0.054	0.042	0.129	−0.002	0.067	−0.031	−0.006
Maintenance	−0.058	−0.036	−0.018	0.055	−0.017	−0.016	0.050	0.108
Placement							0.068	−0.027
Job supports	0.116	0.100	0.147	0.099	0.137	0.064	0.142	−0.118
Assistive technology							0.072	−0.019
Orientation and mobility							0.035	0.326
	Short run							
Diagnosis and evaluation	−0.043	−0.154	−0.019	−0.098	−0.001	0.028	0.056	0.216
Training	−0.016	0.089	−0.017	−0.063	−0.065	0.010	0.067	−0.011
Education			0.087	−0.105	0.033	0.126	0.044	−0.049
Restoration	−0.067	0.020	−0.006	0.129	0.015	0.051	0.009	−0.035
Maintenance	−0.057	−0.016	−0.004	0.034	0.001	−0.023	0.081	0.112
Placement							0.130	0.092
Job supports	0.064	0.141	0.075	−0.038	0.076	0.066	0.047	−0.226
Assistive technology							0.059	−0.010
Orientation and mobility							0.108	0.213

Fig. A.2 Virginia long-run employment estimates

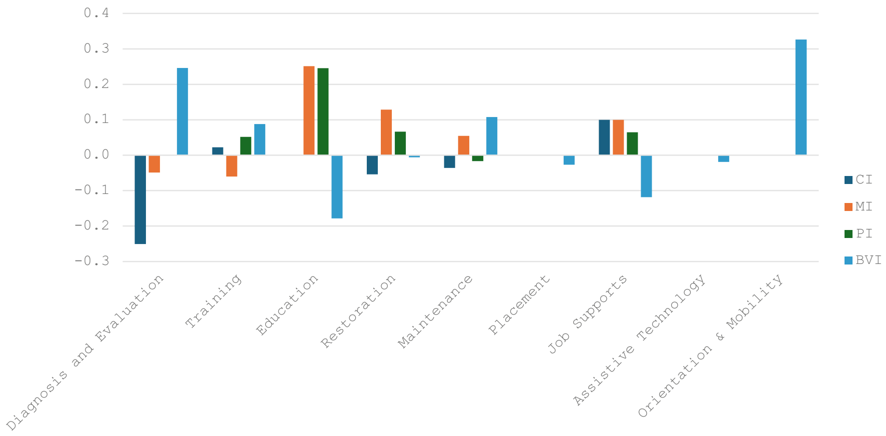

Fig. A.3 Virginia long-run earnings estimates

Table A.14 Short- and long-run effects of VR Services, Maryland

	CI		MI		PI		BVI	
Service type	Employment	Earnings	Employment	Earnings	Employment	Earnings	Employment	Earnings
	Long run							
Diagnosis and evaluation	−0.100	−0.132	0.029	0.015	0.017	0.011	0.009	0.110
Training	0.045	0.023	0.028	−0.008	0.043	0.067	0.139	0.156
Education	−0.055	−0.223	0.002	−0.118	0.032	0.077	0.041	0.353
Restoration	−0.123	−0.168	−0.013	0.070	0.030	−0.062	−0.077	0.027
Maintenance	−0.012	0.041	0.059	−0.041	0.065	0.066	−0.115	−0.300
Placement							0.193	0.015
Job supports	0.038	0.027	0.061	−0.024	0.034	0.036	0.185	−0.095
Assistive technology							0.095	0.015
Orientation and mobility							−0.034	−0.179
	Short run							
Diagnosis and evaluation	−0.098	−0.112	0.001	−0.003	0.016	0.038	−0.053	0.141
Training	0.005	0.052	−0.086	−0.034	−0.001	−0.014	0.052	0.120
Education	−0.018	0.058	−0.001	−0.013	0.018	0.100	0.018	−0.122
Restoration	−0.005	0.005	0.010	0.058	0.055	0.049	−0.051	−0.029
Maintenance	0.015	0.009	0.114	0.046	0.036	0.052	−0.100	−0.214
Placement							0.116	−0.130
Job supports	0.116	0.143	0.119	0.029	0.027	0.042	0.060	−0.089
Assistive technology							0.105	−0.028
Orientation and mobility							−0.075	−0.160

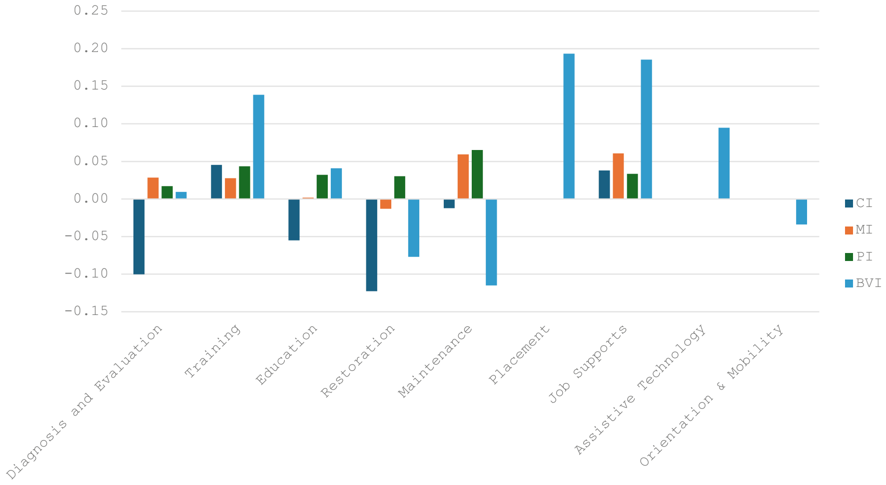

Fig. A.4 Maryland long-run employment estimates

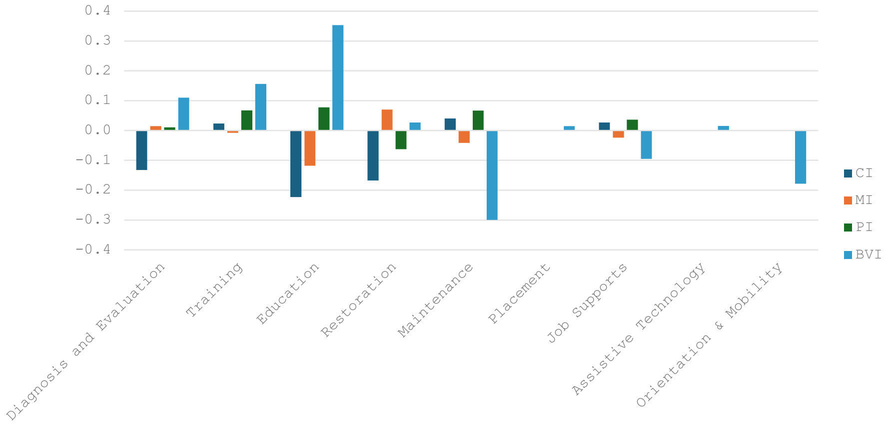

Fig. A.5 Maryland long-run earnings estimates

Table A.15 Short- and long-run effects of VR Services, Kentucky

	CI		MI		PI	
Service type	Employment	Earnings	Employment	Earnings	Employment	Earnings
	Long run					
Diagnosis and evaluation	−0.085	−0.171	−0.002	−0.004	−0.061	−0.117
Training	0.007	0.005	0.049	0.048	−0.039	−0.076
Education	0.029	0.235	0.049	0.214	0.044	0.151
Restoration	−0.121	−0.109	−0.066	−0.093	−0.090	0.046
Maintenance	0.077	0.100	0.023	0.010	0.063	0.002
Placement	0.124	0.079	0.138	0.219	0.209	0.055
Job supports	0.117	−0.145	0.135	−0.006	0.123	−0.023
Assistive technology						
Orientation and mobility						
	Short run					
Diagnosis and evaluation	−0.100	−0.093	0.048	−0.005	0.017	−0.150
Training	0.033	0.006	0.074	−0.115	−0.001	−0.107
Education	0.100	0.016	0.000	−0.023	0.019	−0.082
Restoration	−0.211	−0.069	−0.172	−0.091	−0.091	0.103
Maintenance	0.173	0.120	0.022	0.040	0.038	0.068
Placement	−0.055	0.038	−0.092	0.157	−0.096	0.022
Job supports	−0.162	−0.076	−0.075	0.054	−0.096	0.078
Assistive technology						
Orientation and mobility						

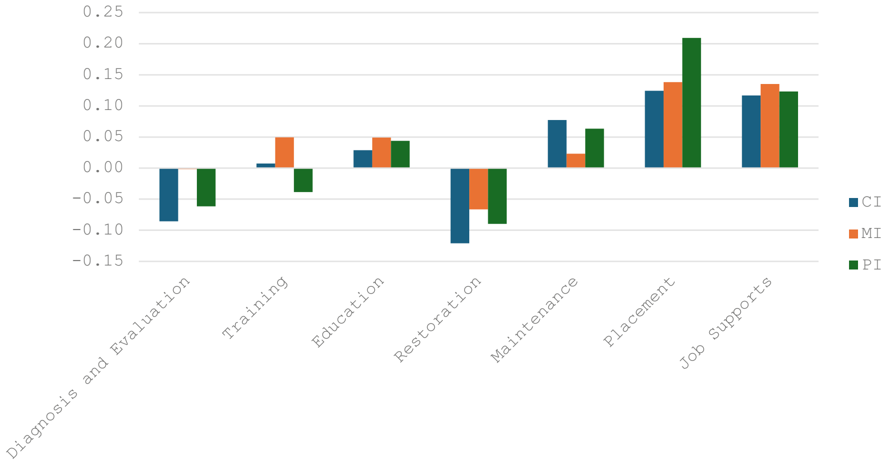

Fig. A.6 Kentucky long-run employment estimates

Fig. A.7 Kentucky long-run earnings estimates

Table A.16 Short- and long-run effects of VR Services, North Carolina

	CI		MI		PI		BVI	
Service type	Employment	Earnings	Employment	Earnings	Employment	Earnings	Employment	Earnings
	Long run							
Diagnosis and evaluation	−0.076	−0.102	−0.037	−0.127	−0.018	−0.050	0.027	0.160
Training	0.126	0.237	0.086	0.099	0.095	0.162	0.213	0.204
Education	0.040	0.038	0.012	−0.018	0.058	0.031	0.046	0.082
Restoration	0.001	−0.054	0.026	0.011	−0.112	0.038	−0.108	−0.103
Maintenance	0.019	−0.004	0.076	0.039	0.098	0.092	0.055	−0.125
Placement	0.267	0.123	0.189	0.013	0.059	−0.084		
Job supports	0.004	−0.062	0.089	0.146	0.180	0.109	0.247	0.638
Assistive technology							0.108	0.118
Orientation and mobility							0.108	−0.483
	Short run							
Diagnosis and evaluation	−0.063	−0.051	−0.058	−0.121	−0.025	−0.071	0.038	0.070
Training	0.056	0.170	0.057	0.122	0.060	0.117	0.198	0.044
Education	−0.016	−0.032	−0.040	−0.232	0.018	−0.085	−0.012	−0.197
Restoration	0.000	−0.032	−0.006	−0.019	−0.068	0.067	−0.096	−0.079
Maintenance	0.040	0.005	0.101	0.108	0.060	−0.030	−0.074	−0.199
Placement	−0.065	−0.129	−0.105	−0.302	−0.119	−0.332		
Job supports	0.057	0.070	0.082	0.167	0.134	0.118	0.229	0.656
Assistive technology							0.071	0.093
Orientation and mobility							0.241	0.122

Fig. A.8 North Carolina long-run employment estimates

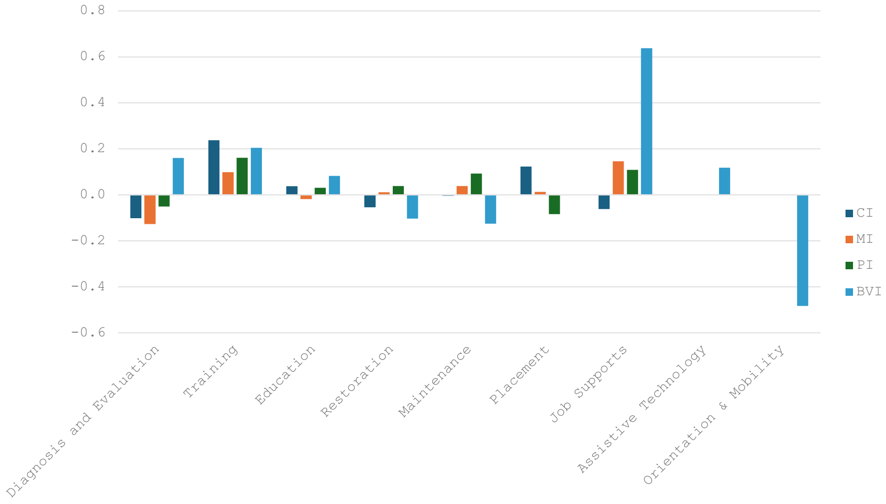

Fig. A.9 North Carolina long-run earnings estimates

Table A.17 Short- and long-run effects of VR Services, Texas

	CI		MI		PI		BVI	
Service type	Employment	Earnings	Employment	Earnings	Employment	Earnings	Employment	Earnings
	Long run							
Diagnosis and evaluation	0.062	0.035	0.027	−0.084	0.026	−0.005	0.032	0.519
Training	0.119	0.198	0.089	0.175	0.135	0.015	0.113	−0.049
Education	0.027	0.265	0.024	0.052	0.105	0.052	0.193	0.058
Restoration	−0.181	−0.379	−0.078	−0.067	−0.092	−0.077	−0.223	−0.281
Maintenance	−0.008	−0.037	0.054	0.025	0.013	0.047	−0.054	−0.075
Placement	0.046	−0.072	0.092	0.064	0.240	−0.037		
Job supports	0.016	−0.223	0.209	0.203	0.205	0.042	0.162	−0.024
Assistive technology							0.248	0.121
Orientation and mobility							−0.084	−0.280
	Short run							
Diagnosis and evaluation	0.037	0.010	−0.027	−0.073	0.012	−0.010	−0.015	0.134
Training	0.038	0.078	0.023	0.038	0.071	−0.071	−0.032	−0.010
Education	−0.009	0.148	−0.145	−0.166	−0.116	−0.242	0.086	−0.248
Restoration	−0.129	−0.371	−0.057	0.021	−0.038	0.013	−0.174	−0.179
Maintenance	0.022	−0.023	0.074	0.040	−0.015	−0.011	−0.121	−0.214
Placement	0.052	−0.068	0.094	0.063	0.135	−0.137		
Job supports	0.040	−0.084	0.150	0.206	0.117	0.014	0.031	−0.115
Assistive technology							0.204	0.053
Orientation and mobility							−0.144	−0.252

Fig. A.10 Texas long-run employment estimates

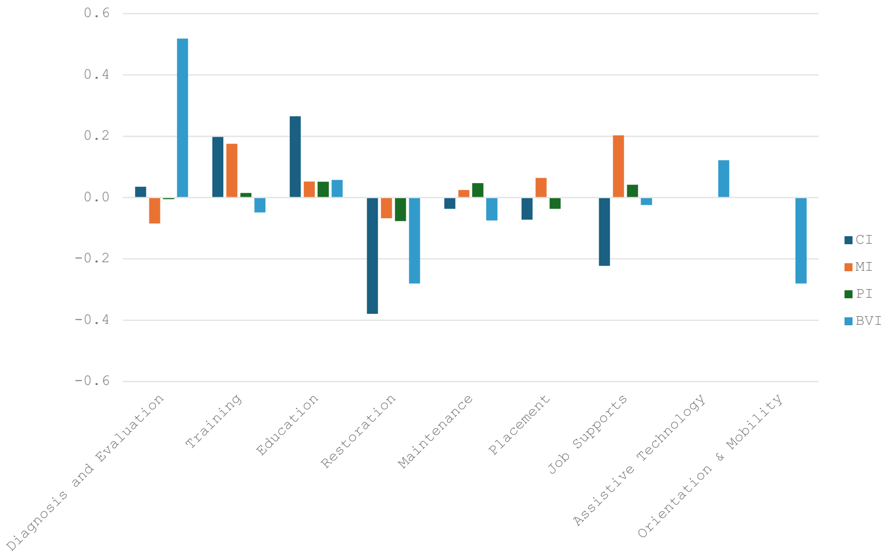

Fig. A.11 Texas long-run earnings estimates

Table A.18 Virginia, CI ($n = 3184$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−1.242	**	0.023	−54.817	6.466	**	0.019	336.687
1 Qtr pre-app dummy		−1.420	**	0.057	−24.828	6.144	**	0.060	102.204
Short run dummy		−0.924	**	0.024	−38.763	6.508	**	0.021	306.752
Long run dummy		−0.589	**	0.023	−25.451	6.934	**	0.019	361.739
Male	0.570	−0.038	**	0.005	−7.460	0.083	**	0.004	20.385
White	0.569	0.092	**	0.005	17.507	0.029	**	0.004	6.813
High school diploma	0.186	−0.003		0.007	−0.387	−0.159	**	0.006	−27.171
Some post-high school	0.071	0.346	**	0.009	37.078	0.169	**	0.008	21.935
Special education certificate	0.424	−0.108	**	0.007	−16.327	−0.198	**	0.006	−35.475
Married	0.041	0.039	**	0.010	4.007	0.279	**	0.009	30.312
# dependents	1.909	−0.006	**	0.002	−4.135	0.001		0.001	0.450
Cognitive impairment type—Intellectual	0.402	−0.120	**	0.008	−15.987	−0.221	**	0.007	−33.442
Cognitive impairment type—Learning	0.514	0.089	**	0.007	11.974	0.197	**	0.006	31.310
Additional disability—Mental illness	0.264	−0.235	**	0.006	−41.665	−0.133	**	0.005	−27.493
Additional disability—Physical	0.168	−0.374	**	0.007	−55.939	−0.335	**	0.005	−63.047
Additional disability—Substance abuse	0.038	−0.316	**	0.010	−31.346	−0.216	**	0.009	−23.289
Additional disability—ASD	0.050	−0.248	**	0.013	−18.849	−0.144	**	0.012	−11.826
Additional disability—ADHD	0.193	0.204	**	0.006	32.344	0.164	**	0.006	29.635
Significant disability	0.080	−0.006		0.015	−0.391	0.059	**	0.015	3.895
Most significant disability	0.896	0.085	**	0.012	7.061	0.128	**	0.014	9.418
Dummy for young age	0.066	−0.977	**	0.052	−18.838	0.055		0.065	0.851
Dummy for prior VR spell	0.194	0.624	**	0.009	70.209	0.261	**	0.009	28.796

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	t-value	Coef	Sig	Std err	t-value
Age/100	0.246	0.103	0.142	0.728	1.536	**	0.027	57.162	2.669	**	0.022	123.067
Monthly Govt assistance ($1000s)	0.257	0.349	0.000	1.922	−0.548	**	0.008	−70.991	−0.631	**	0.006	−112.333
Transformed fed Govt Empl propensity	−0.191	0.035	−0.238	−0.003	−0.251	**	0.118	−2.119	1.003	**	0.059	17.064
Transformed out-of-state commuting	−0.184	0.046	−0.240	0.059	−1.358	**	0.099	−13.698				
Local employment rate	0.579	0.147	0.259	0.999	0.089	**	0.018	5.031	−0.118	**	0.016	−7.564

** is statistically significant at the 5% level, and * at the 10% level

Table A.19 Virginia, MI ($n = 3578$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.361	**	0.020	−18.009	6.622	**	0.018	376.783
1 Qtr pre-app dummy		−0.863	**	0.049	−17.645	6.189	**	0.057	108.715
Short run dummy		−0.448	**	0.022	−20.606	6.433	**	0.020	318.642
Long run dummy		−0.532	**	0.021	−25.569	6.546	**	0.018	358.117
Male	0.510	−0.040	**	0.004	−9.245	0.157	**	0.004	38.930
White	0.630	−0.037	**	0.005	−8.086	0.053	**	0.004	12.605
High school diploma	0.304	0.150	**	0.006	24.846	0.100	**	0.006	17.328
Some post-high school	0.313	0.241	**	0.006	38.900	0.283	**	0.006	49.456
Special education certificate	0.155	−0.022	**	0.009	−2.364	−0.129	**	0.008	−15.315
Married	0.110	0.152	**	0.007	22.496	0.264	**	0.006	41.945
# dependents	1.156	−0.025	**	0.002	−15.443	−0.014	**	0.002	−8.758
Additional disability—Intellectual	0.102	0.051	**	0.009	5.785	−0.095	**	0.008	−11.813
Additional disability—Learning disability	0.135	−0.063	**	0.007	−8.498	0.043	**	0.006	6.840
Additional disability—Mental illness	0.895	−0.079	**	0.007	−10.557	0.020	**	0.007	2.799
Additional disability—Physical	0.256	−0.048	**	0.005	−9.624	0.076	**	0.005	16.303
Additional disability—Hearing/speech	0.025	0.161	**	0.015	10.475	0.188	**	0.012	16.277
Additional disability—Substance abuse	0.186	−0.080	**	0.005	−14.957	−0.025	**	0.005	−4.728
Additional disability—ASD	0.046	−0.204	**	0.014	−14.138	−0.089	**	0.012	−7.174
Additional disability—ADHD	0.175	−0.067	**	0.007	−10.180	−0.079	**	0.006	−13.445
Significant disability	0.084	0.524	**	0.012	43.447	0.372	**	0.012	30.824
Most significant disability	0.880	0.192	**	0.011	17.431	0.110	**	0.011	9.721
Veteran	0.033	0.323	**	0.010	32.487	0.351	**	0.010	34.269
SMI (MI subgroup)	0.106	−0.400	**	0.007	−54.073	−0.383	**	0.007	−55.613
Dummy for young age	0.211	−0.394	**	0.008	−49.347	−0.570	**	0.010	−57.834
Dummy for prior VR spell	0.234	0.131	**	0.008	17.457	−0.096	**	0.008	−12.539

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.348	0.135	0.150	0.775	−1.087	**	0.021	−51.240	0.264	**	0.021	12.853
Monthly Govt assistance ($1000s)	0.383	0.452	0.000	4.889	−0.353	**	0.005	−74.126	−0.264	**	0.004	−64.555
Transformed fed Govt Empl propensity	−0.194	0.034	−0.238	−0.003	−2.021	**	0.111	−18.256	−1.005	**	0.040	−25.374
Transformed out-of-state commuting	−0.188	0.043	−0.240	0.059	0.236	**	0.099	2.381				
Local employment rate	0.595	0.149	0.220	0.999	0.172	**	0.014	12.472	0.211	**	0.012	17.376

** is statistically significant at the 5% level, and * at the 10% level

Table A.20 Virginia, PI (n = 2725)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.310	**	0.019	−16.087	7.300	**	0.016	466.712
1 Qtr pre-app dummy		−0.540	**	0.064	−8.489	7.143	**	0.060	119.973
Short run dummy		−0.392	**	0.024	−16.394	7.017	**	0.022	319.028
Long run dummy		−0.477	**	0.022	−21.612	7.214	**	0.019	389.728
Male	0.531	−0.112	**	0.005	−23.236	0.188	**	0.004	45.186
White	0.632	−0.032	**	0.005	−6.152	−0.083	**	0.004	−18.628
High school diploma	0.309	0.157	**	0.007	22.907	0.085	**	0.006	13.912
Some post-high school	0.256	0.142	**	0.007	19.773	0.246	**	0.006	38.473
College degree	0.095	0.301	**	0.009	33.372	0.592	**	0.008	77.635
Special education certificate	0.144	−0.034	**	0.011	−3.245	−0.234	**	0.009	−24.661
Married	0.194	0.086	**	0.006	14.066	0.296	**	0.005	56.115
# dependents	1.259	0.033	**	0.002	16.941	0.036	**	0.002	22.240
Additional disability—Intellectual	0.098	−0.019	**	0.009	−2.057	−0.285	**	0.008	−35.421
Additional disability—Learning disability	0.111	0.350	**	0.009	39.609	0.363	**	0.008	43.837
Additional disability—Mental illness	0.362	−0.125	**	0.005	−24.507	−0.119	**	0.004	−26.718
Additional disability—Physical	0.877	0.036	**	0.014	2.551	0.176	**	0.012	15.210
Additional disability—Hearing/speech	0.055	0.250	**	0.011	23.788	0.108	**	0.009	12.023
Additional disability—Substance abuse	0.097	0.013	*	0.008	1.626	−0.047	**	0.008	−6.079
Additional disability—ADHD	0.098	0.213	**	0.010	21.606	0.083	**	0.008	9.968
Significant disability	0.168	0.181	**	0.012	14.776	0.084	**	0.015	5.595
Most significant disability	0.776	0.183	**	0.011	16.117	0.041	**	0.015	2.776
Veteran	0.042	0.080	**	0.010	8.016	0.069	**	0.009	7.914
Mobility (PI subgroup)	0.279	0.202	**	0.013	15.249	0.157	**	0.011	14.327
Internal (PI subgroup)	0.553	0.097	**	0.013	7.541	−0.092	**	0.011	−8.507
Dummy for young age	0.184	−0.790	**	0.011	−72.182	−0.761	**	0.014	−55.814
Dummy for prior VR spell	0.231	0.246	**	0.008	30.025	−0.056	**	0.008	−7.275

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.390	0.149	0.142	0.800	0.165	**	0.022	7.490	0.770	**	0.020	38.815
Monthly Govt assistance ($1000s)	0.442	0.516	0.000	4.889	−0.254	**	0.004	−57.998	−0.164	**	0.004	−45.621
Transformed fed Govt Empl propensity	−0.191	0.036	−0.238	−0.003	−0.245	**	0.118	−2.068	0.086	*	0.050	1.715
Transformed out-of-state commuting	−0.186	0.045	−0.240	0.059	−0.231	**	0.102	−2.256				
Local employment rate	0.582	0.150	0.220	0.999	0.052	**	0.017	3.110	−0.318	**	0.013	−23.746

** is statistically significant at the 5% level, and * at the 10% level

Table A.21 Virginia, BVI ($n = 2419$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		0.682	**	0.026	25.932	7.607	**	0.015	495.144
1 Qtr pre-app dummy		0.329	**	0.073	4.497	7.367	**	0.052	142.213
Short run dummy		−0.014		0.028	−0.482	7.303	**	0.018	399.892
Long run dummy		−0.347	**	0.027	−12.721	7.397	**	0.016	449.204
Male	0.516	−0.077	**	0.007	−11.244	0.167	**	0.005	34.702
White	0.588	0.037	**	0.007	5.160	0.083	**	0.005	16.361
High school diploma	0.351	−0.112	**	0.008	−13.643	−0.296	**	0.006	−51.828
Some post-high school	0.212	−0.189	**	0.009	−21.514	−0.136	**	0.006	−21.580
Special education certificate	0.020	−0.314	**	0.041	−7.625	−0.663	**	0.040	−16.713
Married	0.280	0.222	**	0.008	28.630	0.293	**	0.005	53.419
# dependents	0.958	−0.062	**	0.003	−18.215	−0.074	**	0.002	−34.632
Additional disability—Intellectual	0.014	0.286	**	0.047	6.073	−0.710	**	0.015	−47.520
Additional disability—Learning disability	0.005	1.057	**	0.060	17.503	0.609	**	0.099	6.155
Additional disability—Mental illness	0.030	0.012		0.022	0.553	−0.498	**	0.015	−33.871
Additional disability—Physical	0.278	−0.202	**	0.008	−24.251	−0.140	**	0.006	−21.561
Additional disability—Hearing/speech	0.050	−0.240	**	0.020	−11.995	−0.166	**	0.011	−15.068
Additional disability—Substance abuse	0.002	−0.183		0.127	−1.441	0.033		0.171	0.193
Additional disability—ASD	0.006	−0.059		0.130	−0.451	−0.819	**	0.127	−6.460
Additional disability—ADHD	0.007	−0.192	**	0.068	−2.845	−0.033		0.092	−0.356
Significant disability	0.598	−0.021	**	0.011	−1.961	0.082	**	0.008	10.511
Most significant disability	0.217	−0.100	**	0.015	−6.773	0.204	**	0.010	19.703
Dummy for young age	0.178	−0.696	**	0.013	−55.237	−0.770	**	0.016	−48.153
Dummy for prior VR spell	0.038	0.507	**	0.026	19.237	−0.021		0.034	−0.623
Base Application during SFY 2007	0.168	0.033	**	0.014	2.391	−0.354	**	0.010	−35.430
Base Application during SFY 2008	0.153	0.034	**	0.013	2.528	−0.202	**	0.010	−19.446
Base Application during SFY 2009	0.203	0.076	**	0.012	6.153	−0.127	**	0.009	−13.397
Base Application during SFY 2010	0.155	0.093	**	0.012	7.821	−0.150	**	0.009	−16.663
Base Application during SFY 2011	0.179	−0.078	**	0.012	−6.444	−0.157	**	0.009	−16.603

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.399	0.162	0.140	0.920	−0.881	**	0.028	−31.736	0.883	**	0.020	44.127
Monthly Govt assistance ($1000s)	0.496	0.565	0.000	5.970	−0.260	**	0.006	−40.822	−0.173	**	0.004	−41.259
Transformed fed Govt Empl propensity	−0.185	0.048	−0.241	0.000	0.865	**	0.143	6.039	−0.303	**	0.049	−6.144
Transformed out-of-state commuting	−0.178	0.055	−0.241	0.059	−0.678	**	0.130	−5.234				
Local employment rate	0.634	0.177	0.232	1.041	0.024		0.021	1.152	0.041	**	0.013	3.087

** is statistically significant at the 5% level, and * at the 10% level

Table A.22 Maryland, CI ($n = 3010$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−1.433	**	0.033	−43.050	5.782	**	0.025	235.498
1 Qtr pre-app dummy		−1.483	**	0.071	−20.923	5.565	**	0.087	64.142
Short run dummy		−1.063	**	0.034	−30.811	5.789	**	0.026	226.414
Long run dummy		−0.787	**	0.034	−23.445	6.096	**	0.024	255.487
Male	0.565	0.015	**	0.006	2.591	0.029	**	0.005	5.543
White	0.408	0.295	**	0.006	49.135	0.091	**	0.005	16.765
High school diploma	0.316	0.394	**	0.007	54.146	0.338	**	0.007	47.437
Some post-high school	0.091	0.479	**	0.010	49.925	0.411	**	0.009	44.579
Special education certificate	0.280	0.184	**	0.009	21.187	−0.049	**	0.008	−6.072
Cognitive impairment type—Intellectual	0.362	0.052	**	0.007	7.053	−0.178	**	0.007	−24.685
Cognitive impairment type—Learning	0.526	−0.074	**	0.008	−9.894	0.059	**	0.007	7.919
Additional disability—Mental illness	0.357	−0.173	**	0.006	−28.334	−0.226	**	0.006	−38.631
Additional disability—Physical	0.198	−0.137	**	0.007	−20.088	0.015	**	0.007	2.321
Additional disability—Hearing/speech	0.035	−0.026	*	0.014	−1.844	−0.049	**	0.017	−2.984
Additional disability—Substance abuse	0.038	−0.012		0.014	−0.875	0.253	**	0.018	14.252
Additional disability—ASD	0.045	−0.137	**	0.017	−8.123	0.042	**	0.014	3.000
Additional disability—ADHD	0.170	0.147	**	0.009	16.581	0.193	**	0.008	23.136
Significant disability	0.207	0.016		0.014	1.123	0.072	**	0.012	5.793
Most significant disability	0.753	−0.154	**	0.014	−11.240	−0.193	**	0.012	−16.070
Dummy for young age	0.033	−0.924	**	0.056	−16.378	−0.235	**	0.092	−2.552
Dummy for prior VR spell	0.266	0.491	**	0.009	54.740	−0.089	**	0.010	−8.997

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.271	0.116	0.150	0.760	0.868	**	0.028	31.335	2.432	**	0.027	88.526
Monthly Govt assistance ($1000s)	0.272	0.409	0.000	9.000	−0.302	**	0.007	−40.946	−0.354	**	0.007	−51.730
Transformed fed Govt Empl propensity	−0.201	0.031	−0.234	−0.129	2.887	**	0.261	11.079	1.707	**	0.213	8.000
Transformed out-of-state commuting	−0.159	0.081	−0.234	0.008	−0.195	**	0.068	−2.881				
Local employment rate	0.575	0.092	0.355	0.730	1.509	**	0.066	22.799	1.000	**	0.070	14.293

** is statistically significant at the 5% level, and * at the 10% level

Table A.23 Maryland, MI ($n = 4665$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.219	**	0.024	−8.992	6.968	**	0.022	310.310
1 Qtr pre-app dummy		−0.793	**	0.055	−14.484	6.138	**	0.079	77.710
Short run dummy		−0.475	**	0.026	−18.256	6.712	**	0.028	237.951
Long run dummy		−0.454	**	0.025	−18.245	6.929	**	0.025	280.085
Male	0.525	−0.068	**	0.004	−16.606	0.036	**	0.005	7.582
White	0.474	0.075	**	0.004	17.267	0.099	**	0.005	20.442
High school diploma	0.405	0.308	**	0.006	52.345	0.309	**	0.007	43.656
Some post-high school	0.192	0.339	**	0.007	50.066	0.370	**	0.008	46.192
College degree	0.074	0.413	**	0.008	49.430	0.614	**	0.010	62.520
Special education certificate	0.085	0.017	*	0.010	1.706	−0.180	**	0.011	−16.400
Additional disability—Intellectual	0.092	0.220	**	0.007	30.920	−0.070	**	0.008	−8.835
Additional disability—Learning disability	0.122	−0.061	**	0.007	−8.838	−0.067	**	0.008	−8.336
Additional disability—Mental illness	0.926	−0.056	**	0.008	−7.040	−0.013		0.009	−1.475
Additional disability—Physical	0.316	−0.009	**	0.004	−2.013	0.100	**	0.005	20.548
Additional disability—Hearing/speech	0.035	−0.018	*	0.011	−1.718	0.021	*	0.011	1.906
Additional disability—Substance abuse	0.133	−0.073	**	0.006	−12.290	−0.049	**	0.007	−6.842
Additional disability—ASD	0.035	−0.023	*	0.014	−1.658	−0.084	**	0.016	−5.320
Additional disability—ADHD	0.147	0.150	**	0.007	22.830	0.104	**	0.008	13.565
Significant disability	0.171	−0.095	**	0.010	−9.384	−0.095	**	0.012	−7.626
Most significant disability	0.775	0.013		0.009	1.401	−0.101	**	0.012	−8.700
Veteran	0.035	0.248	**	0.011	22.747	0.177	**	0.013	13.422
SMI (MI subgroup)	0.104	−0.247	**	0.007	−35.024	−0.338	**	0.008	−41.988
Dummy for young age	0.183	−0.328	**	0.008	−43.033	−0.424	**	0.013	−33.607
Dummy for prior VR spell	0.284	0.259	**	0.007	39.641	−0.225	**	0.008	−26.796

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.363	0.137	0.153	0.743	−1.194	**	0.019	−61.975	0.731	**	0.024	30.397
Monthly Govt assistance ($1000s)	0.381	0.469	0.000	9.000	−0.231	**	0.004	−53.769	−0.230	**	0.004	−54.880
Transformed fed Govt Empl propensity	−0.204	0.030	−0.234	−0.129	1.694	**	0.204	8.320	3.051	**	0.188	16.250
Transformed out-of-state commuting	−0.170	0.074	−0.234	0.008	−0.560	**	0.053	−10.535				
Local employment rate	0.580	0.091	0.355	0.730	0.837	**	0.052	16.049	0.620	**	0.062	10.050

** is statistically significant at the 5% level, and * at the 10% level

Table A.24 Maryland, PI ($n = 3414$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.601	**	0.027	−22.422	6.832	**	0.022	305.466
1 Qtr pre-app dummy		−0.792	**	0.066	−12.090	6.491	**	0.077	84.606
Short run dummy		−0.755	**	0.030	−25.541	6.542	**	0.030	218.887
Long run dummy		−0.859	**	0.029	−29.515	6.778	**	0.028	243.982
Male	0.515	0.086	**	0.005	18.882	0.178	**	0.005	35.568
White	0.467	0.118	**	0.005	24.995	0.099	**	0.005	19.507
High school diploma	0.414	0.494	**	0.007	72.942	0.381	**	0.008	48.784
Some post-high school	0.227	0.403	**	0.008	53.636	0.447	**	0.009	51.547
College degree	0.103	0.531	**	0.009	60.167	0.807	**	0.009	85.198
Special education certificate	0.066	0.256	**	0.012	21.683	−0.357	**	0.012	−28.928
Additional disability—Intellectual	0.077	0.204	**	0.009	23.867	−0.125	**	0.009	−14.066
Additional disability—Learning disability	0.091	−0.036	**	0.008	−4.245	−0.090	**	0.011	−8.366
Additional disability—Mental illness	0.469	−0.147	**	0.005	−30.367	−0.200	**	0.005	−36.763
Additional disability—Physical	0.886	0.067	**	0.014	4.763	0.037	**	0.015	2.526
Additional disability—Hearing/speech	0.070	0.307	**	0.008	37.537	0.056	**	0.009	6.389
Additional disability—Substance abuse	0.108	−0.228	**	0.008	−30.005	−0.174	**	0.009	−18.421
Additional disability—ADHD	0.076	0.393	**	0.010	41.256	0.356	**	0.010	35.380
Significant disability	0.269	−0.036	**	0.011	−3.291	−0.232	**	0.017	−13.813
Most significant disability	0.673	−0.003		0.011	−0.312	−0.212	**	0.016	−12.892
Veteran	0.052	−0.092	**	0.010	−9.057	0.135	**	0.013	10.429
Mobility (PI subgroup)	0.192	−0.036	**	0.013	−2.715	0.167	**	0.014	12.089
Internal (PI subgroup)	0.660	−0.133	**	0.013	−10.522	−0.021	*	0.013	−1.614
Dummy for young age	0.110	−0.726	**	0.010	−73.618	−0.666	**	0.016	−40.854
Dummy for prior VR spell	0.310	0.147	**	0.007	20.829	−0.198	**	0.009	−21.876

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.416	0.140	0.150	0.858	−0.353	**	0.020	−17.381	0.865	**	0.024	35.855
Monthly Govt assistance ($1000s)	0.479	0.548	0.000	9.000	−0.176	**	0.004	−44.140	−0.149	**	0.004	−35.216
Transformed fed Govt Empl propensity	−0.203	0.030	−0.234	−0.129	−0.553	**	0.231	−2.392	0.933	**	0.210	4.451
Transformed out-of-state commuting	−0.169	0.076	−0.234	0.008	0.052		0.057	0.910				
Local employment rate	0.577	0.091	0.355	0.730	0.497	**	0.060	8.347	0.359	**	0.068	5.279

** is statistically significant at the 5% level, and * at the 10% level

Table A.25 Maryland, BVI ($n = 2254$)—Descriptive Statistics and Estimated Impacts of Exogenous Variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		1.213	**	0.041	29.459	7.427	**	0.023	318.186
1 Qtr pre-app dummy		0.776	**	0.101	7.672	7.175	**	0.090	79.692
Short run dummy		0.791	**	0.042	18.699	7.349	**	0.029	255.342
Long run dummy		0.473	**	0.041	11.665	7.432	**	0.024	305.012
Male	0.508	0.085	**	0.007	12.497	0.069	**	0.006	11.724
White	0.440	0.079	**	0.007	11.434	0.073	**	0.006	12.388
High school diploma	0.357	0.071	**	0.011	6.543	0.242	**	0.009	26.407
Some post-high school	0.236	0.089	**	0.011	7.824	0.365	**	0.010	36.289
College degree	0.173	0.125	**	0.012	10.065	0.820	**	0.010	78.337
Special education certificate	0.039	0.000		0.020	0.016	−0.610	**	0.019	−32.469
Additional disability—Intellectual	0.034	−0.018		0.019	−0.952	−0.393	**	0.017	−23.560
Additional disability—Learning disability	0.048	0.028	*	0.016	1.769	0.251	**	0.013	18.922
Additional disability—Mental illness	0.161	−0.117	**	0.009	−12.682	−0.191	**	0.008	−22.606
Additional disability—Physical	0.455	−0.284	**	0.007	−42.560	−0.200	**	0.006	−35.350
Additional disability—Hearing/speech	0.066	−0.010		0.016	−0.625	−0.082	**	0.011	−7.531
Significant disability	0.101	−0.164	**	0.015	−10.775	−0.344	**	0.012	−28.643
Most significant disability	0.825	−0.019		0.013	−1.453	−0.115	**	0.011	−10.835
Veteran	0.037	−0.466	**	0.018	−26.281	−0.300	**	0.017	−17.669
Blind (BVI subgroup)	0.542	−0.343	**	0.008	−44.897	−0.048	**	0.006	−7.363
Congenital (BVI subgroup)	0.317	0.019	**	0.008	2.534	−0.236	**	0.006	−36.909
Dummy for young age	0.112	−0.683	**	0.017	−39.497	−0.668	**	0.020	−32.615
Dummy for prior VR spell	0.311	0.171	**	0.013	13.348	−0.237	**	0.011	−21.136
Base Application during SFY 2007	0.216	0.123	**	0.012	10.188	0.270	**	0.010	27.476
Base Application during SFY 2008	0.150	0.188	**	0.012	15.177	0.117	**	0.010	11.816
Base Application during SFY 2009	0.153	0.069	**	0.012	5.726	0.021	**	0.010	2.134
Base Application during SFY 2010	0.181	−0.054	**	0.011	−4.818	0.177	**	0.009	18.738
Base Application during SFY 2011	0.151	0.073	**	0.012	6.237	0.206	**	0.010	19.866

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.416	0.140	0.140	0.895	−1.383	**	0.029	−47.073	0.861	**	0.025	34.753
Monthly Govt assistance ($1000s)	0.545	0.611	0.000	8.200	−0.219	**	0.005	−41.640	−0.096	**	0.004	−22.090
Transformed fed Govt Empl propensity	−0.206	0.028	−0.238	−0.129	0.265		0.368	0.720	1.551	**	0.239	6.503
Transformed out-of-state commuting	−0.166	0.077	−0.235	0.008	−1.907	**	0.086	−22.221				
Local employment rate	0.591	0.086	0.355	0.778	−1.072	**	0.088	−12.234	0.248	**	0.079	3.154

** is statistically significant at the 5% level, and * at the 10% level

Table A.26 Kentucky, CI (n = 2372)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.887	**	0.039	−22.770	6.988	**	0.034	203.220
1 Qtr pre-app dummy		−0.864	**	0.119	−7.250	6.991	**	0.135	51.830
Short run dummy		−0.422	**	0.040	−10.666	7.264	**	0.033	220.946
Long run dummy		−0.286	**	0.036	−7.904	7.738	**	0.028	278.225
Male	0.561	0.002		0.008	0.228	0.279	**	0.007	39.007
White	0.792	−0.165	**	0.011	−15.623	−0.146	**	0.009	−15.894
High school diploma	0.342	0.013		0.009	1.371	−0.005		0.008	−0.559
Some post-high school	0.098	0.351	**	0.014	24.925	0.513	**	0.012	41.253
Special education certificate	0.048	0.100	**	0.020	4.946	−0.314	**	0.015	−20.322
Cognitive impairment type—Intellectual	0.516	−0.160	**	0.014	−11.483	−0.243	**	0.012	−19.757
Cognitive impairment type—Learning	0.396	0.128	**	0.013	9.520	0.078	**	0.012	6.545
Additional disability—Mental illness	0.352	−0.082	**	0.009	−8.760	−0.012	*	0.008	−1.590
Additional disability—Physical	0.189	0.080	**	0.011	7.356	−0.083	**	0.009	−9.532
Additional disability—Substance abuse	0.043	−0.500	**	0.022	−22.917	−0.411	**	0.018	−23.124
Additional disability—ADHD	0.164	0.219	**	0.012	17.872	0.186	**	0.010	17.872
Significant disability	0.298	0.272	**	0.026	10.670	−0.039	*	0.023	−1.681
Most significant disability	0.678	−0.068	**	0.026	−2.607	−0.357	**	0.023	−15.534
Dummy for young age	0.071	−0.744	**	0.087	−8.533	−0.350	**	0.115	−3.043
Dummy for prior VR spell	0.224	0.623	**	0.017	36.821	0.324	**	0.018	17.743

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.269	0.118	0.145	0.715	−0.179	**	0.042	−4.248	1.050	**	0.035	29.799
Monthly Govt assistance ($1000s)	1.324	2.013	0.000	13.464	−0.037	**	0.002	−15.881	−0.063	**	0.002	−34.543
Transformed fed Govt Empl propensity	−0.191	0.050	−0.241	−0.047	0.863	**	0.198	4.352	2.863	**	0.126	22.653
Transformed out-of-state commuting	−0.175	0.066	−0.238	0.010	−0.031		0.127	−0.249				
Local employment rate	0.552	0.160	0.230	0.979	0.468	**	0.048	9.807	0.374	**	0.038	9.786

** is statistically significant at the 5% level, and * at the 10% level

Table A.27 Kentucky, MI (n = 4656)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Esitmate	Sig	Std Err	*t*-value
Pre-app dummy		−0.309	**	0.025	−12.421	6.951	**	0.023	307.969
1 Qtr pre-app dummy		−0.470	**	0.061	−7.697	6.549	**	0.066	98.919
Short run dummy		−0.373	**	0.026	−14.176	6.886	**	0.025	272.874
Long run dummy		−0.449	**	0.025	−18.127	7.059	**	0.022	317.295
Male	0.472	−0.042	**	0.005	−8.406	0.175	**	0.005	35.679
White	0.813	−0.133	**	0.007	−19.846	0.020	**	0.006	3.074
High school diploma	0.423	0.208	**	0.007	29.951	0.272	**	0.007	38.813
Some post-high school	0.284	0.320	**	0.008	40.913	0.429	**	0.008	55.622
College degree	0.061	0.403	**	0.011	37.878	0.527	**	0.011	49.222
Additional disability—Intellectual	0.093	−0.261	**	0.010	−27.319	−0.437	**	0.010	−45.841
Additional disability—Learning disability	0.073	0.089	**	0.010	9.013	0.121	**	0.009	12.901
Additional disability—Mental illness	0.926	−0.096	**	0.011	−8.650	0.086	**	0.010	8.317
Additional disability—Physical	0.323	−0.114	**	0.006	−19.224	0.051	**	0.006	8.728
Additional disability—Hearing/speech	0.046	0.048	**	0.012	3.990	0.006		0.011	0.537
Additional disability—Substance abuse	0.184	−0.141	**	0.007	−20.748	−0.175	**	0.007	−26.084
Additional disability—ADHD	0.116	−0.048	**	0.009	−5.569	−0.002		0.008	−0.213
Significant disability	0.398	0.391	**	0.019	20.511	0.203	**	0.018	11.399
Most significant disability	0.579	0.269	**	0.019	13.826	0.058	**	0.018	3.187
Veteran	0.043	−0.236	**	0.013	−18.135	−0.219	**	0.013	−17.391
SMI (MI subgroup)	0.067	−0.271	**	0.011	−24.890	−0.494	**	0.011	−46.317
Dummy for young age	0.150	−0.315	**	0.010	−30.100	−0.419	**	0.014	−30.674
Dummy for prior VR spell	0.236	0.181	**	0.010	17.396	−0.095	**	0.011	−8.295

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.362	0.127	0.147	0.765	−0.979	**	0.024	−41.337	0.686	**	0.024	28.140
Monthly Govt assistance ($1000s)	1.508	2.410	0.000	15.312	−0.030	**	0.001	−28.790	−0.028	**	0.001	−29.253
Transformed fed Govt Empl propensity	−0.198	0.047	−0.241	−0.047	−1.473	**	0.113	−13.040	1.890	**	0.086	21.963
Transformed out-of-state commuting	−0.182	0.065	−0.238	0.010	0.922	**	0.080	11.593				
Local employment rate	0.579	0.155	0.230	0.979	0.165	**	0.025	6.703	0.110	**	0.024	4.516

** is statistically significant at the 5% level, and * at the 10% level

Table A.28 Kentucky, PI ($n = 3526$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		0.403	**	0.026	15.576	7.530	**	0.020	377.329
1 Qtr pre-app dummy		0.393	**	0.075	5.221	7.277	**	0.069	105.062
Short run dummy		0.344	**	0.029	11.971	7.459	**	0.024	305.735
Long run dummy		0.223	**	0.027	8.248	7.587	**	0.022	352.900
Male	0.485	−0.108	**	0.006	−18.163	0.126	**	0.005	25.565
White	0.816	0.024	**	0.008	2.882	0.194	**	0.007	28.282
High school diploma	0.391	0.066	**	0.008	7.797	0.211	**	0.007	28.532
Some post-high school	0.316	0.205	**	0.009	21.776	0.394	**	0.008	48.191
College degree	0.088	0.159	**	0.012	13.777	0.421	**	0.009	44.498
Additional disability—Intellectual	0.071	−0.006		0.012	−0.495	−0.617	**	0.011	−55.999
Additional disability—Learning disability	0.045	0.203	**	0.014	14.346	0.122	**	0.012	9.876
Additional disability—Mental illness	0.424	−0.173	**	0.006	−27.076	−0.188	**	0.005	−35.949
Additional disability—Physical	0.921	−0.142	**	0.020	−7.128	−0.033	*	0.020	−1.630
Additional disability—Hearing/speech	0.106	0.450	**	0.010	44.860	0.326	**	0.009	37.964
Additional disability—Substance abuse	0.076	0.000		0.011	−0.002	−0.115	**	0.010	−11.948
Additional disability—ADHD	0.041	0.031	**	0.016	2.029	−0.075	**	0.017	−4.429
Significant disability	0.492	−0.119	**	0.022	−5.318	−0.074	**	0.018	−4.033
Most significant disability	0.483	−0.319	**	0.023	−13.892	−0.143	**	0.019	−7.600
Veteran	0.062	−0.053	**	0.013	−4.172	0.062	**	0.011	5.522
Mobility (PI subgroup)	0.429	0.173	**	0.017	9.959	0.179	**	0.018	10.044
Internal (PI subgroup)	0.467	0.263	**	0.017	15.100	0.133	**	0.018	7.420
Dummy for young age	0.139	−0.522	**	0.015	−34.283	−0.796	**	0.017	−47.729
Dummy for prior VR spell	0.242	0.033	**	0.012	2.789	−0.230	**	0.011	−20.126

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.411	0.145	0.155	0.858	−1.018	**	0.027	−38.257	0.237	**	0.021	11.048
Monthly Govt assistance ($1000s)	2.051	2.934	0.000	23.163	−0.042	**	0.001	−43.022	−0.039	**	0.001	−48.940
Transformed fed Govt Empl propensity	−0.195	0.048	−0.241	−0.047	−0.130		0.141	−0.921	1.483	**	0.080	18.457
Transformed out-of-state commuting	−0.182	0.063	−0.238	0.009	−0.490	**	0.102	−4.808				
Local employment rate	0.567	0.157	0.230	0.979	−0.043		0.028	−1.494	0.000		0.023	−0.019

** is statistically significant at the 5% level, and * at the 10% level

Table A.29 North Carolina, CI ($n = 6131$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	t-value	Estimate	Sig	Std Err	t-value
Pre-app dummy		−1.480	**	0.024	−62.913	6.224	**	0.021	294.737
1 Qtr pre-app dummy		−1.484	**	0.069	−21.408	6.082	**	0.074	82.021
Short run dummy		−0.898	**	0.022	−40.272	6.392	**	0.018	354.975
Long run dummy		−0.485	**	0.021	−23.243	6.871	**	0.014	476.053
Male	0.605	0.104	**	0.006	18.661	0.241	**	0.004	55.455
White	0.424	0.127	**	0.006	22.400	0.043	**	0.005	9.431
High school diploma	0.296	0.168	**	0.006	26.265	0.126	**	0.005	24.829
Some post-high school	0.061	0.352	**	0.011	31.644	0.342	**	0.008	41.942
Special education certificate	0.168	0.132	**	0.009	15.031	−0.031	**	0.007	−4.542
Married	0.056	−0.009		0.011	−0.863	0.089	**	0.009	10.342
Cognitive impairment type—Intellectual	0.738	−0.108	**	0.009	−12.193	−0.056	**	0.007	−8.158
Cognitive impairment type—Learning	0.249	0.186	**	0.009	20.694	0.205	**	0.007	29.004
Additional disability—Mental illness	0.257	−0.141	**	0.007	−20.038	−0.102	**	0.006	−18.048
Additional disability—Physical	0.076	−0.222	**	0.011	−20.906	−0.067	**	0.009	−7.759
Additional disability—Substance abuse	0.030	−0.285	**	0.017	−16.993	−0.055	**	0.012	−4.679
Additional disability—ASD	0.030	−0.198	**	0.020	−10.101	−0.330	**	0.013	−25.267
Additional disability—ADHD	0.076	0.017	*	0.011	1.569	−0.103	**	0.009	−11.340
Significant disability	0.458	−0.246	**	0.008	−29.999	−0.111	**	0.007	−15.994
Most significant disability	0.417	−0.410	**	0.009	−44.155	−0.349	**	0.008	−45.278
Dummy for young age	0.105	−0.552	**	0.061	−9.005	−0.056		0.074	−0.756
Dummy for prior VR spell	0.298	0.868	**	0.012	72.028	0.242	**	0.012	19.433

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.266	0.118	0.140	0.720	0.336	**	0.029	11.741	1.207	**	0.023	53.556
Monthly Govt assistance ($1000s)	0.229	0.360	0.000	2.792	−0.453	**	0.008	−54.371	−0.624	**	0.006	−98.872
Transformed fed Govt Empl propensity	−0.191	0.042	−0.240	−0.061	−0.608	**	0.243	−2.501	1.220	**	0.102	12.017
Transformed out-of-state commuting	−0.188	0.048	−0.240	0.216	0.853	**	0.186	4.578				
Local employment rate	0.532	0.140	0.222	0.815	0.457	**	0.039	11.833	0.093	**	0.029	3.276

** is statistically significant at the 5% level, and * at the 10% level

Table A.30 North Carolina, MI ($n = 8085$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.569	**	0.020	−27.939	6.747	**	0.018	377.437
1 Qtr pre-app dummy		−0.769	**	0.039	−19.722	6.298	**	0.042	149.222
Short run dummy		−0.484	**	0.021	−23.172	6.574	**	0.018	363.769
Long run dummy		−0.503	**	0.020	−25.059	6.807	**	0.017	407.187
Male	0.494	−0.023	**	0.005	−4.879	0.163	**	0.004	38.396
White	0.506	0.040	**	0.005	8.171	0.099	**	0.004	22.800
High school diploma	0.355	0.272	**	0.006	44.277	0.236	**	0.006	41.938
Some post-high school	0.254	0.228	**	0.007	33.956	0.246	**	0.006	39.844
College degree	0.062	0.590	**	0.010	59.640	0.693	**	0.008	81.768
Special education certificate	0.033	0.075	**	0.016	4.762	−0.102	**	0.012	−8.339
Married	0.106	0.115	**	0.007	15.506	0.155	**	0.006	24.279
Additional disability—Intellectual	0.150	0.054	**	0.008	6.789	−0.009		0.007	−1.281
Additional disability—Learning disability	0.034	0.348	**	0.014	25.614	0.242	**	0.012	19.713
Additional disability—Mental illness	0.936	−0.075	**	0.010	−7.730	−0.018	**	0.008	−2.103
Additional disability—Physical	0.128	−0.151	**	0.008	−20.079	−0.005		0.007	−0.777
Additional disability—Substance abuse	0.158	−0.108	**	0.007	−15.810	−0.080	**	0.006	−13.437
Additional disability—ASD	0.019	0.381	**	0.018	21.385	0.044	**	0.013	3.399
Additional disability—ADHD	0.047	0.337	**	0.011	31.572	0.286	**	0.009	30.186
Significant disability	0.612	−0.087	**	0.007	−13.053	−0.041	**	0.006	−6.964
Most significant disability	0.259	−0.254	**	0.008	−30.376	−0.246	**	0.007	−33.164
Veteran	0.043	−0.288	**	0.011	−26.137	−0.112	**	0.010	−10.679
SMI (MI subgroup)	0.142	−0.111	**	0.007	−15.001	−0.197	**	0.007	−27.996
Dummy for young age	0.137	−0.350	**	0.010	−34.710	−0.425	**	0.013	−32.094
Dummy for prior VR spell	0.310	0.241	**	0.010	25.090	−0.127	**	0.012	−10.967

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.369	0.127	0.150	0.767	−0.480	**	0.022	−21.368	0.626	**	0.020	31.587
Monthly Govt assistance ($1000s)	0.296	0.436	0.000	4.800	−0.445	**	0.006	−76.783	−0.442	**	0.004	−108.814
Transformed fed Govt Empl propensity	−0.198	0.038	−0.240	−0.061	0.616	**	0.253	2.429	1.050	**	0.095	11.041
Transformed out-of-state commuting	−0.196	0.045	−0.240	0.215	0.110		0.208	0.529				
Local employment rate	0.559	0.140	0.221	0.815	0.508	**	0.030	17.205	0.083	**	0.026	3.251

** is statistically significant at the 5% level, and * at the 10% level

Table A.31 North Carolina, PI ($n = 6329$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.084	**	0.024	−3.434	7.260	**	0.017	423.584
1 Qtr pre-app dummy		−0.190	**	0.054	−3.495	6.876	**	0.049	140.018
Short run dummy		−0.121	**	0.026	−4.596	6.968	**	0.020	344.757
Long run dummy		−0.138	**	0.026	−5.417	7.151	**	0.018	397.659
Male	0.504	−0.123	**	0.005	−23.342	0.197	**	0.004	45.495
White	0.526	−0.127	**	0.005	−24.183	0.046	**	0.004	10.732
High school diploma	0.377	0.294	**	0.007	41.672	0.252	**	0.006	43.272
Some post-high school	0.326	0.249	**	0.007	33.310	0.263	**	0.006	44.398
College degree	0.076	0.493	**	0.011	46.253	0.572	**	0.009	64.922
Special education certificate	0.020	0.212	**	0.021	10.065	−0.128	**	0.018	−6.980
Married	0.245	0.104	**	0.006	17.716	0.190	**	0.005	40.619
Additional disability—Intellectual	0.064	0.088	**	0.013	6.823	−0.061	**	0.011	−5.609
Additional disability—Learning disability	0.013	0.490	**	0.030	16.298	0.415	**	0.020	20.379
Additional disability—Mental illness	0.178	0.093	**	0.008	12.392	0.117	**	0.006	18.123
Additional disability—Physical	0.931	0.054		0.041	1.295	−0.045	*	0.027	−1.645
Additional disability—Hearing/speech	0.013	0.408	**	0.024	16.704	0.111	**	0.018	5.994
Additional disability—Substance abuse	0.028	−0.153	**	0.019	−8.245	−0.009		0.015	−0.589
Significant disability	0.692	−0.157	**	0.006	−24.563	−0.132	**	0.005	−24.326
Most significant disability	0.129	−0.318	**	0.010	−30.326	−0.444	**	0.009	−48.043
Veteran	0.058	0.094	**	0.012	7.871	0.024	**	0.010	2.423
Mobility (PI subgroup)	0.563	0.051		0.040	1.283	0.199	**	0.026	7.691
Internal (PI subgroup)	0.360	0.034		0.040	0.842	0.113	**	0.026	4.356
Dummy for young age	0.064	−0.562	**	0.016	−34.900	−0.631	**	0.018	−34.093
Dummy for prior VR spell	0.274	−0.013		0.011	−1.178	−0.275	**	0.011	−24.934

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.439	0.127	0.002	0.808	−0.633	**	0.023	−27.838	0.149	**	0.019	8.046
Monthly Govt assistance ($1000s)	0.422	0.549	0.000	5.800	−0.319	**	0.005	−63.257	−0.281	**	0.004	−72.499
Transformed fed Govt Empl propensity	−0.191	0.040	−0.240	−0.061	−0.138		0.258	−0.534	0.799	**	0.099	8.085
Transformed out-of-state commuting	−0.187	0.049	−0.240	0.216	0.303		0.199	1.524				
Local employment rate	0.536	0.142	0.221	0.815	0.141	**	0.034	4.164	−0.074	**	0.027	−2.735

** is statistically significant at the 5% level, and * at the 10% level

Table A.32 North Carolina, BVI (n = 7089)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		0.866	**	0.028	31.037	7.868	**	0.018	444.616
1 Qtr pre-app dummy		0.718	**	0.081	8.902	7.603	**	0.055	138.107
Short run dummy		0.620	**	0.025	24.787	7.648	**	0.016	488.349
Long run dummy		0.306	**	0.024	12.741	7.664	**	0.015	525.956
Male	0.516	−0.028	**	0.006	−4.783	0.326	**	0.004	77.064
White	0.593	−0.050	**	0.006	−8.778	0.026	**	0.004	6.323
High school diploma	0.336	0.158	**	0.008	19.726	0.141	**	0.006	24.804
Some post-high school	0.308	0.167	**	0.008	21.278	0.161	**	0.006	28.928
College degree	0.096	0.341	**	0.010	33.122	0.555	**	0.007	74.739
Married	0.328	0.104	**	0.006	16.707	0.155	**	0.004	34.659
Additional disability—Mental illness	0.014	0.408	**	0.023	17.902	0.230	**	0.019	11.872
Additional disability—Physical	0.456	0.154	**	0.006	26.215	0.069	**	0.004	16.187
Additional disability—Hearing/speech	0.018	0.542	**	0.022	24.989	0.420	**	0.017	25.174
Significant disability	0.120	−0.198	**	0.009	−22.401	−0.109	**	0.007	−16.598
Most significant disability	0.015	−0.462	**	0.029	−15.651	−0.174	**	0.016	−11.178
Blind (BVI subgroup)	0.185	0.043	**	0.009	4.919	0.164	**	0.006	25.573
Congenital (BVI subgroup)	0.076	0.234	**	0.010	22.357	0.159	**	0.008	20.279
Dummy for young age	0.062	−0.631	**	0.021	−30.627	−0.845	**	0.019	−45.073
Dummy for prior VR spell	0.092	0.643	**	0.034	18.740	0.303	**	0.026	11.596
Base Application during SFY 2007	0.181	−0.943	**	0.011	−88.228	−0.679	**	0.008	−81.251
Base Application during SFY 2008	0.161	−0.887	**	0.010	−89.479	−0.630	**	0.008	−76.044
Base Application during SFY 2009	0.182	−0.979	**	0.010	−101.506	−0.750	**	0.007	−102.651
Base Application during SFY 2010	0.168	−0.735	**	0.009	−82.640	−0.491	**	0.007	−73.245
Base Application during SFY 2011	0.166	−0.423	**	0.008	−51.179	−0.396	**	0.006	−68.434

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.462	0.135	0.140	0.850	−2.436	**	0.026	−92.511	−0.935	**	0.019	−50.357
Monthly Govt assistance ($1000s)	0.274	1.000	0.000	73.074	−0.222	**	0.006	−37.817	−0.168	**	0.005	−35.410
Transformed fed Govt Empl propensity	−0.194	0.040	−0.240	−0.060	−0.052		0.270	−0.193	0.762	**	0.107	7.128
Transformed out-of-state commuting	−0.191	0.048	−0.240	0.216	−0.757	**	0.187	−4.037				
Local employment rate	0.541	0.137	0.220	0.840	−0.201	**	0.043	−4.711	0.046		0.031	1.491

** is statistically significant at the 5% level, and * at the 10% level

Table A.33 Texas, CI ($n = 3580$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		−0.937	**	0.038	−24.754	6.172	**	0.033	188.036
1 Qtr pre-app dummy		−1.036	**	0.083	−12.427	6.030	**	0.085	70.995
Short run dummy		−0.514	**	0.038	−13.411	6.385	**	0.032	202.276
Long run dummy		−0.269	**	0.037	−7.204	6.676	**	0.029	234.029
Male	0.573	0.060	**	0.007	8.723	0.258	**	0.006	43.057
White	0.685	0.016	**	0.007	2.135	0.062	**	0.007	9.355
High school diploma	0.385	0.366	**	0.011	33.826	0.190	**	0.009	20.540
Some post-high school	0.089	0.428	**	0.014	31.098	0.383	**	0.012	31.031
Special education certificate	0.361	0.215	**	0.011	19.290	0.024	**	0.010	2.551
Married	0.045	0.135	**	0.015	9.237	0.250	**	0.013	18.752
# dependents	2.125	−0.022	**	0.002	−9.098	0.016	**	0.002	8.404
Cognitive impairment type—Intellectual	0.271	−0.068	**	0.017	−4.095	−0.075	**	0.015	−4.931
Cognitive impairment type—Learning	0.707	−0.015		0.016	−0.962	0.206	**	0.014	14.330
Additional disability—Mental illness	0.213	−0.254	**	0.009	−28.606	−0.196	**	0.008	−25.766
Additional disability—Physical	0.156	−0.228	**	0.009	−24.117	−0.145	**	0.008	−18.050
Additional disability—Hearing/speech	0.034	−0.196	**	0.020	−9.597	−0.129	**	0.015	−8.526
Additional disability—ASD	0.037	−0.252	**	0.021	−11.773	−0.415	**	0.016	−26.585
Additional disability—ADHD	0.109	0.138	**	0.011	12.096	0.009		0.010	0.888
Significant disability	0.730	−0.132	**	0.015	−9.025	−0.120	**	0.012	−9.954
Most significant disability	0.208	−0.187	**	0.016	−11.345	−0.181	**	0.014	−13.078

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.248	0.105	0.153	0.822	1.106	**	0.037	30.243	1.264	**	0.032	40.061
Monthly Govt assistance ($1000s)	0.209	0.349	0.000	2.916	−0.377	**	0.011	−35.872	−0.396	**	0.009	−45.319
Transformed fed Govt Empl propensity	−0.205	0.032	−0.241	−0.054	0.181		0.739	0.245	1.714	**	0.130	13.218
Transformed out-of-state commuting	−0.206	0.034	−0.243	−0.048	0.528		0.699	0.756				
Local employment rate	0.576	0.126	0.200	0.835	0.664	**	0.037	18.001	0.467	**	0.032	14.438

** is statistically significant at the 5% level, and * at the 10% level

Table A.34 Texas, MI (n = 4590)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		0.251	**	0.036	6.955	6.865	**	0.031	218.086
1 Qtr pre-app dummy		−0.084		0.079	−1.062	6.626	**	0.109	60.857
Short run dummy		0.383	**	0.038	10.166	6.761	**	0.034	198.734
Long run dummy		0.310	**	0.036	8.637	6.988	**	0.030	231.517
Male	0.495	−0.146	**	0.006	−23.276	0.067	**	0.006	11.452
White	0.673	0.024	**	0.007	3.700	0.175	**	0.006	29.806
High school diploma	0.351	0.298	**	0.010	30.242	0.251	**	0.009	27.691
Some post-high school	0.340	0.255	**	0.010	25.451	0.341	**	0.009	37.215
College degree	0.080	0.257	**	0.013	19.873	0.363	**	0.011	32.411
Special education certificate	0.086	−0.071	**	0.015	−4.626	0.007		0.015	0.497
Married	0.131	0.104	**	0.009	11.648	0.300	**	0.008	36.378
# dependents	1.704	−0.001		0.003	−0.323	0.019	**	0.003	7.243
Additional disability—Intellectual	0.047	0.146	**	0.015	9.664	−0.138	**	0.017	−8.035
Additional disability—Learning disability	0.125	0.256	**	0.010	25.853	0.139	**	0.010	14.539
Additional disability—Mental illness	0.950	−0.296	**	0.014	−21.820	−0.125	**	0.011	−11.708
Additional disability—Physical	0.313	0.017	**	0.007	2.471	0.088	**	0.006	13.894
Additional disability—Hearing/speech	0.047	0.063	**	0.014	4.464	0.027	**	0.011	2.339
Additional disability—Substance abuse	0.133	−0.036	**	0.010	−3.774	0.042	**	0.009	4.938
Additional disability—ASD	0.032	−0.154	**	0.019	−8.090	−0.293	**	0.017	−17.662
Additional disability—ADHD	0.091	0.098	**	0.011	8.728	−0.068	**	0.010	−6.626
Significant disability	0.773	−0.139	**	0.012	−11.988	−0.124	**	0.010	−12.226
Most significant disability	0.154	−0.301	**	0.014	−21.608	−0.245	**	0.013	−19.313
Veteran	0.042	−0.060	**	0.016	−3.772	0.041	**	0.014	2.915
SMI (MI subgroup)	0.091	−0.243	**	0.013	−19.363	−0.412	**	0.012	−35.421

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.371	0.132	0.157	0.723	−0.740	**	0.029	−25.628	0.412	**	0.028	14.863
Monthly Govt assistance ($1000s)	0.309	0.463	0.000	4.791	−0.376	**	0.007	−56.030	−0.288	**	0.006	−45.703
Transformed fed Govt Empl propensity	−0.211	0.028	−0.241	−0.054	0.368		0.652	0.564	1.072	**	0.124	8.666
Transformed out-of-state commuting	−0.214	0.031	−0.242	−0.048	0.559		0.612	0.914				
Local employment rate	0.599	0.119	0.199	0.937	0.349	**	0.032	10.954	0.191	**	0.028	6.804

** is statistically significant at the 5% level, and * at the 10% level

Table A.35 Texas, PI ($n = 7746$)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Estimate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		0.280	**	0.025	11.158	7.059	**	0.016	443.776
1 Qtr pre-app dummy		−0.040		0.063	−0.636	6.940	**	0.051	136.690
Short run dummy		0.284	**	0.028	10.056	7.024	**	0.019	373.973
Long run dummy		0.117	**	0.027	4.298	7.155	**	0.017	422.186
Male	0.540	−0.130	**	0.004	−29.990	0.233	**	0.003	69.464
White	0.734	−0.054	**	0.005	−11.041	0.072	**	0.004	19.028
High school diploma	0.332	0.091	**	0.006	14.119	0.137	**	0.005	27.389
Some post-high school	0.367	0.122	**	0.006	19.243	0.320	**	0.005	64.320
College degree	0.099	0.233	**	0.008	28.439	0.664	**	0.006	114.666
Special education certificate	0.042	0.385	**	0.014	28.457	0.139	**	0.013	10.991
Married	0.289	−0.054	**	0.005	−9.878	0.082	**	0.004	19.806
# dependents	1.918	0.093	**	0.002	51.960	0.078	**	0.001	54.207
Additional disability—Intellectual	0.026	−0.636	**	0.017	−38.106	−0.686	**	0.015	−46.665
Additional disability—Learning disability	0.048	−0.103	**	0.010	−10.156	−0.220	**	0.009	−24.913
Additional disability—Mental illness	0.183	−0.204	**	0.006	−35.762	−0.216	**	0.004	−48.169
Additional disability—Physical	0.974	−0.129	**	0.019	−6.728	−0.323	**	0.012	−26.660
Additional disability—Hearing/speech	0.079	−0.027	**	0.008	−3.353	−0.066	**	0.006	−11.338
Additional disability—Substance abuse	0.038	−0.055	**	0.012	−4.587	−0.078	**	0.010	−8.149
Additional disability—ADHD	0.021	−0.275	**	0.017	−16.043	−0.391	**	0.013	−30.426
Significant disability	0.767	−0.257	**	0.006	−39.558	−0.133	**	0.005	−26.138
Most significant disability	0.106	−0.554	**	0.010	−55.144	−0.333	**	0.009	−38.592
Veteran	0.056	−0.286	**	0.009	−30.582	−0.064	**	0.007	−9.370
Mobility (PI subgroup)	0.473	0.075	**	0.014	5.550	0.344	**	0.009	39.244
Internal (PI subgroup)	0.472	0.244	**	0.013	18.109	0.357	**	0.009	41.131

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.434	0.135	0.153	0.885	−0.243	**	0.019	−13.005	0.437	**	0.014	30.345
Monthly Govt assistance ($1000s)	0.333	0.541	0.000	4.791	−0.303	**	0.004	−78.902	−0.160	**	0.003	−53.044
Transformed fed Govt Empl propensity	−0.205	0.032	−0.242	0.000	0.594		0.428	1.389	0.889	**	0.066	13.564
Transformed out-of-state commuting	−0.206	0.034	−0.243	0.000	0.541		0.405	1.335				
Local employment rate	0.581	0.129	0.199	1.273	0.274	**	0.021	12.752	0.180	**	0.017	10.792

** is statistically significant at the 5% level, and * at the 10% level

Table A.36 Texas, BVI (n = 1446)—descriptive statistics and estimated impacts of exogenous variables

	Descriptive statistics	Employment				log earnings (if employed)			
Dummy variables	Proportion	Esitmate	Sig	Std Err	*t*-value	Estimate	Sig	Std Err	*t*-value
Pre-app dummy		1.115	**	0.082	13.575	7.118	**	0.047	151.110
1 Qtr pre-app dummy		1.351	**	0.324	4.165	7.042	**	0.252	27.934
Short run dummy		0.841	**	0.083	10.182	7.056	**	0.062	113.945
Long run dummy		0.247	**	0.081	3.044	6.707	**	0.056	120.515
Male	0.521	−0.017		0.012	−1.426	0.229	**	0.008	28.392
White	0.788	0.005		0.015	0.293	−0.016	*	0.010	−1.629
High school diploma	0.077	0.342	**	0.028	12.172	0.526	**	0.020	26.033
Some post-high school	0.093	−0.200	**	0.028	−7.150	0.268	**	0.020	13.246
Education missing	0.768	−0.174	**	0.023	−7.446	0.199	**	0.018	10.861
Married	0.370	0.257	**	0.013	19.298	0.233	**	0.009	25.154
# dependents	1.969	0.077	**	0.005	14.617	0.140	**	0.004	37.758
Additional disability—Physical	0.529	−0.193	**	0.013	−15.220	−0.042	**	0.009	−4.774
Additional disability—Hearing/speech	0.081	0.305	**	0.020	15.137	0.112	**	0.018	6.268
Significant disability	0.754	−0.077	**	0.022	−3.436	−0.190	**	0.019	−9.903
Most significant disability	0.189	−0.478	**	0.027	−17.899	−0.374	**	0.023	−16.342
Blind (BVI subgroup)	0.594	0.180	**	0.014	12.433	0.194	**	0.010	19.323

Continuous variables	Mean	Std dev	Min	Max	Coef	Sig	Std err	*t*-value	Coef	Sig	Std err	*t*-value
Age/100	0.463	0.146	0.138	0.930	−2.112	**	0.053	−40.043	−0.336	**	0.036	−9.347
Monthly Govt assistance ($1000s)	0.370	0.543	0.000	3.618	−0.534	**	0.012	−46.166	−0.388	**	0.009	−44.235
Transformed fed Govt Empl propensity	−0.203	0.031	−0.242	0.000	0.997		1.166	0.855	0.362	**	0.146	2.479
Transformed out-of-state commuting	−0.204	0.034	−0.242	0.000	−1.661		1.101	−1.509				
Local employment rate	0.566	0.122	0.274	1.289	1.073	**	0.062	17.205	1.015	**	0.040	25.478

** is statistically significant at the 5% level, and * at the 10% level

Appendix of Chap. 6

Appendix 4: Estimating the Effects of Vocational Rehabilitation Programs Using Difference-in-Differences Models

This Appendix provides the necessary information necessary to estimate the difference-in-differences (DID) model used in Chap. 6. It also includes STATA, SAS, and Excel software code which, when combined with the necessary data, can be used to estimate these models.

We first formalize the basic DID model estimated in Sect. 6.2.1 and provide the STATA software code for estimating this model. Then we describe some of the extensions estimated in Sect. 6.3. See Meyer (1995), Manski and Pepper (2013, 2018), Hansen (2022, Chap. 18), Roth et al. (2023) for a more detailed discussion of DID models.

Basic DID Model Specification

Consider the simplest DID models analyzed in Sect. 6.2.1 where there is only one treatment—VR training services—and two time periods, $t = 0$ and $t = 1$. In period $t = 0$, VR clients have not yet applied and thus not received services (the pre-application period). In period $t = 1$, some clients have received training services and others have not (the post-application period).

Given this setup, the general form of our DID regression model is specified as

$$E(Y_{it}|\text{Post}, \text{Treatment}) = b_0 + b_1\text{Post}_t + b_2\text{Treat}_i + qZ_{it},$$

where Y_{it} represents the outcome variable (e.g., employment status or earnings) for individual i at time t. Post_t is a dummy variable for the post-treatment period (equals 1 if $t = 1$ and 0 if $t = 0$), Treat_i is a dummy variable for the treatment group (equals 1 if the client received VR services and 0 otherwise), and $Z_{it} = \text{Post}_t \times \text{Treat}_i$ is the interaction term that equals 1 for treated clients in post-treatment period, and 0 otherwise. Finally, (b_0,b_1,b_2,q) are unknown parameters measuring how the expected labor market outcomes vary over the two periods, b_1, across the treatment and control groups, b_2, and by VR service receipt, q. Our interest is in estimating q, the effect of VR services on expected labor market outcomes.

This is a standard linear mean regression model that can be estimated using any basic statistical software package including STATA, SAS, SPSS, or Excel. The STATA command, for example, is

$$reg\ Y\ Post\ Treat\ Z,$$

where $Z = \text{Post} \times \text{Treat}$. In SAS, the code is

$$proc\ reg\ data = name\ of\ data\ set; model\ Y = post\ Treat\ Z;.$$

where *name of data set* is replaced by the name and location of the data. In Excel, the code is

$$LINEST(Y\_range, X\_range, True, True),$$

where *Y_range* is replaced with the range of the outcome variable Y (e.g., A2: A:100), and *X_range* with the range of the covariates including the interaction term Post, Treat, Z (e.g., B2:D100 for Post, Treat and Z). The first *true* option indicates the intercept b_0 is included in the linear regression and the second indicates that additional diagnostic statistics should be reported (e.g., R^2).

Extensions

The models estimated in Chap.6 extend this basic setup. Here is a summary of the key variations:

(i) Disability: Estimates vary by the client's disability, namely CI, MI, and PI. We estimate the DID models for each cohort separately.
(ii) VR Services: Estimates vary by the seven different service types. In some cases, this is done by estimating the models separately for each service. In others, we estimate the effects jointly (see Sect. 6.3.2.2). The latter approach is straightforward but somewhat more complicated.
(iii) Covariates: Estimates control for the rich set of covariates used in the VR-ROI model. Let X denote this set of covariates. To include these in the model, add Xg to the set of regressors, where g is an unknown parameter associated with the effect of X on the average value of Y.
(iv) Short versus Long Run: estimates vary by the short and long run. To do this, we add new period variables to distinguish between the pre-period, the short-run post-period (0–8 quarters) and the long-run post-period (>8 quarters). We then interact these two post-periods with the treatment indicator, Treat.

The STATA command used to estimate displayed for CI in Table 6.2 is

$$reg\ Y\ Treatment\ POST\_short\ POST\_long\ z\_short\ z\_long\ if\ CI\_smpl == 1$$

where *Post_Short* indicates between 0 and 8 quarters in the post-treatment period, *Post_Long* indicated nine or more quarters after treatment, and *z_short* and *z_long* are the interactions of the short and long post-period indicators with Treat.

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